WHEN IS A DEVELOPMENTAL ISSUE NOT A DEVELOPMENTAL ISSUE?

THE CASE OF THE CONSERVATION TASK CONTROVERSY

by

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I declare that I alone carried out the work involved in the design of and research for this thesis.
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ABSTRACT

There are grounds for suspecting that the vast controversy over the interpretation of conservation task (C.T.) results reflects an underlying descriptive confusion. I therefore set out to interrogate assumptions about the task that are shared by all parties to the debate. Piaget's classical version and a modified "incidental" version of the volume/amount C.T. are selected as representative examples, and evidence from both nursery-age children and adults is drawn on. The argument takes the form of a series of challenges to most previous research in that it involves various shifts of emphasis. Thus, in place of the preoccupation with explaining instances of "interesting" failure by the child, I focus on what is involved in any instance of success. And, instead of dwelling on the possible significance of the array and its (actually irrelevant) transformation, I focus on the potential role of the questions asked.

From such challenges I conclude:— (a) that both right and wrong answers on both sorts of task are compatible with both the presence and absence of reasoning ability; (b) that, given the background of deception and lack of information, "failure" and "success" are always inappropriate terms for describing performance; (c) that the C.T. results are most parsimoniously explained in terms solely of the ambiguity in the form of questioning; and (d) that since it becomes implausible to argue for any necessary age-dependency in relation to performance, the C.T. cannot be used to help resolve any developmental issue. Drawing on the insights of Merleau-Ponty, I identify the shared source of error in a confusion of perspectives common in cognitive approaches to the interpretation of conduct. The implications for child psychology are discussed.
ABBREVIATIONS

C.T. : conservation task.

Task "P" : Piaget's classical volume/amount conservation task.

Task "O" : modified "incidental" version of "P" (after Light et al (1979) ).

Container A : the comparison beaker (contents unchanged throughout).

Container B : beaker holding the pre-transformation contents (to be poured into container C).

Container C : beaker holding the post-transformation contents (originally in container B).

Q1 : the initial "establishing equivalence" question: "Is there more in A, less in A, or the same in each [beaker]?".

Q2 : the final, "conservation" question - identically worded to Q1.

Q3 : the additional question asked in incidental/accidental versions of the task: "where were we?" / "what were we saying".

Question A : Why does the representative young child fail on "P", but succeed on "O"?

Question B : Why does the representative young child fail on "P", while the adult succeeds on the same task?

Question C : What is the most parsimonious general account able to be offered of success / failure by anyone on any bona fide C.T.? (see chapter 4, section A).

S : subject.

E : experimenter.

C.I. Task : class-inclusion task (see chapter 9).
First Conservation Story

I arrive at the pre-allotted time for my mystery appointment and there to greet me is a man wearing a top-hat. He picks an orange from the fruit bowl and tells me he is going to show me something interesting. Taking off his hat, he drops the orange into it with a flourish. Then, laying a red silk handkerchief over the top of it, he says portentously, "Abracadabra. Oranges will become bananas" three times over, all the while slowly turning the hat in a clockwise direction ...

At this point the account of the episode is interrupted in order to reflect on what sense I am making of it.

In the absence of any questions being put to me, what sort of questions might I put to myself? Of course, I may not see a problem, in which case there is no need for questions. Thus I may dismiss the episode as "just a game for kids", or the product of a deranged mind. But, choosing to attend seriously to the events and circumstances, the following three alternatives might be envisaged:

(1) I might assume that the "something interesting" has happened and that there is, somehow, now a banana in the hat, and all that is left for him to show me is the transformed piece of fruit. One question that occurs to me then is "I wonder how he did it?"

(2) I might assume, trusting the evidence of my eyes, that the orange that was in the man's hand is now in the hat. Having been told that something interesting was going to happen, the question that occurs to me is: "I wonder when the interesting thing (involving the orange?) is going to happen?"

(3) I might not assume anything, including that the man was telling the truth when he said "something interesting" would happen. Suspending judgement, I acknowledge that I have not got enough information to know (a) what the intended point is, (b) how long the episode will last, and thus (c) if the intended point has occurred or lies ahead. Suitably baffled the question that occurs to me is "I wonder what on earth is going on?"
While response (3) cannot be faulted on rational grounds, (1) and (2) are as justifiable as each other, though either may turn out to be based on an erroneous belief. Asking "I wonder how he did it?" is wrong if, in fact he did not do anything. And wondering when the interesting thing is going to happen is wrong if, in fact, it has already happened and there is to be no more to the episode.

Wishing to round off the story we can think up some alternative endings, e.g.:

(A) The man, being a good conjuror, and naturally with a flourish, plucks a banana out of an otherwise empty hat.
(B) The man, being a bad conjuror, hesitantly peers into the hat and takes out the orange, saying, "Oh dear. It hasn't worked."
(C) The man, being some sort of comedian trying out a new routine, says to me, "There is now a banana in the hat. And at this point I am going to turn it back into an orange." Whereupon he says "Arbacadarba [sic]. Bananas will now become oranges" three times, while turning the hat in an anticlockwise direction. He then reaches into the hat and picks out ... an orange - needless to say, with a flourish.

But let us suppose that what actually happens is as follows. The man, being a psychologist, puts the still-covered hat plus contents away in a box, saying "That's all there is to it. I was interested in what you made of it, and how you reacted. Now let's do something else."

As will become clear, this episode bears a crucial resemblance to a conservation task. What is missing, however, is the experimenter's questions. Without asking the subject any questions about the arranged events in a situation like this, we are unable to even start investigating what he/she makes of those events and the adequacy of that understanding. In the absence of questions, a rational and fully-justifiable reaction here, in the light of the background of deception and lack of information is one of total bafflement - hardly a promising start for a cognitive psychological enquiry. In the following story - as in the conservation task proper - questions referring to the arranged events are asked, and the job of interpreting what the subject does and says in terms of what he/she understands becomes manageable. However, as a consequence of changing things by adding questions, our view of what is to be understood by the...
subject must also be modified. From the point of view of a cognitive enquiry, the price to be paid is a severe one.

Second Conservation Story

I have been finishing off weeding the vegetable patch while my wife runs a bath for me. As I enter the bathroom she turns off the tap and pours bubble-bath liquid into the tub. She starts to stir up the soapy water with her hand and asks me, "Is that enough water for you?" I see that the level is just below the level that I like and say "No. Just a drop more, please." But as I am saying this there is an explosion in the kitchen. I rush off to attend to it. When I return from putting out the fire, I note that the taps are in the off-position and that the surface of the water is now obscured by foam.

After enquiring if I remembered to fill in the house insurance renewal form, my wife asks me the question, "Is that enough water for you?"

Without probing the bubbles and/or demanding clarification, what am I to make of this question, identical to the pre-explosion one? Do I assume that she didn't hear, or attend to, my answer the first time she asked it, and is now repeating the question? Assuming accordingly that no water was added and that the question refers to the same amount as before, my answer again is "No. Just a drop more, please". On the other hand I might assume that she did hear me the first time round and added some more. In this case the point of asking the second question will be to find out if the new amount referred to is sufficient. The new amount representing an increase, I presume it must be enough, say "yes" and jump in.

The answer I give here (whichever one), and why I say it, together with my accompanying conduct is to be best explained by the imaginary investigator without reference either to what actually happened (water added/not added), or to my understanding of the events and circumstances concerning the amount, or apparent amount, in the bath. The required information about the amount is not available to me. It is sufficient, in terms of the details given, and thus desirable in the interests of parsimony, to explain my "no" or "yes" answer here solely in terms of the circumstance that two identically-worded questions requiring answers are asked in sequence. If I am observed to say "no" again, I can be presumed to have justifiably heard the second question as a pointless, or memory-jogging repetition of the first, thereby calling for a
repeated answer. Answering thus, I would presumably anticipate that something else, relevant to getting to the desired end-point of a good bath, would happen. Thus I might expect my wife (a) to run more water, or (b) to tell me I'm a big boy now, and old enough to run my own baths. However, what could happen is that she does not tell me that she did in fact add more, and proceeds to run yet more - resulting in an overflow when I jump in. Here a justifiable mistake in interpretation has come to light, her second question not having been a pointless repetition.

If I say "yes", it can be assumed that I did not take the "repeated" question as a mere repetition. I would have seen the point to it as referring to a new state of affairs as regards the amount, even though - because of the bubbles - the information about the amount is missing. I would therefore expect the bath-running episode to be concluded. But what could happen now is that my wife says, "Actually I didn't add any. I just whipped up the foam, which maybe makes it look more." Whether or not the presence of foam did, or could, make it appear that there was more, again a justifiable mistake in interpreting the significance of the form of questioning could be seen to have occurred. There is no need to invoke any other sort of mistake. Sometimes such mistakes may not be discovered. I may (a) not be in a position to be able to check the correctness of my interpretation, or (b) not bother to, or (c) not even think of doing this.

One point here is to demonstrate that in an episode like this, questions are not to be viewed just as transparently referring to the context (tapping the subject's understanding of those events), but also are part of the situation that the subject participates in and responds to. But more than this, the aim in the second story is to show that, when it comes to providing a parsimonious psychological explanation of the subject's responses, this episode is best viewed solely as an "interrogative context" - i.e. where all anyone can know is that two identically-worded questions requiring answers are asked in sequence. Here it is only of secondary importance, from the point of view of interpreting conduct, what the questions are and what "in reality" they refer to. "Reality" here is not playing fair, as the relevant information concerning the amount of water is missing. This being the case, what the investigator needs to recognize is that the second question may - or, equally justifiably, may not - be taken by the subject as a repetition of the first question. This explanatory
framework may be sufficient to allow what is seen to be said and done to be accounted for.

What stood out as making the first story different from a conservation task was the absence of questions. In the second story we might again assume that nothing relevant happens (no water actually added). One thing that makes the latter story different from a conservation task is the absence of the intention to deceive. There is already a shortage of relevant information. If, in addition, the subject is justified in supposing that information is being deliberately and successfully hidden, then the only properly justifiable response to the second question must be "I don't know".

The price paid for taking my wife's second question as a repetition of the first is here, at worst, flood damage to add to the cost of explosion damage. Unlike in this case, we will see that there is always a price to be paid for taking the second question in a conservation task as a repetition - even though it guarantees the prescribed right answer. This price is a consequence of the deceptive component present in all such tasks. Any subject will presume that something, rather than nothing, is going to happen. The essence of a conservation task being that nothing relevant actually happens, the successful conserver is therefore in for a disappointment.

Questions need to be asked of the subject in order to understand what he/she makes of the events and circumstances. But, with the form of questioning used here, we do not need to bother now about those events and circumstances to explain what the subject does and says.
PART I - INTRODUCTION AND REVIEW
A. What This Investigation Is And Is Not About

This investigation is about conservation tasks and those who designed and used them. What it turns out not to be about is young children and the cognitive abilities possessed by them.

The issue of the presence or absence of reasoning abilities in young children has been the central one for the conservation enterprise. And the target of this group of experiments was consequently subjects in the age-range, 3 to 8 years. In line with the project of those involved in the enterprise, my aim is to explain the conservation task (C.T.) results. But in the attempt a somewhat startling conclusion emerges. This is that no version of the C.T. is able to be used to resolve any developmental issue of interest to psychologists.

Nor is my investigation concerned with finding a version of the C.T. that is able to serve as a usefully discriminating test. As the above conclusion applies to any test that defines itself by its properties as a bona fide C.T., this would be a fruitless project. Insofar as Piaget's original "classical" versions do generate age-related performance findings, and thus do appear to possess the intended discriminatory potential, I argue that the "interesting" pattern of findings from his "ingenious" experiments is able to be explained in terms that are - far from being developmentally significant - psychologically trivial.

This may sound like the wholly negative conclusion of an impoverished and pedantic research strategy. Limiting the horizon to
the conservation enterprise, it is certainly negative. If my analysis seems pedantic, it is so only because that enterprise was misguided in being founded on a combination of questionable assumptions. To tease out and interrogate in a methodical manner that which has tended to be taken for granted requires patience and caution. If it is impoverished, the source of that barrenness should, in the first place, be sought in the nature of the context investigated. We are dealing with a context whose intended essence is that nothing relevant actually happens. As such, I will claim it could never do the work for psychological theory required of it. But this raises the following questions:— (1) why has so much effort gone into research involving the C.T.? and (2) suspecting that the context was not a promising one from the point of view of yielding psychological insights, why was it still worth bothering about?

I will start with the latter question, putting the points of my argument in the form of a defense.

B. Why Do More Research On The Conservation Task?

The conservation task is now the most repeated experiment in the history of psychology. Research into it has been and continues to be accompanied by an extraordinary amount of confusion and controversy for developmental psychology. Controversy has followed on the heels of a wide range of Piaget's empirical findings, but as Bryant (1982 a, p.3) points out,

Dramatic as the controversy over transitivity has been, it looks like a polite drawing room comedy in comparison to the Grand Guignol of the debate over Conservation ... This is Piaget's best known and most important experiment and it has been repeated more often and argued over more strongly than any other of his techniques.

And the huge debate has been accompanied by a correspondingly colossal pile of literature on the topic.

This thesis is the latest addition to that pile. But its most conspicuous conclusion is that the conservation enterprise has been for the most part a waste of time. The initial undertaking to do more research involving the conservation task might therefore be regarded with some scepticism. When the primary aim of a piece of research turns out to be to criticize all previous research in the area, there should
be a good reason to justify doing it. If the charge of nihilism is to be rebutted, productive consequences of the critique should be readily discernable.

Among the points that may be made in my defence here, the following four stand out:

(1) The negative conclusions emerge; they are not explicitly anticipated

The ultimately negative conclusion that I would arrive at was not initially suspected. The investigation is not to be viewed as an explicit hypothesis-testing exercise, with the hypothesis being that the C.T. cannot be used to resolve any developmental issue. If there was an "hypothesis", it was merely that there has been something amiss with the main assumptions relating to the C.T. and what the obtained results might reveal. I started with a concern to show what developmental significance the results did have, but my approach called for a rigorous interrogation of assumptions to see where it led (see section F, below). To this extent, not only was there no explicit hypothesis as to how the crucial questions (Questions A and B: see chapter 2) were to be answered. I did not even know whether or not these questions would turn out to be legitimately posed. In fact they did not.

(2) The negative conclusions as a platform for asking other questions

Even when it becomes clear that the C.T. cannot do the developmental work assigned to it, this does not rule out the possibility of there being significant implications of the research for developmental psychologists. As a consequence of such a negative conclusion, other important questions present themselves, e.g. (a) How could so many investigators have been misled for so long and to such an extent? and (b) Why has the controversy about the presence/absence of reasoning ability in the young child revolved around the question of the presence/absence of conserving ability; and why has it, most conspicuously, centered on the findings of conservation tasks?
(3) The negative conclusions may be seen to offer as a "by-product" a parsimonious explanation of all C.T. results.

Taking a narrow view of the investigation as merely aiming to provide the best account of the C.T. results, the value of the negative conclusions may be seen in their yielding, as an associated by-product, an economical and empirically supportable explanation of those results. With the goal set at this level, there is no harm in at least attempting to have the last word on the subject of explaining all the C.T. findings. If this seems an immodest project, it becomes less so when the price to be paid is noted: that is the absence of an accompanying specific developmental insight. In the end, this fact itself might serve to justify the attempt to draw the line on the explanation here; i.e. by making it less likely that further time and effort will be wasted on trying to make the C.T. do work that it cannot do. Besides, whatever Piaget has taught us, it has not been through setting a precedent for modesty in making claims about the significance of the results of his experiments.

(4) The negative conclusions are less important than the means of arriving at them.

But in the end, the productive implications of the investigation are seen to lie less in the negative conclusions themselves than in the means of coming to them. Issues relating to approach and method are raised. They have relevance for the posing and answering of other developmental (?) questions (for example, see Class Inclusion, mentioned in the final chapter). These issues also have a bearing on reviewing methodology in psychology in general, and in particular in alerting us to the limitations of contemporary constructivist approaches to the interpretation of behaviour.

C. The Perceived Importance of the Conservation Task

The conservation task did not attain its perceived importance merely because of the amount of controversy it led to. From the very
start Piaget saw that which it was intended to test for as being of central significance in his theory of cognitive development.

For Piaget, to chart the development of intelligence is to chart the progressive move away from the "radical egocentricism" of the neonate towards the capacity for fully "decentred" thinking. The latter was seen as marking the culmination of mental development:

[The egocentric starting point is] a cognitive state in which the cognizer sees the world from a single point of view only - his own - but without knowledge of the existence of viewpoints or perspectives and, a fortiori, without awareness that he is the prisoner of his own ... It might be said that the egocentric subject is a kind of solipsist aware of neither self nor solipsism. (Flavell, 1963, p. 60, paraphrasing Piaget, 1958)

The end point, and goal of cognitive development, is seen as the decentred or detached objectivity epitomized by scientific thought. The subject is now fully able to consider his/her own point of view as merely one among many possible perspectives which can be flexibly co-ordinated in the service imposing order on the world.

It is with the appearance of "operational" mental structures, "assimilating" and organizing the world, that the subject is able to start breaking free of the prison of his/her own viewpoint. "The concept of conservation has been the focus of research inspired by Piaget's theory into the mechanism of intellectual development" (Bolton, 1972, p.89). And to get an idea of the specific importance of that concept in Piaget's investigations of the earliest form of operational thought, we need only refer to a brief "autobiography" (Piaget, 1969):

The central mechanism of intelligence is found in the construction of operations which derive from the general co-ordination of actions. The fundamental operations, such as uniting, ... serialating, ... equalizing, putting in correspondence, etc., are actions that are interiorized, ... reversible (through inversion and reciprocity), and co-ordinated in wholistic structures. ... The numerous works (concerning my investigations into the development of these aspects of operational thought) demonstrate clearly that operational structures are characterized by the forming of notions of conservation (conservation of a whole, of continuous quantity, in concrete material, of length, of surfaces, etc.) The beginnings of such notions can ... be observed
between four and six years of age at the pre-operational stage of thinking. [my emphasis]

The developmentally interesting results from C.Ts were thus elicited from children at the pre-operational stage. This investigation carefully avoids becoming unnecessarily involved with the debates concerning Piaget's posited stages of development. It is sufficient to note that this sub-stage is seen to begin at around eighteen months, at the end of the sensori-motor stage. By this time the concept of the permanent object has been formed. The infant has come to be able to distinguish events that are contingent on his/her own activity, from events and objects whose existence is independent of his/her actions. It ends at around the age of seven when the stage of concrete operations begins.

The most conspicuous marker of this latter point in development is given by the observation of failure on C.Ts giving way to success. The child ceases to be "seduced" by appearances and recognizes "that certain properties of a display are invariant despite changes in other properties in the display" (Bolton, 1972, p.64). This heralds an important step towards objective, centred thought, Butterworth (1980, p.22) summarizes the progressive "escape from egocentricity" over the first seven years of life:-

[It] lies firstly in the coordination of actions in infancy, hence to impose an invariant structure on the world. This is followed by a period of egocentric thought in which the coordinations established at the level of action must be reestablished conceptually. Coordinated internalized actions (known as operations) mark the onset of the concrete operational period and a logic defined by reciprocal and reversible relationships.

The onset of concrete operations does not mark a total 'liberation'. Internal mental operations are tied to the concretely present reality of the subject's environment. The child at this stage is not yet able to deal with the possible or hypothetical:-

The achievements characteristic of concrete operations involve the manipulation of things. It is because of this that they are called 'concrete': their starting-point is always the real and the given, rather than the potential.

(Bolton, 1972, p.70)

However this is as far as we need go with Piaget's stage theory. The ability to conserve is no longer in question and, indeed, C.Ts. are
rarely performed with anyone over the age of seven – the present investigation being an exception. The point here is that the start of concrete operations is seen to indicate a crucial step in intellectual development, and a discussion of the achievements of this stage is always closely linked to the notion of conservation. "Conservations ... provide the best index of the formation of operational structures" (Piaget, 1972 a, p.36) and, in turn, are invariably associated with the emergence of successful performance on conservation tasks.

Thus Winer (1980), while discussing class inclusion competence and performance in young children, states that:–

Conservation represents a type of behaviour that almost serves as a benchmark of concrete-operational thought, ... class inclusion [presumably representing] the ability to conserve the whole.

Flavell (1963, p.415) makes a similar point, expanding on it and discussing the implications in terms of Piaget's achievement, as well as in terms of the new-found achievements of the child. Having arrived at the stage of concrete operations, he/she is able to apply the concept of invariance in the presence of perceptual change.

[For this child] the world is beginning to stand still and stay put, a world which, like the child himself, knows something of law and order, and above all a world in which thought really counts for something, in which thought can be a more trustworthy guide to action than, perception ... It was an act of creative inspiration when Piaget hit upon the idea that a wide variety of cognitive areas - number, quantity, time, etc. - are in certain crucial respects mastered according to a common procedure: to discover what values do and do not remain invariant (are and are not conserved) in the course of any given kind of change or transformation; only when this is done is the way paved for further operations ... There is no question but that the formation of concrete operations is the richest chapter in Piaget's developmental story, in the sense of sheer abundance of highly interesting empirical data. It does not seem likely that this would or could have come about without the concept of conservation-formation and related unifiers.

(my emphasis)

I have emphasized that with the attainment of concrete operations the child is seen as taking a crucial step towards decentred thinking. With thought becoming "a more trustworthy guide to action than perception" the subject, e.g. in a volume C.T., is now less likely to be seduced by appearances. He/she is now able to see that there is no
change in the amounts to be compared even though it may look like there has been a change. Thus the perceptual "centering" on the comparative heights alone that would be a characteristic of pre-operational egocentric thought can now be overcome. With decentering, the fact that the containers are of different diameter can also be taken into account (see below).

In examining how egocentric thought is overcome, it is necessary to point out that for Piaget the key feature of flexible decentered thinking lies in the notion of reversibility. As Glachan and Light (1982) state:--

The transition to operational thinking involves the replacement of this egocentricism by a system of reversible operations, relations and classes which are decentered with respect to the self. In the intellectual field the child becomes able to attend to multiple features of situations, and his thinking begins to show flexibility.

The concept of reversibility is thus of central importance in Piaget's developmental theorising. It can be seen to be of crucial relevance in relation to his interpretation of successful performance on the C.T.

He meant that [with the emergence of the capacity for reversible thought, children] begin to be able to manipulate perceptual representations and that, when they see a perceptual change, to be able to cancel out its effects by imagining the inverse change. It is an interesting idea but what is most impressive about it is the large number of very diverse aspects of child development to which Piaget managed to apply it. He used it of course in his account of the conservation experiment in which the child actually has to witness a perceptual change: but he used it too, and as cogently, in his research on social communication, on logical inferences, on the understanding of ordinal number, on the logic of classes and even on moral development.

[my emphasis]

(Bryant, 1982 a, p.1)

Thus "the development of the concept of conservation is attributed to the operations of reversibility and compensation" (Bolton, 1972, p.75). Again we see that the C.T. evidence, now harnessed to the notion of reversibility, appearing at the very centre of Piaget's theory of
development. It is a pivotal point around which debates about is his theory of progress revolves.

To summarise the perceived importance of the C.T., we might say that its significance lies in its use as an indicator of when the developing child comes to be no longer able to believe that things happen or change capriciously. Things do not happen without a reason. When something is observed to change, the intellect is seen to demand that the change in circumstances are adequately accounted for. In the absence of any possible reason for a change along a certain dimension (number, amount, volume or whatever), the application of reversible, decentred thought leads to the conclusion that in fact there has been no change. This must be so even if the manifest appearance would seem to belie the conclusion of operational thought. For, on Piaget's approach to human understanding, "mere appearances" are essentially and invariably misleading and untrustworthy. It is the function of objective thought to overcome or by-pass them. In the C.T. we shall see that the subject (or his/her intellectual apparatus) is supposedly called upon to work out that, despite the appearance to the contrary, there has in fact been no change in the aspect of the array the subject is being asked about. The change that has occurred - e.g. the level of the liquid in the volume C.T. - can be fully accounted for without concluding that some liquid or powder has been lost or gained.

Put like this, the relationship between conserving ability and scientific thinking is emphasized. This becomes especially significant when it is remembered that Piaget's larger project, beyond child psychology, was to explain scientific thought and knowledge in terms of its genesis. If we adopt a certain view of the essential nature of scientific thought - and one shared by Piaget - we can discern what is, in effect, a "conservation principle" running through it: directing its course and serving as the central criterion for evaluating its products, i.e. scientific explanations of observed phenomena.

I will return to this theme in the final chapter. But I have said enough here to indicate that, and why, the issue of the presence/absence of conserving ability occupied centre-stage for Piaget and many others curious about the mind of the young child.
From this outline of the perceived significance of the C.T., three central questions arise:

(1) Given the importance assigned to conserving ability, does the conservation task constitute a valid test of it - or indeed of any rational ability?

(2) If not, what can it be taken to be a valid test of?

(3) Should the ability in question have been assigned such a crucial status in the first place, i.e. in relation to the processes of human understanding and their development? What is the price to be paid for such an emphasis on the notion of "conservation"?

The third question is addressed in the final chapter. The main part of my investigation addresses the first two questions and involves bringing to light the mistakes made by those who did, or sought to, use versions of the C.T. as a test of the reasoning ability in question. I have alluded both to the function of the C.T. in Piaget's theory and the actual results obtained. It is now necessary to discuss the actual task and consider the specific versions that this investigation will be concerned with.

D. The Versions of the Task Investigated

Significantly, Bryant (1982 a, p.3) says "In essence the [conservation] experiment is a simple one, and hardly needs describing" (my emphasis). However, he goes on:

But since all the discussion about this experiment turns in the end on its design it is as well to get the details clear. The experiment is about the understanding of invariance, and asks the simple question whether the young child understands that changing the appearance of a quantity by spreading out, say, a row of counters or pouring a liquid from a fat container into a thin one, does not affect the actual number of counters or the volume of liquid. The experiment is divided into two parts. In the first the child is shown two equal quantities (A and B) which also look alike - two rows of counters arranged alongside each other like two ranks of soldiers or two identical glasses holding the same amount of liquid. He is asked to compare A with B and usually judges them to be the same. Then the appearance of one of the quantities (B) is transformed, a transformation which we can
describe as B $\rightarrow$ C. One row for example is spread out or bunched up so that it is now a different length from the other. Once this is done the child is asked to compare the two quantities (A and C now) again ...

[my emphasis]

Bryant is correct in saying that the discussion about the C.T. "turns in the end on its design". But, this being the case, the above description is not complete. In spite of wishing "to get the details clear", he brushes over those relating to the experimenter's questions, merely mentioning that the subject is "asked to compare the two quantities" and, later, "asked to compare the two quantities ... again". But what does E actually say?

In this respect Donaldson's (1978, p.61) description of "the essential principles common to all variants of conservation tests" is better as it emphasizes that the same question is asked in each part of the episode.

(a) The initial equality of the critical attribute (length, weight, etc.) is combined with perceptual similarity (sticks placed so that their ends are aligned, balls of plasticene which are the same shape as well as the same weight, etc.)

(b) The child is questioned about the initial equality of the critical attribute, and accepts it.

(c) A transformation occurs which destroys the perceptual similarity without affecting the critical attribute.

(d) The child is again questioned about the critical attribute.

If, on the second questioning, the child still affirms the equality of the critical attribute then he is said to 'conserve' ... otherwise he is said to fail to conserve or to be a 'non-conserver' ... What actually happens when a 'non-conserving' response occurs [is that] in a short space of time, the child gives two conflicting answers to what, for an adult, is the same question with 'the same meaning'.

[p.62-63, my emphasis]

In the present investigation it is important to note the fact that Piaget's original, "classical" or "standard" C.Ts involved the "establishing equivalence" and "conservation" questions being identically-worded. This is also the case of most subsequently appearing "modified" versions of the task. Any variety of the "C.T." where this is not so, or where one or other of the questions is omitted
(e.g. see Rose and Blank, 1974) will not be viewed here as a *bona fide* C.T. Interesting though the results might be, they will not be directly addressed by this investigation, nor covered by the explanation of the results of *bona fide* C.T.s that I arrive at.

The actual question asked in both phases has the form: "Is there more in this one [row, container, lump of plasticene, etc.], more in that one [the other row, container, etc.], or is there the same [number, volume, amount, etc.] in each?" Alternatively, referring to Bryant's description above, the experimenter (E) may say: "Is there more in this one [A], less in this one [A], or is there the same in each?" While several authors have pointed out that small changes in the wording of the question can influence the results obtained, the latter was the question normally asked in my own empirical studies.

The significance of the C.T. derives from the fact that children under the age of seven commonly fail to give the right answer to the second question in standard tasks. Piaget concluded that these children do not understand the principle of invariance, while the older children, who succeed, do. Spelling out this perceived significance and making the link to what was said in the previous section, Donaldson (1978, p.62) reminds us that:

Piaget takes this ... as evidence of failure to *decentre* and of failure to reason. The correct answer, he says, depends on the ability to make an inference from two premises - namely: (1) these things were the same length (or weight etc.), before; (2) nothing has been done which alters the length (or weight etc.) - to the conclusion: therefore they must be the same length now, even though they look different. Failure to reason in this way is held to stem from an inability to *decentre*, both with regard to the immediate perceptual situation and with regard to the relation between one moment in time and a succeeding one. The child centres on one feature of the immediate situation and neglects others ... Also he centre`s on the present moment, failing to think back to how things were before, failing to see that one act is in principle reversible ... The ability to make use of this principle of reversibility in one's thinking is, for Piaget, one of the main signs of having reached the stage of *concrete* operational thinking.

We thus have here the rationale for the existence of that category of C.T.s on which young children tend to fail. But in giving us these tests Piaget also set the scene for the controversy about the interpretation of the child's failure on them. This will be discussed
in chapter 3. The point here is that out of this dispute sprung the other category of C.Ts: those on which the young child tends to succeed in giving the conserving response. Piaget was more concerned with using the C.T., via a theory of intellectual genesis, to help explain successful, objective thinking and knowledge itself. However, what Piaget viewed as unproblematic evidence supporting an explanation here, others saw as problematic and itself still to be adequately explained. Thus there arose a major preoccupation among developmental psychologists (with more modest horizons than Piaget) with explaining what they saw as the interesting and surprising mistakes of young children on his tasks. Piaget's experimental ingenuity was widely applauded, his results were undisputed and the fact that they were of developmental significance unquestioned. But it was argued that Piaget's interpretations, here and elsewhere, were too "pessimistic" with respect to the child's intellectual competence and that there were other possible reasons for failure. The results of their "modified" C.Ts were intended to have a bearing on this issue. To highlight the polarisation of views here, we might simply say that the "pessimist" camp used C.Ts to show that young children could not reason, while the "optimists" employed them to show the opposite.

Any general explanation of C.T. results should try not only (1) to address the question of why young children fail on standard C.Ts while older subjects succeed, but also (2) to address the question of why young children fail on the one sort of C.T. while succeeding on the other. (See Questions B and A: chapter 2, section A).

My project involves the attempt to offer a parsimonious, empirically-supportable general explanation. In the first place this called for the selection of two representative, bona fide C.Ts corresponding to the respective "standard-failure" and "modified-success" categories.

The Tasks

The versions of the tasks used and taken as representative exemplars throughout are designated "P" and "O" respectively. "Representative" young children, say in the 3 - 5 years age-range, would be expected to fail on the first and succeed on the second. They are versions of the conservation of volume task (if the subject is asked to
compare quantities of liquid) or the conservation of amount task (if the subject is asked to compare quantities of powder). The liquid used was generally water, though sometimes a drink such as orange juice. The powder used was always Carnation "Coffee-Mate" - a fine grained milk-substitute substance. No significant difference in performance was observed in relation to the type of liquid used, nor between liquid and amount versions.

The arranged difference between "P" and "O" lies only in E's verbal and non-verbal behaviour, the materials and order of arranged events being the same in any direct comparison between the two tasks. The exception is that it should be arranged that in "O" container B (see below) should be chipped or have a speck of dirt on the rim. In the first phase of every episode the containers used are two identical (usually half-pint) glasses, A and B, which are filled to the same level with the water (or powder). The second phase involves the introduction of a wider (usually one-pint) glass into which the contents of B are poured.

The descriptions that follow refer to the procedure adopted for the young child, usually taken from the nursery where he/she had been playing with other children. It might here be noted that when "P" is subsequently done with adults, the subject is not invited to come and play a game, and also that task "O", in the form given, appears a particularly odd and inappropriate sort of experiment to try to do with adults - even more so than "P". Such facts need to be borne in mind when it comes to making comparisons between age groups and attempting to draw developmental conclusions (see chapter 7).
Classical (or Standard) Version of the Volume C.T. "P"
(after Piaget, 1952)

1. E says to the subject(s): "Let's play a game / Let's do something interesting - with the beakers and water" and gets S to attend to the beakers A and B, filled to the same height with water.

2. E asks the "establishing equivalence" question, Q₁: "Is there more in this one [indicates A], less in this one [A] or is there the same in each [A and B]?

3. If adjustments are demanded by the S to ensure equality, they are carried out, and Q₁ is repeated until S answers with "the same". At this point E says "Now watch this" or "Watch carefully", and introduces Container C and pours the contents of B into it.

4. E asks the "conservation question", Q₂: "Is there more in this one [A], less in this one [A] or is there the same in each [A and C]?

5. When the subject answers, E terminates the episode, eg. by saying "Thank you. That's all. Let's go back to the nursery" or "Thank you. O.K., lets do something different."
1. E says to S: "Let's play a game / "Let's do something interesting - with the beakers and water. We'll give one to Tommy to drink and one to Susan, and make it fair by giving them equal amounts."

2. E asks the "establishing equivalence" question, Q₁: "Is there more in this one [A], less in this one [B], or is there the same in each [A and B]?

3. If adjustments are demanded by the S to ensure equality, they are carried out, and Q₁ is repeated until S answers with "the same". At this point E says "Oh wait a minute! Look, this glass [B] has a chipped/dirty rim. We can't let Susan drink out of that. What shall we do? I know, let's put her drink in another glass." E searches around the table behind and, deciding that C "will do" pours the contents of B into it.

4. E says: "Where were we / what were we saying? [Q₂] Oh yes, ..." and proceeds to ask the conservation question", Q₂, "... is there more in this one [A], less in this one [B], or is there the same in each [A and C]?"

5. When the subject answers, E terminates the episode, eg. by saying "Thank you. That's all. Let's go back to the nursery" or Thank you. O.K. let's do something different."

E. The Empirical Evidence

(1) Sources of data

Children

(i) The Literature

The conservation literature constitutes a vast repository of illuminating information on versions of classical and modified C.Ts
and the variety of results obtained from 3 to 8 year olds. It has to be pointed out, however, that this source was illuminating often for the information omitted. Thus, what in this work are considered important details about procedure (e.g. in relation to the "extraneous" things that E does and says) and observable conduct (e.g. the manner of answering and aspects following answering) were customarily missing.

(ii) Tape-recorded interactions in task contexts

Between 1982 and 1986 I talked to 88 children in the 3 - 5 years age-range. Most of these subjects attended the nursery connected to the Edinburgh University Department of Psychology, and were from predominantly middle-class backgrounds. All except two of the children (tested in France) were English-speaking. This generated approximately 24 hours of tape-recorded interaction between individual subjects (occasionally pairs) and myself in a variety of mainly Piagetian contexts. Besides varieties of the C.T., these contexts included versions of the class-inclusion experiment and a replication of the "tunnels task" - the latter being employed by Piaget (1970 b) in his investigation of the child's understanding of movement and speed.

(iii) Video-recordings

An important source of observations involved the filming of young children doing Piagetian tasks, with the emphasis on the C.T. Thus in 1986 I filmed 13 subjects from the nursery while they participated in such tasks, including versions "P" and "O". Teaching tapes involving classical and modified versions of the C.T., obtained from the psychology departments of Aberdeen and Edinburgh universities, also proved useful.

Adults

Overall I involved 43 adults in my empirical investigation. In a preliminary phase I "tested" 6 subjects on the classical task, "P", the substance used being water on half the occasions and Coffee-mate powder on the other half. The next phase involved 8 subjects who, as well as
being (a) encouraged to discuss the volume C.T. episode (again "P") as it happened, were also (b) asked to write a retrospective commentary on their impression of what had taken place. The transcripts from these tape-recorded interactions and the written protocols are reproduced in Appendix I.

The third phase involved "testing" a group of 29 adults on the powder version of "P", but this time embedding it in the context of an enjoyable, competitive game with prizes (see Chapter 7, section E).

(2) **Empirical observations**

These are referred to in the text under the headings of the nine "Empirical Notes":-

1\textsuperscript{st} **Empirical Note** (Chapter 4)

This involved young children and replications of various C.Ts - including the classical and "naughty teddy" (see chapter 3) versions of the number C.T., and versions "P" and "O" of the volume / amount C.T. The aim here was simply to draw attention to some general features of the arranged contexts and the nature of the experimenter - subject interaction.

2\textsuperscript{nd} **Empirical Note** (Chapter 4)

This involved a replication of the above-mentioned "tunnels task", the subjects again being 3 - 5 year olds. The aim corresponded to that of the 1\textsuperscript{st} empirical note, one point being to draw parallels between features of this episode and the C.T. The relevance of such a comparison is highlighted in the final chapter.

3\textsuperscript{rd} **Empirical Note** (Chapter 5)

The subjects involved were 3 - 5 year olds and my interest was especially in those instances where the conserving response was elicited. This tended to make the focus of attention the modified version, "O". Specifically, I set out to confirm:-(1) that subjects of bona fide C.Ts start off expecting that something significant will happen, without knowing when, (2) that this belief is the product of a combination of absent and misleading information common to all C.Ts.
(3) that successful subjects, unlike non-conservers, answer the second question, Q2, correctly while incorrectly believing that the episode is not finished.

Among the evidence sought, the most crucial related to the subject's reaction to the termination of the episode by the experimenter, and to how conservers differed from non-conservers in this respect. (See also Appendix Ia)

4th Empirical Note (Chapter 5)

The observations made here were intended to be linked to those relating to the previous empirical note. From an original selected sample of 59 nursery children, 30 had been initially tested on "P" and 29 on "O". Of the former, 19 had failed and 11 had succeeded in conserving. Of the latter, 12 had failed and 17 had succeeded. The first potentially relevant point made was that 3 of the successes, but none of the failures, changed their answers when I indicated to them after they had answered that that was the end of the episode. I also looked at the effect on subjects (a mixture of non-conservers and conservers) of, instead of actually terminating the episode, asking them "what would you say now if I told you that was the end?" The prediction here was that more conservers than non-conservers would change their answers.

5th Empirical Note (Chapter 5)

This involved 14 adult subjects and task "P" (For full transcripts and protocols on 8 of these, see Appendix I). The aims here were to see if (a) a case could be made that the representative adult succeeded on "P" in the same way that the representative child succeeded on "O", and if (b) this might be linked to the above temporal ambiguity feature identified in the C.T. The crucial evidence here, therefore, again concerned the subject's observed / reported reaction to E's termination of the episode.

6th Empirical Note (Chapter 6)

This involved a reanalysis of the above evidence from adults. The focus is now switched from the temporal aspect of the context to what has generally been intended to be viewed as the key event of the episode: the transformation of the array. The aim was to see, from
observation and from the subject's report, in what way and to what extent the subject attended to this event and its perceptual consequences.

7th Empirical Note (Chapter 6)

The focus moved back to the successfully conserving child. The view being examined was again whether the representative 3 - 5 year old could be seen to succeed on "O" in the same way that the adult succeeded on "P". This time, however, the hypothesis being considered was that both "succeed" by ignoring / disregarding the transformation and its consequences. The "false positive" criticism of the optimists' project (see Chapter 3) was relevant here.

8th Empirical Note (Chapter 7)

The aim here was to demonstrate that adults can be induced to fail on a bona fide version of "P" and that a case can be made that they do so in the same way and for the same reason that the representative young child fails on "P". As mentioned in the previous section, this involved presenting an intact version of "P" to subjects, but one that was embedded in the context of an enjoyable and interesting game.

9th Empirical Note (Chapter 8)

This involved presenting the initial array and transforming it as in "P" and "O". But instead of the characteristic "closed" form of questioning at the "establishing equivalence" and post-transformation phases, I employed an open-questioning procedure, asking "What do you see in front of you?" and "Now what do you see? / What happened?", respectively.

F. General Strategy of the Investigation

The main part of this work comprises an attempt to arrive at a single-factor, empirically-supported level of explanation of C.T. results. The findings are intended in the end to be "self-sufficient", resting on evidence readily available to anyone concerning (a) the properties of the task and (b) the conduct of subjects. However, it is
to be noted that the observable conduct of the experimenter (i.e. in relation to what he/she arranges to occur) and the subject (in response to what has been arranged) becomes exactly that which is at issue. Because of this, my analysis identifies the problem, in the first place, as one of description rather than - as has been the case - one of psychological interpretation.

What is seen to be therefore required is a framework that can aid in bringing to light suspected descriptive errors or oversights by those involved in the conservation debate. In chapter 2 I suggest that the work of Merleau-Ponty may be helpful in this respect. This is confirmed in chapter 8; and in Appendix II I provide the background from which my specific descriptive challenge emerges. At the same time I indicate some wider implications for those dissatisfied with the constraints imposed by Piagetian and post-Piagetian constructivist approaches to cognition and its development.

The more general lesson to be drawn from the phenomenological approach is that taken-for-granted assumptions should not be taken for granted. Assumptions are necessary for any investigatory discipline. But we should be aware that they shape and pervade our enquiries. And if we have not already made explicit what presuppositions are in play, we should at some stage be prepared to identify, examine and - if / where necessary - allow them to be challenged. This is particularly important in those cases where a domain of enquiry has manifestly run into difficulties. It is therefore a particularly pertinent prescriptive message for contemporary experimental psychology, where the facility for interrogating taken-for-granted assumptions is often conspicuously missing. MacLeod complains:

I am regularly appalled by the innocent, unthinking dogmatism with which students accept the psychology they have been taught as though it had been handed down on a tablet from Mount Sinai. (They are critical), but only in the sense that they are uncomfortable about a statement which lacks a confidence level, or an observation reported without a control group. What one misses is the critical spirit which leads back to the challenging of implicit assumptions.

[my emphasis]

This criticism is no less justified today than in 1965, when it appeared. And it is tempting here to relate the reference to the Mt. Sinai tablet to a level of uncritical reverence among researchers in
experimental child psychology for the work of Piaget. This may not be the case in relation to the answers he offered. But he is almost universally praised for the supposedly interesting developmental questions he posed, for raising "many provocative issues" (Bryant, 1982 a, p.5), and for doing so with what all seem to agree are simple but ingenious experiments.

There are already a number of assumptions here that need not be immune to some fundamental probing. And the conservation enterprise, as well as representing the issue that has been the most "provoking", is also the one that has run into the most severe difficulties (see chapter 3). This then would seem to be the arena most ripe for an assumption-challenging approach to clarify what is, and what is not, at stake.

Husserl's phenomenological reduction (e.g. see Husserl, 1927) was intended to provide a method which could lead to a "return to the things [i.e. phenomenal] themselves" as they arise in experience in all their purity, stripped of overlaying assumptions. What was prescribed was an investigatory approach based on the "bracketting" or "putting out of play" of all assumptions. We can certainly take issue with the practical possibility, or even intelligibility, of such a project, going along with Merleau-Ponty (1962, p.xiv), who said that "the most important lesson which the reduction teaches us is the impossibility of a complete reduction." But, where descriptive clarification is called for, there is no doubt that a "bracketting" orientation can be a useful one.

My approach to the conservation task episode, as I have said, is founded on a project of descriptive clarification. And chapters 4 to 7 can be seen to represent a step-by-step attempt to put various apparently unquestioned but challengeable assumptions out of play. My initial purpose is merely to see where this leads, and to find out how far I can get in my project of arriving at a useful understanding of the aspect of conduct in question - i.e. C.T. performance - without those assumptions. The conclusion will be that we can get far enough without having to readmit the main presuppositions - including the "developmental assumption".

A parallel strategy that will be discerned here (see chapter 6) is one based on exploiting key assumptions inherent in the Piagetian approach to questions of intellectual development and, specifically, to
explaining successful performance on the C.T. This involves making assumptions that are in play explicit in order to push them to their limits. As such, this sub-strategy is able to be seen as closely linked to the above general strategy, complementing it rather than deviating from it.

NOTES

'Logotheti (personal communication) drew my attention to a nice example of this from her own experience of doing C.Ts. with Greek children. In Greek there are two equally appropriate ways of saying "bigger". Yet the results obtained largely depend on which form is used in the questioning.
CHAPTER 2

SUSPECT QUESTIONS: A PROBLEM OF DESCRIPTION RATHER THAN INTERPRETATION?

A. Introduction

In Chapter 1 I provided some background and set the goal of my investigation. Thus I set out to offer as parsimonious as possible an explanation of the results of all C.Ts. Taking the two representative versions, i.e. "P" on which the representative young child fails and "O" on which the same subject succeeds, any such explanation should allow the following questions to be answered:-

QUESTION A: Why does the subject involved fail on task "P", while succeeding on task "O"?
QUESTION B: Why does the young child fail on "P", while the adult succeeds on the same task? (The developmental question).

These questions therefore become my focus. How they are answered will depend on how the given evidence from subjects' conduct on the two versions is interpreted. In Chapter 3 I will identify and discuss a major issue in the dispute over interpretation before embarking, in Chapter 4, on the central argument that culminates (in Chapters 6 and 7) in the required answers to these questions. In the present chapter I argue that the central problem might turn out to be one of description rather than interpretation. The muddle over interpretation, illustrated in the following chapter, merely reinforces this point. Questions relating to (1) what the task episodes consist of, and (2) what exactly is the "given evidence" obtained from them, still can be and need to be asked. In other words, before setting about trying to answer Questions A and B, we need first to check that they are legitimately posed questions. I find that, as they stand, there is good reason to suspect that they are not. Certain types of descriptive error, relating to the way the task and the response to it are viewed, are anticipated. This
sets the scene for the sequence of challenges of the argument-proper, the issue of the shared source of error being returned to in Chapter 8.

B. Piaget's Theory: Some Reasons for Scepticism

Let us suppose that Piaget's theory is wrong. Further, let us suppose that it is wrong in the most fundamental way that a genetic theory, purporting to account for progress in some domain, can be wrong. That is, it cannot provide an account of any sort of progress in the domain(s) addressed by the theory. Such a supposition has important consequences for the way questions A and B are to be approached and needs some justification.

Piaget's theory of genetic epistemology does not merely set out to explain progress when it is observed to occur in the living world (including people and their cultural products). It is a "necessitarian progressivist" theory (Boden, 1979, p.119) in that it insists that progress must occur:

I fear I have given the impression of a man who has touched many fields. But in fact, I have followed a single goal that has remained always the same: to try to understand what a living development is in all its perpetual construction and novelty and its progressive adaptation to reality.

(Piaget, quoted by Rotman, 1977, p. 8; my emphasis)

Piaget's overriding concern is the progressive growth of knowledge - by which he means scientific knowledge.

The specific problem of genetic epistemology is that of the growth of knowledge: the passage from an inadequate, relatively poorer kind of knowledge to one richer both in intention and extension ... Science is always developing.  

(Piaget, 1972 a, p.16)

Notwithstanding this ambitious single aim, "the fundamental hypothesis of genetic epistemology is that there is a parallelism between the progress made in the logical and rational organisation of knowledge and the corresponding formative psychological processes" (Piaget, 1970 a, p.13). Because of this and because his empirical study of the growth of intelligence is intended to support a biological theory of mind and explanation of knowledge, Piaget's theory links several
domains. Rotman (1977, p. 18) outlines the relationship between epistemology, psychology and biology in Piaget's theory:-

[Piaget's] genetic epistemology extends his psychological findings from the structure of children's thought to the structure of scientific and mathematical knowledge, [starting] from the thesis that our capacity for rational thought must be seen within a biological context, as an evolutionary invention or adaptive device that enables us to respond to, to control, and above all to survive in, our physical environment.

While Piaget's empirically-supported theory of cognitive development can be, and has been, taken on its own, he would not want it to be so for those wishing to evaluate his work and its significance. Thus we arrive at the statement that this work must be seen as an attempt to answer what have traditionally been seen as philosophical questions, in terms of biological function and mechanism, the telling evidence that holds the edifice together coming from the psychological domain.

Piaget therefore exposes his theory to challenges from critics working within each of these domains. Psychologists often criticize him on theoretical grounds for neglecting language and social factors in favour of the world and logic of objects, and on empirical grounds for the "loaded" design of inadequately controlled experiments. In relation to the former criticism:-

[Piaget's] logic of the real leaves culture, language, and social formations in a blurred penumbra.  
[Rotman, 1977, p.180 - see Appendix II (B) for extended quotation]

Again, those philosophers who have bothered with Piaget's work can be particularly hard on him. For example Ennis (1978) criticizes his narrow notion of logic; and Garfield (1983) points out that Piaget's epistemological orientation depends on an attempt to find a middle way between empiricism and rationalism, whereas he merely succeeds in oscillating back and forth between these two epistemological postions. In general, when Piaget addresses philosophical issues he is often to be found guilty of convolution, paucity of argument and obfuscation of the central points at issue (Phillips, 1982, p. 14). On the other hand, when, as is often the case, he seems to reject the usefulness of philosophy in addressing questions about mind and knowledge, he can be accused (a) of a misapprehension as to the scope and goals of
philosophical knowledge (Mischel, 1976, p. 164), and (b) of incoherence insofar as he fails to acknowledge that such a rejection itself constitutes the taking up of a philosophical position (Zaner, 1966).

Piaget comes in for criticism for the way he links and argues across the above domains. Phillips (p. 25) takes exception to Piaget's critique of empiricism, pointing out that Piaget overlooks that epistemology is concerned with the logical justification of knowledge, not the details of the biologically-based process by means of which knowledge is acquired. This author (p. 24), referring to Piaget and Inhelder (1972) delivers the severest of reprimands:

This paper effectively dispels any lingering doubts about Piaget's philosophical acumen. It rapidly becomes apparent that Piaget ... [has] little understanding of the empiricist philosophical tradition ...

Piaget is accused of not understanding the role of epistemology and the problems that must surround a biological explanation of knowledge and, more generally, any attempt to offer a scientific account of science. Husserl (e.g. 1982), who also adopted a genetic approach to knowledge, was particularly aware of the "scientism" difficulty. Piaget is thus charged with failing to show the relevance of psychology for epistemology. How could an understanding of the psychological genesis of beliefs lead to them being able to be justified as knowledge claims?

Again quoting Phillips (p. 28):

It is interesting to reflect upon what would have to be argued in order to establish that issues concerning genesis were relevant to whether or not knowledge claims were valid or justifiable. It would need to be shown that there are some psychological or developmental processes, the unadulterated operation of which ensures that a true or valid or warrantedly assertable endpoint would be reached. Piaget is right in believing that the dominant tradition in epistemology denies that this can be shown (see also Feyerabend (1978): no methods can be ruled out, or counted on, in the search for scientific truth) ... Piaget has not been able to make a break-through and he has often nimbly sidestepped the central issues.

But Piaget offers, at its base, a biological theory of progress, involving an extrapolation of a view of biological processes to those of thought and the growth of knowledge. Conceivably, we might therefore
allow Piaget to be judged by the way his theory copes with the biological realm.

Even sympathetic authors have been sceptical about the ability of Piaget's theory, as it stands, to account for progressive change and innovation (e.g. see Gruber and Voneche, 1977, p.xxxv). Goodwin (1982) finds Piaget's assumption that biological evolution is to be viewed essentially in terms of "problem-solving processes, initiated by environmental challenges which are to be conquered or overcome", as unduly restricting. It might be seen as part of the force of Piaget's theorising to forge a close link between a "cognitive" view of biology and a biology of cognition (see chapter 9, section B). But I would suggest that we have additional good reason to be uneasy about lifting the concept of problem-solving from the area of cognition and applying it to the biological arena, if this is in turn to be applied back to cognition in the course of attempting to explain it. A metaphor turned back on itself like this is liable to mask difficulties. Apart from that, the equating of thought with problem-solving may constitute an unduly narrow view of cognition in the first place (see Appendix II and chapter 9).

In general, it is not unfair to say that Piaget's view of evolution rests on a neo-Lamarckian progressivist notion of adaptive processes that has been widely discredited by biologists.

Piaget's overall evolutionary theory ... in the crudest terms portrays evolution as directed and inevitable, the result of increasing adaptation, harmony, and equilibrium, whose stages unfold and transcend each other according to the universal biological property for recapitulation that Piaget calls convergent reconstruction. It would be a rare contemporary biologist who accepted a theory of this sort. Indeed, leaving aside the obscurities surrounding the notion of equilibrium, most would reject the ideas of necessary progress and stage-by-stage recapitulation, seeing them as relics perhaps of the pre-Darwinian theorising of Spencer and Lamarck.

[Rotman, 1977, p.115]

Boden (1979, p. 119) makes a similar point and adds that some modern biologists "would even deny that any objective sense can be given to notions of evolutionary progress".

Piaget insists that his psychology is "impossible to understand if one does not begin by analysing in detail the biological presuppositions from which it stems" (quoted by Boden, p.106). If we are to allow that
these presuppositions are possible to understand, I have said enough to suggest that they are suspect. And if the general biological framework is suspect we can presumably look to his psychology for symptoms.

In the first place we might expect that key explanatory concepts lifted from within that framework and applied to a theory of cognition to be also problematic. Four such notions that spring to mind are assimilation, accommodation, equilibration and the role of conflict. Several authors, for example, Gurwitsch (1964), have questioned whether the concepts "assimilation" and "accommodation" have anything more than descriptive value in the domain of psychology. More recently, referring to these two terms as well as the prevalent scepticism about the notion of "equilibrium", Boden (p.24) says:

Whether the [use of these three terms is] ... clear in psychological or epistemological contexts (or in wider biological contexts, such as evolutionary theory and embryology) is controversial. So too is the question of whether they offer explanations of cognitive growth, as opposed to polysyllabic descriptions of it ... [Bruner] dismissed his concept of equilibrium as 'surplus baggage', contributing nothing to theory or to experimental design save some 'confusing imagery' and serving merely to give Piaget 'a comforting sense of continuity with his early biological apprenticeship'.

I have mentioned doubts about the role of conflict between organism and environment in evolutionary theory. And, while Piaget saw conflict as "the motor of development" (T.G. Bower - personal communication), Bryant (1982 b) is correspondingly sceptical about its application in a theory of intellectual growth.

In the second place, if the theory is suspect in its ability to account for progress in any of the domains that it touches upon, we might predict that there will be something correspondingly suspect about the evidence invoked to support it. The domain of interest here is that of intellectual development, and the crucial evidence invoked in support of a theory of some sort of progress is that obtained from observing the performance of subjects of different ages on tasks like "P". This takes me back to the C.Ts investigated, the approach to describing the conduct associated with them and the questions that can and cannot be asked concerning the observed and described behavioural results.
C. Addressing Questions A and B: Implications of These Doubts

The original standard form of the C.T. (see "P") was just what Piaget and his theory needed, and is helpfully viewed as a direct product of a theory of progress that required evidence of progress in the domain addressed by the theory. Thus his view of the task and what is involved on the part of the subject are best understood in terms of the sort of theory of cognitive progress for which the results of the standard C.T. were originally intended to provide support.

In the last section I gave a critical outline in order to substantiate the assertion that there is good reason to suppose that there is something basically wrong with Piaget's theory. I went on to draw the inference that it is therefore justifiable to suppose that there is something basically and correspondingly wrong with the evidence used to support that theory. However even Piaget's "optimist" critics do not dispute that evidence, nor the fact that it could support his theory. They praise Piaget for his experimental ingenuity, customarily saying something modest to the effect that "they are merely standing on Piaget's shoulders". Then they tend to suggest that the "incontrovertibly robust" evidence of the child's "interesting" or "puzzling" failure on a task like "P" does not necessarily support his theory. That is, what they dispute is his particular "progressive" interpretation. They suggest that the non-conserving response of the young child could be due to some failure other than that resulting from a lack of reasoning ability. Thus there is agreement that a developmental account of the results is called for - i.e. in terms of the progressive emergence of some ability with age - but, in this instance at least, perhaps not Piaget's account. There is still seen to be implicit support for some theory of progress to be derived from the C.T.

However I went further in suggesting that what is basically wrong with Piaget's theory is that it may be unable to provide an account of any sort of progress. Continuing to view the design of the crucial C.T. as a direct product of his theory, we might infer then that the results from the task are unlikely to provide evidence for any theory of progress. This might seem absurd and suggest that I have arrived at an impasse. As it stands a task like "P" must be seen to provide supporting evidence for some theory of progress. After all, any
accepted description of it shows that the subject has got to choose
between right and wrong answers and it is to be presumed that getting
the right, rather than wrong, answer involves some ability. When it is
confirmed that younger subjects fail to give the right answer while
older subjects succeed, the conclusion that the C.T evidence must have
some developmental significance is impossible to avoid.

There is only one way of escaping from, or at least putting off,
such a conclusion. Thus, if we are to suppose that the evidence in
question is such that it cannot provide support for a story of progress,
and yet find that the accepted description of the evidence does support
such a story, then the accepted description must be wrong in one or more
ways.

Now, for better or for worse, the C.T. not only exists, but exists
in a multiplicity of forms. Here it is the classical task "P" and the
derived version "O" that came under close scrutiny. Certainly the
dominant question is and remains the developmental one referring to the
results of classical C.Ts. I have posed this in the form of Question B.
But, thanks to the optimists' project, we now have the other question:
Why do the subjects investigated (i.e. young children) tend to fail on
classical versions but succeed on modified versions? I have posed this
in the form of Question A. The value of being able to pose the latter
question separately, and the advantage of tackling it first lies in:
(a) the recognition that much of the conservation controversy now
revolves around the mixed bag of failure and success results obtained
from young children doing classical and modified C.Ts; (b) the
recognition that the posing of Question A does allow the introduction of
the developmental assumption to be put off. We may arrive at a level of
explanation of the young child's failure on classical, and success on
modified, versions: e.g. something to do with reasoning, or memory,
attention, communication, etc. But it does not become converted into a
developmental account until (1) evidence from another age-group is
acknowledged and admitted into the investigation, and (2) some reason
why it could be and should be viewed as an age-dependent result is put
forward.1

In posing Question A, being only immediately concerned with one
age-group of subjects (3 - 5 year olds), a developmental inference is
premature. The developmental question is thus able to be put in
abeyance.
Neither Piaget nor those who criticized his pessimistic interpretations did separate this question from the developmental (?) question Piaget raised. Indeed Piaget usually refused to concern himself with questions such as Question A, and with tasks such as "O", which he would not have viewed as a bona fide C.T. His answer to Question A would take the following form: The young child fails on "P" because it is a good test of concrete reasoning ability, and the child has not got it. The fact that the child succeeds on "O" shows that it cannot be a good test of the reasoning ability. On the other hand, the optimists would presumably answer Question A: the young child succeeds on "O" because it is a good test of reasoning ability and the child possesses it. The same child fails on "P" because it is not a good test of reasoning ability in the young child.

At the moment I am not concerned with answering either Question A or Question B (see Chapters 4 to 7), nor with the nature of the dispute between pessimists and optimists over the interpretation of what occurs in C.Ts (see Chapter 3). I have merely raised the possibility that the source of the controversy may be able to be traced to a confusion over that which is to be interpreted. That is, the "conservation problem" may in the first place revolve around a problem of description rather than interpretation. As such, the very legitimacy of the two questions posed (Questions A and B) comes into question and will need to be subjected to scrutiny. Attempting to answer inadequately framed questions is a misguided excercise.

This merely gives a foretaste of the danger of becoming involved with a dispute over interpretation if it has not been ensured that the base level of description that we work from is sound. How the evidence is described determines the questions that can be asked about it. If, for example, we are able to doubt the appropriateness of the terms "success" and "failure", then asking why one subject fails while another succeeds on the same C.T. (Question B), or why a subject fails on one version while succeeding on another (Question A), can be correspondingly guaranteed to be misleading.

I have shown that, even before we look closely at what actually happens in tasks such as "P" and "O", reservations about the sort of theory that originally provided the rationale for them gives us good
reason to be sceptical about that which examination of the evidence can reveal. When I actually look at these tasks in operation, the doubts will be seen to be confirmed rather than dispelled.

But before I actually move to the evidence I will need some general guidelines for description. The framework that provides these also alerts me to two prejudices associated with cognitive - constructivist approaches in psychology, their epistemological source, and their likely consequences when it comes to describing (a) the task, and (b) the behavioural results.

D. Guidelines for Description: Two Sorts of Bias Anticipated
(See also Appendix II)

1. Possible Sorts of Descriptive Error

Basically, what I need to ensure is (a) that what a subject is described as doing is related to an episode that the subject does, rather than does not, participate in, and (b) that what a participating subject is described as doing is related to a cognitive accomplishment that is, rather than is not, required of him/her.

The claim that I will argue is that the whole conservation enterprise has gone wrong precisely in these respects. What the subject is described as doing depends on what the arranged situation is viewed to consist of. Possible mistakes that can arise, therefore, are:-
(1) those of commission - i.e. where something that does not occur is described as taking place; and
(2) those of omission - i.e. where significant aspects of what the C.T. episode consists of are overlooked, resulting in the neglect of alternative and perhaps more helpful ways of describing what takes place.

The suggestion here is that both these sorts of error have occurred. Thus descriptive assumptions which have served as the platform for interpretation, and which Piaget and his optimist critics share, may be able to be challenged, with the aid of appropriate descriptive guidelines. The key issue for both camps has concerned the question of the presence or absence of a cognitive ability in young children. And investigators intent on using the C.T. to demonstrate, or
test for, the presence/absence of the rational ability in question, would agree that getting the right answer should be seen to involve some identifiable ability. Consequently there has been a consensus that the observable behavioural results obtained can be described in terms of success and failure.

Whether such shared assumptions are justified can be checked. And to do so I derive some guidelines from the work of Merleau-Ponty.

2. Likely Sorts of Descriptive Error

Both Piaget and those critics who objected to his pessimistic interpretations of the results of this and other tasks are to be firmly located within a neo-Kantian constructivist framework. The general assumption of these and other cognitive approaches to understanding what people do, and why, is that the subject is faced with the task of constructing sense out of a problematic world. Unlike in behaviourist interpretations, what the subject does is seen to be based on the meaning of the situation for him/her, the birth of meaning being an active process. This process involves the functioning of internal mediating structures, the rationally-bound operation of which imposes order on the world received by the senses. Meaning is thereby assumed to be constructed in a rationally-organized way. The constructed representation or "mental model" (Johnson-Laird, 1983) proves its adequacy - or inadequacy - through "reality-matching" with subsequent perceptual input, and through the "reality-testing" function of its behavioural consequences. The criterion of adequacy is satisfied if the posited constructed meaning enables specified problems thrown up by the rationally-ordered world to be solved and made to disappear. The smaller the conflict or discrepancy between the representation of the world and the world as it really is (i.e. independent of the subject and his/her constructing activity), the less problematic the world becomes and the more effective is the conduct associated with that construction.

The abilities associated with those posited cognitive structures must, like all abilities, come from somewhere. It is therefore appropriate to consider the question of their genesis. Piaget of course was preoccupied with the question of origin and growth of intellectual structures. With this emphasis, the above framework was given a biological underpinning as well as being extended to account for the
progressively evolving structures of scientific knowledge. This was also seen in terms of the function of constructing sense out of a problematic world, representing it closer and closer to the way it really is and hence making disappear problematic aspects associated with observed scientific phenomena (see chapter 9 for the limitations of such a characterization).

On the whole, the critics I am concerned with - broadly labelled "the optimists" - did not see the need to pursue Piaget across the frontier leading to a theory of knowledge. Also - being optimists - they might, in the extreme, hold that reasoning ability can be taken as a given and does not have to develop. This does not mean they would dispute the possibility of - or the need for - a genetic account. But to give one they would have to step into a domain that addresses processes prior to those of human psychological development. And, on the whole, they also refrained from following Piaget back over this other frontier, into biology and down the sub-human evolutionary tree.

It might be said that they had their work cut out challenging Piaget solely in the area of cognitive development where they remained preoccupied with the child - and usually the child between the ages of 0 and 7. The important point here is that these critics, tend to adopt the same general constructivist framework as Piaget. Disagreements tend to concern the question of timing of the appearance of the cognitive abilities investigated. And even when they attempted to patch up shortcomings in Piaget's object-centred approach, for example by taking more into account aspects of social cognition, we are still left with a scheme that tends to see: - (a) the world (including the social world) as it really is having an existence independent of any meaning-giving activity on the part of a subject, (b) this world as problem, (c) meaning given to the world in terms of the rational order imposed on, or constructed out of, unorganized and inherently meaningless constituents received from the external environment, and thus (d) understanding in terms of a problem-solving, intellectual accomplishment.

Piaget's indebtedness to Kant, what Rotman (1977, p.24) refers to as his "Kant connection", is evident and self acknowledged. In his approach to the acquisition and growth of knowledge, Piaget explicitly rejects empiricism for its failure to acknowledge the knower's active role and thus account for the structure of knowledge. He also rejects
rationalist philosophy. Insofar as it tends towards idealism and fails to give an account of the *genesis* of knowledge from the knower's contact with the world. He thus opted for Kant's constructivist framework, minus the apriorism, which he replaced with a biologically-based account of genesis. His approach has thus been called "biological Kantianism" (Rotman, p. 118), and Phillips (p. 22) outlines the rationale offered by Piaget for the introduction of the biological component.

(Piaget) has acknowledged, like a good Kantian, the impossibility of obtaining an absolute gauge of external reality by means of which we could appraise the adequacy of our cognitive structures, for our contact with reality is, of course mediated by these same cognitive structures. So the test of adequacy becomes a functional - and in the final analysis, a biological - one ... Piaget believes that the most important of the adaptive mechanisms which have helped the human species to flourish is the capacity to exercise intelligence.

I have already alluded to charges that Piaget's epistemology and biology are decidedly suspect. Addressing the weakness of his biological theory of progress served to put us *on guard* about the evidence that was supposed to provide support for the theory. Going in the other direction and focussing on the Kantian constructivist component will now help us to anticipate specifically *what is likely to be wrong* with the - largely undisputed - description of the evidence. Several authors (including Chomsky, 1979; Garfield, 1983; Rojciewicz, 1987) have criticized Piaget's simplistic reading of Kant and failure to understand and maintain the Kantian line. But it can be said that he follows it close enough for Merleau-Ponty's telling critique of rationalist epistemology, and its neo-Kantian offshoot in cognitive psychology, to be relevant. Some brief comments are in order.

The work of Merleau-Ponty is particularly pertinent for three reasons in addition to providing a useful critique of constructivism:-

(a) Like Piaget, and for overlapping reasons, he (1962) was motivated to find an epistemological "middle way" that avoided the difficulties of traditional empiricism and rationalism.

(b) Like Piaget, Merleau-Ponty (1962, 1965) was concerned with psychological questions, seeing them as closely linked to epistemological issues. Both were driven by the need to define the relationship between consciousness and nature. Each in their respective ways sought to bring the rational accomplishments of the intellect "down to earth" by relating them to what they presupposed. For Piaget this involved a biological explanation in terms of origins. For Merleau-
Ponty "pre-reflective" meaning was a crucial concept and referred to the direct, unmediated experience of the world that, he argued, had to be presupposed by any account of meaning-giving processes involving acts of the intellect. A further convergence lies in their respective emphases on the role of the body in the emergence of meaning (e.g. see Zaner, 1964).

(c) Both were concerned with the psychology of the child. For Piaget, given his concern with genesis, the turn to this area of investigation was seen as an obvious and legitimate step, even though he saw his child psychology as a "substitute investigation", being unable to "reconstitute the history of human thinking in pre-historic times" (Rotman, 1977, p.51). Merleau-Ponty's interest in the domain of child psychology had a very different orientation. But the relevance of his teachings (1964 a, 1964 b) is amplified in that in the course of his analyses he refers to Piaget's empirical investigations and provides a critique of Piaget's cognitive approach that coheres with his broader critique of rationalism.

Merleau-Ponty's (1962) two objections to rationalism revolve round what he refers to as:-

(1) its "prejudice in favour of the world"
(2) its "intellectualist" bias.

Briefly here, (see also Spurling's (1977) summary quoted in Appendix II (A) ) the first is a prejudice shared with empiricism. Both purport to set out fundamental frameworks for addressing the question of the nature of knowledge and the basis for justifying any claim to true knowledge. But they are each found to make a prior and illegitimate assumption about the world to be known. In the case of empiricism the prejudice in favour of the world is more obvious. The subject is viewed merely as an extension of the world on which the imprint of reality is deposited. The patterning of what is thus laid down leads to the emergence of meaning and true knowledge. The subject's role in the process is a passive one in what is viewed as an essentially automatic process. Empiricism thus tends to end up with a view of knowledge about the world that excludes the knower, and with a prejudice in favour of the objective world to be known.

On rationalist accounts, the knower is faced with the task of actively constructing meaning and knowledge of the world. The danger here is that we end up with a view of knowledge that excludes the world
to be known. Where there is an attempt to avoid idealism, it is again found that there is a presupposition about what is to be made sense of. That is, a judgement about external reality independent and outside of any constructing activity on the part of a knower has to be made before the analysis of knowledge as the product of active construction can begin. Such a judgement thus lies beyond the pale of the criteria for evaluating knowledge claims that rationalist epistemology is trying to establish. Once more, therefore, a "prejudice in favour of the world" is identified.

The "intellectualist" bias refers to what amounts to a "prejudice in favour of mediated experience" (Kestenbaum, 1974). The central point here is that any system that points to the necessary existence of active mediating processes or structures if knowledge is to emerge, also implies a prior unmediated experience of the world. That is, any consideration of the meaning-giving activity of a mediating intellect implies there is something to be given meaning to. And we cannot consider this "something" except by viewing it as something already with some meaning. Put in another way, if the world to be known is to be viewed as essentially problematic, then it is to be recognized that any posited problem-solving accomplishment of the intellect presupposes a problem-posing accomplishment of meaning.

For Merleau-Ponty acknowledging this meant undertaking an investigation of pre-intellectual (i.e. pre-reflective) knowledge or awareness. Without understanding the occasion for and ground of intellectual operations, the operations themselves cannot be understood.

In some ways Merleau-Ponty can be compared with Piaget's optimist critics in that he sees Piaget's experimental contexts as raising interesting developmental issues, while disputing the way he resolves them. Unlike both Piaget and these critics, however, his emphasis is on the difference between the world of the child and, say, that of the adult - a difference that needs in the first place to be described rather than explained away in terms of posited ability deficiencies (reasoning ability, communication ability, etc., etc.) in the child. Nevertheless, I point out that in some instances - and notably with respect to the C.T. - we may not be in a position to invoke a significant child/adult difference, even when it happens that children respond one way and adults respond differently in the "same situation". At least, we are not able to do this until the descriptive base for
interpretation is clarified. And the crucial importance of Merleau-Ponty's insights here is that I am enabled through them to anticipate two likely sources of error in the way the task and the results obtained are described.

1. What Merleau-Ponty calls the "prejudice in favour of the world" translates into the prediction that there will be, in the description of the experimental context, a prejudice in favour of what the investigator has intended to arrange for the subject to make sense of, at the expense of an accurate description of what can be accepted as actually occurring. On the one hand, mention of significant aspects of the situation are omitted or glossed over. On the other hand, factual errors are committed.

2. One possibility that arises from the above is that the C.T. episode may be wrongly described as a reasoning task. And this leads me to a consideration of the consequences of the "intellectualist" component to be found in the cognitive approaches of both Piaget and his optimist critics. The prejudice in favour of mediated experience is liable to lead to a neglect of any accomplishment of meaning that has to be presupposed when the problem-solving function of thought is under scrutiny. This allows for the possibility:

   (a) not only that the problem encountered is not the problem intended (see the first "prejudice"), but also

   (b) that there might not be a problem at all.

The consequences of the intellectualist bias in the description of what is involved on the part of the subject is liable, therefore, to lead to:

   (1) a divorcing of that cognitive act assumed to be required from what prompts it in the first place, rendering it unintelligible as an act of intelligence, and/or

   (2) an overestimation of the intellectual accomplishment called for in generating the response corresponding to the required end-result.

In the interests of parsimony, an explanation of how the right answer comes about should be in terms of the least elaborate process that needs to be invoked, so long as there is no empirical evidence to contradict such an explanation. It should usually, therefore, be the one involving the fewest hidden intermediary constructs.

Now if there is the likelihood of overestimation of what is, or need be, involved for any subject to get the right answer on a C.T., then it also becomes possible that, in terms of abilities, no more can
be said to be involved in arriving at one answer (the prescribed right answer) than is involved in another (the prescribed wrong answer). At the extreme, therefore, the "intellectualist challenge" may provide the basis for a challenge to the legitimacy of describing the results of C.Ts in terms of "success" and "failure".

The prescriptive implications for description here are thus as follows:

(a) The attempt to eliminate any prejudice in favour of the privileged perspective of the investigator involves the closing of the descriptive gap between what is arranged for the subject, and what is encountered by any subject.

(b) The attempt to minimise "intellectualism", involves describing - with the support of empirical evidence - that which any subject does in a way that makes it intelligible as human conduct, but avoids inflating the intellectual accomplishment required of and shown by the subject.

Without precautions against these descriptive biases, we can anticipate later problems. And in the following chapter, I select a key theme - the fraught issue of "false negatives" and "false positives" - that serves as an example of the sort of interpretative muddle that, I argue, is a consequence of lack of clarity at the descriptive level.

It will be seen that identifying the problem as a descriptive one in the end enables the questions that have been of interest to be answered. But perhaps more importantly, it establishes that, if the right questions had been asked in the first place, there might have been little of interest and no ensuing muddle.

NOTES

'E.g., it might hypothetically be suggested that C.Ts. crucially involve memory, and there might be good reason to suppose:-

(i) that children cannot remember as well as, say, adults, but

(ii) modified tasks present less of a memory problem than classical C.Ts.
CHAPTER 3

SUSPECT ANSWERS AND INTERPRETATIVE MUDDLE:
THE FALSE NEGATIVE – FALSE POSITIVE CONTROVERSY

A. Introduction

My aim in this chapter is to affirm that C.T. research has got itself into an interpretative muddle, and to characterize that muddle.

Should we be seeking to identify an initial symptom of such difficulties, we need look no further than the fact that the original developmental question seemingly posed by Piaget's findings is showing signs of being replaced by a different developmental question. Thus we now have the issue of why very young children frequently do better than slightly older young children. Having been told that the ability - whatever this involves - to perform successfully on C.Ts has to develop, we are now, in effect, asked to address the question of how the ability to perform consistently as a conserving failure develops (see, for example, Acredolo and Acredolo, 1980; Sinha and Carabine, 1981; Donaldson, 1982, pp. 48-49).

I will come back to the question of U-shaped developmental curves at the end of chapter 7. But at the moment I prefer not to step further into what I will show is already a sufficiently confusing developmental morass. I therefore restrict this review to the original question, and to the false negative - false positive dispute that arose out of it.

To outline the course of the dispute over interpretation: a group of investigators, more optimistic about the young child's reasoning ability than Piaget, criticized the latter's interpretation of the young child's performance error on tasks like "P". They agreed that Piaget's results were robustly replicable, that the young child tended to fail while older subjects succeed on the same task, and that therefore the evidence posed an interesting developmental question. But they sought to put forward some other (developmental) explanation of that failure than Piaget's reasoning deficit account. This project was therefore
intended to provide support for the view that the negatives Piaget obtained were false negatives.

The strategy here involved introducing a new class of C.Ts.: "modified" C.Ts, such as "O". These were intended to remove obstacles hypothesized to be obstructing successful performance on standard tasks, while leaving the logical demands made by the task intact. That is, the modified version was still to be seen as a bona fide C.T. Sure enough, an increased success rate was obtained. However, the criticism now was that a higher proportion of elicited success was no guarantee that Piaget had underestimated the young child's rational competence. It was argued, with evidence, that the positives obtained in modified versions could be false positives, the right answer here not necessarily reflecting the presence of the ability in question and thus justifying the label "pseudoconservation".

The next recommended move therefore involved the taking of additional steps to ensure that "true" negatives and positives were not conflated with "false" ones. In this way, it was hoped, the actual developmental significance of the C.T. results would be revealed, and the question of the presence/absence of reasoning competence in the young child would move towards resolution. However, I suggest that there are good reasons for scepticism about any such precautionary measures employed to make the C.T. the desired discriminating test of reasoning competence. The conclusion will be that the main line of C.T. research has brought upon itself an apparently insoluble muddle; and it has done so more through the assumptions shared and unchallenged by the disputants, than through contentious differences between them. Prior to setting about trying to resolve the wrangle about interpreting the performance failures and successes of the young child, other questions present themselves. As these concern the essential nature of a C.T. and how to describe it and the results obtained, the link with the previous chapters becomes evident.

B. A Competence vs Performance Issue

The dispute over how the results of a standard Piagetian C.T. like "P" are to be interpreted is, in the broadest terms, a dispute between
"pessimists" and "optimists" with respect to the issue of the absence or presence of reasoning ability in the young child. As such it comes under the rubric of a "competence vs performance" issue. The debate occurring within a cognitive framework, the competence in question is an information-processing ability, the pessimists (with Piaget) arguing that the non-conserving response reflects a processing (or "operational") breakdown or incompetence. On the other hand, the general feature of the optimists' alternative interpretations of the observed performance failure is the suggestion that the errors, at least in part, may reflect only an information communication breakdown. The implication here is, (1) that relevant information is not getting through, or that additional information liable to mislead is being communicated to the young child, and (2) that if it is ensured that the right information gets through, the child would be able to show that he/she can perform the required processing operation.

Now Piaget, at least within the confines of the designated cognitive analysis, can be comfortably defended on this issue because of the strict criteria he adopts. This makes it easier for him to confidently attribute operational incompetence on the basis of performance error as well as refraining from attributing competence merely on the basis of an elicited right answer. In general, he insists that the subject concerned must not only show that he/she has the ability to perform the type of processing operation in question for an attribution of competence to be made. The subject must also show that he/she knows where and when (i.e. the context in which) to use it.

More specifically, the verbal and non-verbal components, whereby the designated reasoning problem is communicated to the subject, are to be viewed as part of the context to be understood. This allows Piaget to assert that understanding the question is part and parcel of exhibiting the competence of answering it correctly. He is therefore dismissive of any argument to the effect that that child knows how to do it, but does not understand what is being asked. For Piaget, the proper understanding of what words (including the relational terms in the C.T. questions) mean is dependent on the prior attainment of the ability to co-ordinate mental operations. Thus (1) a subject with a proper understanding of "more" or "same" should not be misled by communicated information irrelevant to the correct use of these terms and to the corresponding mental operations called for. Further — and apparently really pulling the rug from under his critics, (2) he would expect a
subject lacking the competence in question not to understand the question.

Piaget prescribes strict procedural criteria to be adopted. Thus it must be ensured that the subject attends to the aspect of the array deemed significant and problematic. With respect to the C.T. this is the perceptual consequences of the transformation: hence the "watch carefully" by the experimenter prior to the pouring of the contents of B into C; and hence the rejection of subjects who are suspected to give the conserving response through an attention failure here. Finally, before the given conserving competence is attributed on the basis of correct performance: with the other criteria satisfied, the subject should be able to accompany his/her answer with adequate justification, if not to be categorised as a "pseudoconserver".

If we are concerned with the C.T. as a valid and useful index of the presence/absence of a general reasoning competence, there is already plenty that is questionable here - not least the fact that the design of a task like "P" in tandem with Piaget's strict criteria seem to be loaded in favour of the non-attrition of conserving competence (see Brainerd, 1973, below). But it was his task and they are his criteria. Thus it might seem churlish not to go along with his view of the nature of conserving ability, how to test for it, and when the attribution of the presence of the competence is to be made or withheld. On the other hand (1) his criteria might seem unduly restrictive for application to a "real world" reasoning competence, (2) we seek an explanation of the results that also covers other C.T.s., such as "O", where very different performance data are obtained. Piaget would not grant that a task like "O" can be considered a bona fide C.T. But this is unacceptable here as I have chosen to designate both as bona fide C.T.s, merely on the basis of what they can uncontentiously be agreed to have in common. After considering the views and experiments of those who have criticized Piaget for underestimating the child's reasoning ability, I will consider the views of some of their critics.

As a backdrop to this consideration of the controversy it should be borne in mind that it was Piaget's undisputed "puzzling" negative findings that provided the stimulus for all such research efforts, and the "interesting" questions he posed that most researchers see the need to come back to. That is, no matter what unsuspected early abilities are viewed to emerge from the findings of ever-more ingenious conservation experiments, we are left with - apparently - some sort of
early failure to acknowledge and account for. The performance errors are generally assumed to be bound up with some competence deficit and thus linked to the resolution of some developmental issue.

C. The Optimists' Project: Modified Conservation Tasks

The inventive modified C.T.s of optimistically-inclined researchers come in many forms. Let us consider number conservation, for example. The original classical task (Piaget, 1952) involves the subject being asked to compare two groups of counters, each containing the same number and arranged in rows one below the other in a one-to-one correspondence:

\[
\begin{array}{cccccc}
\circ & \circ & \circ & \circ & \circ & \circ \\
\bullet & \bullet & \bullet & \bullet & \bullet & \bullet \\
\end{array}
\]

When equivalence has been established, one of the rows is bunched up (the transformation) and the subject is asked the same question, i.e. whether there is more in the top row, less in the top row or the same in each row:

\[
\begin{array}{cccccc}
\circ & \circ & \circ & \circ & \circ & \circ \\
\bullet & \bullet & \bullet & \bullet & \bullet & \bullet \\
\end{array}
\]

The young child commonly says that there is more in the unbunched row.

Variations on this include one where the counters are replaced by sweets, where the numbers are not always equal, and where the subject is expected to eat, rather than point to, the row that contains more (Calhoun, 1971). In another, it is a row of toy boats that are to be compared, the "ecologically natural" transformation being effected by one of the rows of boats being allowed to float serenely apart (Miller, 1982). Yet another, and one of my favourites, involves one of the rows being placed on a strip of stretched elastic which the subject lets go to bring about the required transformation in the length of that row (Schnall et al, 1972).

The results obtained from this imaginative but not unrepresentative collection of modified tasks were varied, though generally tended to produce a higher proportion of conserving responses than the standard version. But, in passing, it can be noted that, in each of these experiments the intention of the investigator remains the same: to pose with the C.T. a certain sort of reasoning problem and see how well the subject performs - as per Piaget. However, unlike in Piaget's tasks,
each contains an attempt to disguise this aspect and subordinate the problem for the subject to some wider project - i.e. getting to eat smarties, playing a naturally evolving game with toy boats or interesting stretchy stuff. And it is to be especially noted that, in the latter two tasks/games, the transformation might not appear to the young child to have been intended by the adult co-participant in the game.

This therefore introduces that proliferating generation of C.Ts where the transformation is made to appear unplanned and incidental to the supposedly intended point of the episode. It can be seen that task "O" is a representative example of this "incidental" category, where the pretended goal or point of the game (drinks for Tommy and Susan) is made explicit.' But here I will focus on the original version of Light, Buckingham and Robbins (1979) and the one that in turn prompted it and has done most to fuel the conservation controversy over the last decade or so. This was the "Naughty Teddy" modified conservation of number task of McGarrigle and Donaldson (1975).

The reasons for picking out these two tasks were:

(1) They are easy to do, lend themselves to further modification and translation from one category of C.T. to another and can produce reasonably consistent results.

(2) The results, in terms of conservation performance of the young child, contrast sharply with Piaget's.

(3) There has been extensive and relatively enlightened subsequent discussion of their significance in the literature.

(4) There was a fair degree of experimental rigour in the form of controls, and a determined attempt to preserve the basic form of Piaget's original version in the new tasks, allowing classical and modified tasks to be classed together and compared as bona fide C.Ts.

(5) There are, nonetheless, readily identifiable, diagnostic features that serve to discriminate these modified tasks from Piaget's versions.

The last two points can be verified by referring to Chapter 1 (section D) where tasks "P" and "O" are compared and contrasted. The initial array, what is done to it and the identical before-and-after questions asked about it are the same in each case. Also, limiting the sought-for discriminating general features to those of the verbal kind emanating from the experimenter, it can be seen that the difference between the modified tasks and the classical versions of Piaget, is:
(a) a pre-transformation "watch carefully" (or something similar in the latter, but not in the former.

(b) a post-transformation "where were we?" or "what were we saying?" (or something similar) in the former, but not in the latter, the diversionary "scene-setting" preamble of the incidental version also being missing from the classical task.

Picking up on the main point in (b) it will be seen that the "where were we?" (Qa) becomes important later. It is a readily observable (e.g. see any demonstration, or teaching video, of the "Naughty Teddy" task) diagnostic feature of this sort of modified C.T. As such it is all the more surprising and disappointing that - to my knowledge - it is never acknowledged in the "Procedure" sections of the relevant research papers, let alone making it into the "Discussion" paragraphs. Take, for example, the following claim of experimental rigour by Light et al:-

Although it was obviously necessary to make the interaction in the incidental [condition] appear as natural and unscripted as possible, in fact the experimenter's wording was closely scripted, and this applied especially to the requests for judgments ...

And yet we see no mention in the procedural "script" of the additional question, "where were we?" ... which is closely linked to the crucial request for the conservation judgement. The fact that it is the sort of thing one would say after an inadvertent and irritating disruption should not lead to it being overlooked.

To design a C.T. that was more "natural" was the aim here. It was also the aim of McGarrigle and Donaldson (1975) with their "Naughty Teddy" version of the C.T. So I will now address this often-quoted experiment and the rationale behind it. The authors argued that Piaget's standard tasks underestimate the child's rational competence because of their artificial, potentially misleading nature. Dockrell, Campbell and Neilson (1980) spell out in general terms the reasoning here:

The argument ... runs as follows:- (1) Using other procedures, some children who fail the traditional conservation tasks can be shown to have the knowledge that is supposed to lead to success on these tasks ... Therefore (2) some factor or factors other than those isolated by Piaget contribute to the failure on the traditional C.T. The factor that McGarrigle and Donaldson suggested was that 'the structure of the classical C.T. seems to involve a significant deviation from normal adult-child interaction. In
the C.T. the adult's non-linguistic element (alteration of the row) is irrelevant to the interpretation of the utterance.' They argue that this 'uncoupling' leads the child to misinterpret the experimenter's post-transformational question and thus apparently fail to conserve.

The testable hypothesis that followed from this was that if such "uncoupling" was removed with the form of the C.T. kept otherwise intact, a higher incidence of successful conservation performances should be obtained from the same age-group.

Putting their rationale more concretely, McGarrigle and Donaldson argued that, while the transformation makes no difference to the way the question should be answered on its second (post-transformation) asking, the child is misled on Piaget's tasks into thinking that there has been a change. This is seen as being due to the fact that the experimenter has intentionally (even portentously, with the "watch now ..." or "watch carefully...") done something, apparently significant, to one half of the array. As Donaldson (1978, p.64) says, from Piaget's perspective "it is impossible to see why the agent of the transformation should be critical". In this case, leaving the consequences of the transformation as they were, and thus the form of the problem the same, it is legitimate to emphasize here that who or what brings about the transformation is irrelevant, and see if this makes the C.T. any easier. The opposite communicated message to "this event was intended by the adult" being "this event was not intended by the adult", this led the authors to their "accidental" design.

McGarrigle's idea was to alter events at stage two (the transformation) in such a way as to make them seem accidental - not brought about deliberately by the experimenter, thus not relevant to what he meant when he moved on to stage three and repeated this [stage one] initial question. To achieve this, McGarrigle introduced a character called 'Naughty Teddy' - a small teddy bear who was liable to emerge from his box, swoop over the experimental material, disarrange it and thus 'mess up the game'. McGarrigle found this version of the task - where the transformation was ostensibly accidental - was much more successfully dealt with than the traditional versions ...

[Donaldson, 1978, p.64]

Even if it is assumed that the subject is tricked into viewing the transformation as accidental, this may not be the best way of defining
the crucial aspect of this task. The intention is that the accidental aspect should emphasize the irrelevance of the transformation. But (a) not all that is "unintended" (normal usage) and very little of what occurs in our experience as "irrelevant" is accidental; (b) accidents can change things in a very relevant way; (c) if the "accidental" aspect (the way the transformation comes about) is irrelevant to what is asked about, and to be seen as not what was intended as the point of the episode, a more general and useful way of describing it might be: that which can be viewed as incidental in relation to that point.

With respect to this last point, that the intention is to emphasize that the transforming event is incidental, is implicitly acknowledged by Donaldson (1978) - see above quotation, and by McGarrigle and Donaldson in the original paper when they say, "it happened 'accidentally', as the byproduct of an activity directed towards a different goal". Thus it seems that not only can the arranged crucial feature of the transforming event in the "Naughty Teddy" C.T. be described as "incidental" instead of "accidental"; it may be better described in this way. But the final, probably decisive, argument against the use of the term "accidental" here is that it is implausible to assume that the child subject must view the teddy's actions as a completely unintended accident given that (1) teddy is presented as being intent on sabotage, and (2) the toy bear is quite obviously manipulated by the experimenter. Even if the crucial details (from the investigator's point of view) of the consequences of teddy's cavortings might be seen as an unintended accident, there is ample room for doubt about the authors' description of the significant features of their design, and thus for doubt about their particular explanation of why the representative young child fails on Piaget's task and succeeds on the modified one.

Since the child's interpretation of the tester's intentions is central to our concerns, it is unfortunate that the 'naughty teddy' device involves some ambiguities in this respect. While children in [our replication of] the experiment were willing to 'play the game' by attributing agency to the teddy bear, they clearly also knew that the tester was responsible for both introducing and manipulating it. The term 'accidental' is perhaps a misnomer because the teddy bear was supposedly trying to spoil the game. But the extent to which the child holds separate the intentions of the tester and those of the teddy must remain in doubt.

[Light et al, 1979]
A suggested modification of the already modified C.T. here might be to arrange that the teddy is operated by a supposedly independent third person, apparently in opposition to the original intentions and present wishes of the organiser. Indeed this is how the experiment is sometimes run now, though it should be said that the results are much the same as in the original Naughty Teddy version. The point here is that, with the same aim, and as a consequence of the noted "ambiguities" of McGarrigle and Donaldson's "accidental" context, Light et al were led to design their explicitly "incidental" modified version of the C.T. It is from this prototype that my "representative" task "O" is derived. Here:—

the basic requirements of the conservation task are preserved, but they are placed in the context of a competitive game between two children. [Unlike in Piaget's standard C.Ts] the test of conservation is no longer the focus of the tester/child interaction, but is made incidental to the proceedings involved in setting up the game.

[my emphasis]

The point of arguing that McGarrigle and Donaldson's task is better described as also an "incidental", rather than an "accidental", version is to establish that here too the transforming event, and thus the repeated question it occasions (constituting the "test of conservation"), is also "no longer the focus of the tester/child interaction". With respect to the "conservation question", this might not be an outcome that the authors wanted, but it is likely to be the outcome they get. And we will see that this exposes them to the failure-through-inattention challenge of their "false positive" critics (see below).

The original version of Light et al's test was a test of conservation of discontinuous quantity and involved two subjects who were each given identical beakers with the same amount of pasta shells in them. They were told that they were going to play a game with them, but that, to be fair, they had to make sure that they each started with the same amount. After equivalence is established and before the game gets started, the experimenter "noticed" with consternation that one of the glasses had a potentially dangerous chipped rim. E searches around for an alternative container, "happening" to come across a larger one into which he transfers the contents of the chipped beaker. [At this point the "where were we ... Ah yes ..." occurs.] The experimenter then
asks Q2. When a judgement was elicited, the game was allowed to continue to its conclusion, though - from the point of view of the experimenter - the episode, as a C.T., has been and can be terminated when the answer has been elicited.4

In terms of the results, it was found that "the difference in outcome between the two conditions [the modified task and the classical Piagetian control] is quite as dramatic as that reported by McGarrigle and Donaldson "and the results of both sorts of experiment" support the view that successful conservation judgements are more likely to be made by young children when the transformation of materials in the task is made to seem ... incidental to the main purpose of the interaction" [my emphasis].

One conclusion here then is that both these modified versions can be considered to be of the same (incidental) type, and the points that can be made about one version generally apply to the other. This is important when, for example, I come to use task "0" to make critical comments about the "optimists'" approach in general.

To summarise:-
(1) In any bona fide C.T. the need to ask the conservation test question (Q2) arises because of the occurrence of the transformation.
(2) In both the "naughty teddy" and the "chipped glass" task it is arranged that the transformation event occurs as incidental to the intended main goal, even if in the case of the former the (now successfully performing) subject may be left without a clear idea of what the "main goal" was.
(3) In both these modified tasks we can see that the need to ask the "conservation question" is also supposed to be perceived as arising incidentally, just as unanticipated by the adult as the troublesome interruption was.

The provisional "optimistic" conclusion from the increased rate of conserving responses, on incidental tasks in place of standard versions, is that, on the latter type, at least some of the erroneous non-conserving performances occur in spite of the presence of the competence ostensibly being tested for. Success on the former type is seen as being more likely because they are more sensitive tests of that competence, being less artificial and less potentially deceptive as "interactional settings". This is why what were seen by Piaget as
errors resulting from rational, information-processing difficulties are now seen as errors resulting from a breakdown in the communication of information - because "the child and experimenter are on very different wavelengths" (Bryant, 1982 a, p.4).

The modified tasks here, therefore, can be seen to constitute an attempt to (a) facilitate the getting across of information that was not getting across (e.g. that it is irrelevant who or what brings about the transformation) and/or (b) block misleading information that was getting through (e.g. that the experimenter intended the transformation and therefore might justifiably be viewed to intend that it should make a difference to how the question is to be answered on the second time of asking).

With their evidence McGarrigle and Donaldson are confident enough to make the strong assertion that their results "give clear indication that traditional procedures for assessing conservation seriously underestimate the child's knowledge". So far, we might suppose that there is nothing wrong with Piaget's concept of conservation, nor with the assumption that the way to test for it is with some sort of conservation task. Nor is there a complaint about his findings with young children, description of them as performance "failures" and assumption that they pose some sort of interesting and significant developmental question. But they claim to have found a better test of the competence than Piaget's, which indicates that the child's performance failure on the latter may be cognitively far more impressive than Piaget would admit. Adhering to the framework of Piaget's stages of progressive development, they conclude that the developmental significance of the C.T. results may be that they show that

the achievements of the concrete operational stage are as much a reflection of the child's increasing independence from features of the interactional setting as they are evidence of the development of a logical competence.

The evidence suggests to them therefore that at least a proportion of Piaget's "negatives" are false negatives, and hide the presence of the reasoning competence in question.
D. Criticisms of the Optimists

There are several reasons to be immediately sceptical about the deployment of such tasks, the stated rationale for doing so, and the conclusions arrived at on the basis of the results obtained; e.g.:-

(1) Whether the C.T. used is a standard of modified version, the information that the experimenter intended the transformation is correct information; and whether it is potentially misleading or not, by communicating that E did not intend the transformation to occur, he/she can be seen to be facilitating false information. Without the transformation the episode has no point, and there is no occasion to pose the test question.

(2) We might ask: why, in any C.T., should the transformation be taken to be irrelevant? The occurrence of the transformation and the question that follows as a consequence of it might in normal circumstances be a good clue - and for anyone - that the answer to the second question (Q2) is not the same as the answer to the first (see Rose and Blank, 1974, below).

(3) On the other hand, given that the transformation is and is intended to be irrelevant in any C.T., we might start to become suspicious about the insights into understanding that can emerge from such a context. Prior to addressing the question of why the subject does what he/she does in a C.T., we may need to attend further to what actually happens in a C.T.

The latter two points will assume increasingly greater importance in later chapters. The general point, closely bound up with (1) above, is that: to work in the way intended modified C.T.s depend on the subject being misled in some way or another. As such we can suspect that they too are artificial interactional settings, assuming that normal dyadic social contexts are based on the attempt to convey and share meaning, rather than obscure it.

Light et al, even as they complete the account of their empirical investigation and its significance, show that they are aware of this problem in relation to both their own study and that of McGarrigle and Donaldson, which they correctly consider together.
The assertion of McGarrigle and Donaldson that their results show that 'traditional procedures ... seriously underestimate the child's knowledge' ... seems a curious conclusion, apparently based on the assumption that because [their] condition generated a higher proportion of conserving judgements it must be a more sensitive index of the child's underlying ability.

[my emphasis]

The point to remember here is that, on any C.T., while we may decide to set out criteria "low" enough to ensure that an observed performance error does not necessarily mean that we can say that the subject involved lacks the competence in question, "the usual conclusion - that the right answer in a conservation task means that the child does understand invariance - is also a risky one" (Bryant, 1982 a, p. 4). In other words elicited "positives", if and when they occur, may be "false positives". Bryant goes on, still talking about C.Ts in general:

The trouble is that a child may give the right answer without even noticing the transformation and its results. He may be dimly aware that something has been done to one of the quantities without notifying what the perceptual changes are. In this case the task is not a proper test of invariance.

[my emphasis]

Light et al, arguing against a similar background, go on to spell out why the occurrence of more elicited "positives" does not necessarily indicate a better test. There may be grounds for suspecting that the right answers may occur for the "wrong" reason, and represent what Piaget called "pseudoconservations". The procedures adopted here may not only, as intended, divert the child subject from attaching undue significance to the act and/or agent of transformation, but may also divert his/her attention from the perceptual consequences of the transforming act, such that the subject does not even consider the possibility that something might have changed. The subject may not properly (i.e. in the way intended by E) address the problem posed by the question on the second time of asking. In this case, more right answers would be expected, irrespective of whether the rational competence to conserve is present or absent.

Failures in the standard condition [might be] seen as 'false negatives', non-conservation arising from the implicit message: 'this transformation is important', contained in the tester's action. Should we not then regard successes in [these
At this point, and for these reasons relating to interpretative difficulties, we can discern that the conservation enterprise becomes healthily riddled with enlightened self-doubt. Thus Light et al say:

We seem thus to be further from, rather than nearer, an unbiased assessment of the child's logical abilities.

And Donaldson (1982) says:

The conclusion to which all this tends is that the conservation test is not a very good tool for studying specific conceptual developments. If we want to know what children understand about number - or weight, or volume, or whatever - there are better ways of finding out.

[p. 48, her emphasis]

She goes on: "This is not to say that the conservation test should be discarded". What makes these and other authors stop short of recommending the abandoning of the enterprise? It is the assumption that, while investigations of the C.T. may not be able to yield a straightforward answer to the original, key question of the presence/absence of a reasoning ability in the young child, the results still pose a potentially soluble question of importance to developmental psychology. Thus:

As McGarrigle and Donaldson rightly point out, the findings of these [accidental/incidental] experiments may lead to a new conception of some of the changes which for Piaget mark the transition to operational thinking.

[Light et al]

However, other authors have been less willing to give ground on the central question seen to be posed by Piaget's original findings.
E. Precautionary Measures (and further comments on modified C.Ts)

Can the conservation enterprise, its discriminatory potential as originally conceived, be shored up? If so it will involve the taking of precautionary steps to ensure that "true" negatives and positives are able to be confidently distinguished from, respectively, "false negatives" and "false positives". My focus in what follows is especially on the precautionary measures that have been suggested to discriminate "positives" from "false positives" - and with particular reference to modified tasks, since this is where positives tend to occur.

One strategy, given the "attentional problem", relates to precautions that might be taken during the C.T. to ensure that the subject concerned attends to the "right" things in the "right" way. Taking this line involves addressing further evidence that can be obtained from the above-mentioned tasks as to whether/how subjects attend to the transformation and what they consequently make of the second asking of the question, occasioned by the transformation.

The other strategy also refers back to Piaget's strict criteria, and concerns precautions that may be taken after testing - that is, by eliciting and examining justifications of the subject's responses to the second conservation question. Superficially at least, each strategy may augment the other, given that what a subject has attended to is liable to figure in an elicited justification. However, the issue of how far what appears in a child's justification gives an accurate indication of what he/she made of the episode as it was happening, is a focus of one of the hotter contemporary disputes over the interpretation of C.T. results (see below).

Strategy (1): getting the child to attend properly

With the naughty teddy task, and the problem of false positives, particularly in mind, Bryant says (1982 a):-

The conservation task ... is only a proper test [of the rational competence in question] if the child notices the perceptual change as clearly as he noticed the pre-transformational display, and still is able to say that the two quantities are the same. It is strange but we have no guarantee that this is
It would be very easy to include a check that children have attended to the display after the transformation, and the absence of a simple control of this sort discredits the whole conservation enterprise. This is a soluble problem. Simply do the teddy bear transformation and [other] ingenious transformations as well, together with a measure of the children's attention to the results of the perceptual transformation ... In fact one of the joys of working with Piaget's theory is that so many of his questions are answerable and seem very close to a solution. That they are is largely due to Piaget, partly because he raised so many provocative and important issues and partly through his ingenuity in thinking of ways of looking at these issues. [pp.4-5]

I find this a particularly illuminating passage if only because virtually every contained statement is - or implies something that is challengeable. I include here the closing, enthusiastic, general assertions - but make an exception of the one that implies that the whole conservation enterprise stands discredited.

Some of the points I will come back to later. Here I will merely say that Bryant starts out in this passage seeming to assume what must surely remain in doubt (until proved otherwise) from what he has said earlier (see section D). He was commenting on the false negative - false positive problem in all existing versions of the C.T. At this point then, what is in doubt is whether, irrespective of possible precautions, any bona fide C.T. can be a "proper test" of some reasoning competence. If it turns out, say, that there is never enough information made available to the subject, then whatever the subject attends to will not make it into a proper reasoning task. In this case it would be not at all "strange that we have no guarantee" that some version or other of the C.T. is a "proper test".

Our suspicions can only be enhanced when we note that getting the required "guarantee", according to Bryant, would involve making the subject attend to something that we are insisting, given the nature of any C.T., is irrelevant. Nothing that the E does is to make a difference to how the question is to be answered. This can only add yet another artificial component to what is already an artificial social context - whether it be because of the misleading "watch carefully" on tasks like "P", or through pretend "accidents" and "incidents" (see "C").
Finally, it may not be "the absence of a simple control", but something else that "discredits the whole conservation enterprise." Identifying exactly what this is, is my central concern.

I will not prematurely dwell on these points. Being concerned here with precautionary measures, we need to look closer at Bryant's notion of a "simple control". Specifically I will focus on his statement that "it would be very easy to include a check that children have attended to display after the transformation". Bovet et al (1981), Neilson et al (1983a) and Donaldson (1983) each would dispute that the attention-to-the-final-array problem can be resolved by a simple check.

Bovet et al, working on the Light et al task, argue that many of the correct conserving responses occur here because the children are so keen to get on with the game that, while seeing the results of the "incidental" transformation, they choose not to bother about them. Neilson et al likewise agree that the increased likelihood of success on the accidental/incidental version is not simply a function of the child ignoring the post-transformational state of the array" but say that:

The possibility remains that while the child was aware of the post-transformational state of the array, the fact that it resulted from the actions of Teddy and was portrayed as a disruption to the main game being played by the experimenter led the child to dismiss it as a 'mess' which ought to be sorted in order that the experimenter and the child could continue with their game. This would explain the frequent attempts of children in the [Naughty Teddy] condition to revert the array back to its previous state following Teddy's actions. Such an interpretation of the task [sic] would lead the child to disregard the post-transformational state of the array and to have based his response to the post-transformational question on the array's pre-transformational state since this was the original referent before Teddy disrupted things.

[their emphasis]

There are two points to be made here:

(a) Given that there is no necessary reason why the "attentional problem" should not apply to standard tasks, if conserving responses on the modified tasks are suspected to be "false positives" (e.g. for the above reason), then the correct responses on standard tasks (when they occur) might also be suspected to be "false positives". Indeed Piaget does offer an attentional account when he invokes the phenomenon of "pseudoconservation" to explain non-predicted right answers by children on his tasks.
(b) Wherever it arises, the C.T. "attentional problem" does not seem to be a simple one. More important than the question of whether or not the subject attends to the transformation and the question that follows from it, may be the issue: allowing that the subject does attend, how does he/she attend to the post-transformational array, and what way does this lead him/her to take the post-transformational asking of the question?

Any attentional explanation of conduct should consider both the quantitative and qualitative dimensions of attention. We should be dubious about categorising attention as an all-or-nothing affair, as Bryant might be taken in the above to be suggesting. And we will be missing the point about the function of attentional processes if we do not take into account that we attend to what is important and relevant to us, and that there are always different possible ways of attending in a "given" situation. This is so however rigorously we go about circumscribing the objective properties of the "given" experimental context.

Donaldson (1982, p.48) accepts that there are difficulties in taking full precautionary measures against false positives. But she (1983) is able to defend the work of McGarrigle and Donaldson against Neilsons et al's claim (supported by the evidence of justifications - see below) that what they are getting here is "pseudoconservations" of some sort. In doing so she sees it helpful to make a further distinction in relation to the "attention problem". Thus she says that their "argument ... is flawed, for it equates attention with conscious attention" (her emphasis). More will be said about Donaldson's defense on the false positive charge below.

However the dispute about false positives might be resolved, it must be concluded that it is not going to be by "simply" including "a measure of children's attention to the results of the perceptual transformation", as Bryant suggests. But we may ask what sort of "measure of children's attention" he might have had in mind.

From what has been said above, "simply" checking if the child was watching the array would obviously not be enough. We need also to check that the child attends to the array in a certain desired way. But:-

(1) the sort of attention we require of the child should not only make sense in terms of the arranged context encountered by the child, but also should not be the kind of attention that would increase the
likelihood of anyone (including an adult) giving the wrong answer (see Chapter 7).

(2) We need some empirical criterion - apart from task performance - to judge what way the child is attending in any C.T. situation.

In relation to the latter point, there is a problem as to what will constitute evidence. This would only be slightly mitigated if the subject spontaneously gave a full and reliable running commentary on what he/she is consciously experiencing and attending to during the episode. Apart from the fact that this does not happen, this would still leave the workings of a "cognitive unconscious" open to doubt. So, going beyond the subject's observable conduct during the episode, even though there is a lot more to be said about this (see Chapters 4 to 7), there are two alternatives:

(a) We can turn right away from the subject's conduct, looking instead at what the investigator does. Thus, to get at the essential nature of the (whole) situation we place a subject in when we run a "conservation task", and at what exactly we do ask him/her to do and attend to, we examine in more detail the design of the task context.

(b) We can ask the subject to give a retrospective "commentary" relevant to our purpose of understanding how he/she attended to it. That is, we may attempt to elicit a justification or explanation of his/her right or wrong answer.

The former I will show in later chapters is the more fruitful approach. With respect to the latter alternative, it may turn out to be unjustifiable to expect anyone to give a totally adequate rational justification of a correct answer on any C.T. This in turn raises the question of just how unjustifiable is the non-conserving response - e.g. is the wrong answer any less justifiable than the right one? But the more immediate concern is the question of the reliability of the young child's justifications in general.

A preliminary general point to be made here is that the aim must be to investigate what goes on within the context of a bona fide C.T. A context looked back upon - for anyone, and even assuming no memory problems - can never completely coincide with that context as "lived". This is not merely because only a small proportion of experienced events are subjected to rationalisation. But the disparity is likely to be even greater if, in normal circumstances, it would not even occur to the subject to look back on and justify what the investigator here requires him/her to justify. The problems are merely compounded if the response
that the subject is required to justify is one that was, itself and in
the first place, a judgement that may not have occurred - verbalised or
unverbalised - spontaneously (see chapter 8). If it was only elicited
at the request of the organiser, in further asking for a justification
we are likely to end up with a rationalisation of what was already a
rationalisation. There is therefore a two-fold risk of distortion10 in
relation to the investigator's view of what the subject actually
attended to, and understood at the time.

Such points will re-surface later. However, let us look at the
role justifications have been given in supporting interpretations of
children's performance on C.Ts.

Strategy (2): getting the child to justify his/her responses

To provide continuity with the above, the dominant question that I
will be concerned with is as follows:- given that the other criteria can
be taken to be satisfied (correct judgement elicited; subject apparently
"attends properly" to the final array), can a justification elicited
from the "conserving" child perform a decisive function as a criterion
for attributing the competence in question and eliminating false
positives?

Piaget employed all three criteria in conjunction with the C.T. and
not only sought evidence as to the presence/absence of an ability that
corresponded to a certain cognitive structure, but sought with his
procedure, evidence as to the nature of the child's cognitive structure.

In relation to the prior presence/absence question, Brainerd (1973)
is unequivocal about the role of explanations or justifications. When
the presence of reasoning ability is suggested by a correct judgement
("same") during a C.T., an elicited "inadequate" justification of this
judgement provides no basis on which to withdraw the attribution of
competence. Following an analysis of Piaget's framework for
investigating cognitive structures, he comes to two conclusions:-

(a) [Piaget's] theory not only fails to justify an
explanation criterion, but it makes such a criterion
seem highly inappropriate to the task of determining
the presence of cognitive structures. (b) The
judgement criterion [on its own] seems well suited
to the task of determining the presence of cognitive
structures ... If one makes adequate explanations a
necessary precondition for inferring the presence of structure ... then one has built in a source of Type II error and virtually insured inflated age norms for the acquisition of specific structures.

Brainerd does acknowledge that, in relying on judgements alone, we "run the usual risk of extraneous Type I [false positives] and Type II [false negatives] errors which crop up in any measuring situation." Piaget cannot be blamed for coming across a measuring situation design that threw up an apparently interesting and desired result, whatever we are to make of the negative judgements considered on their own. But he can be blamed for overdoing it with additional restrictive and loaded criteria. And his error is all the more serious in so far as it does not seem to tally with his theory, and methodology elsewhere.

What is verbalised in a justification is presumably what is available to conscious experience. Yet on his theory - applying to a "cognitive un-conscious" - there is no reason why the workings and achievements of the intellect should be made available to conscious experience. This might be especially the case as we are considering the young child and therefore might allow for the possibility - see Donaldson (1983, above) - that making what is implicit and unconscious consciously explicit itself involves an ability that must independently develop. Further, linguistic structures are, on Piaget's theory, dependent on the prior formation of cognitive structures. Thus, in testing for the latter, it would seem that minimal reliance should be placed on the complex verbal ability required in offering a justification. Indeed, elsewhere, Piaget can be seen to be cautious about placing undue reliance on the spontaneous verbalisations of the child. And, notwithstanding his early adoption of the "clinical method", we might suppose that he would have been sophisticated enough therefore to realise that attaching significance to unsolicited verbalisation (justifications) might be fraught with problems. This is especially so when, as is the case here, the original judgement to be justified itself has to be solicited (see above), rather than offered spontaneously.

Brainerd (1973) concludes that:

[The view] that the explanation criterion is more consistent with the presuppositions of Piagetian theory than the judgement criterion ... does not stand up under close scrutiny. Paradoxically ... it would seem that the explanation criterion is more in line with language-created theories of Piaget's
chief opponents (notably, Bruner) than with Piaget's own theory.

But while asserting that justifications are not to be used for inferring the absolute presence/absence of cognitive structures, he goes on to suggest that they can fulfil a function:-

Explanations can supplement judgements in such a way that one is provided with insights into the nature of the structure or structures under consideration.

However, should we be in doubt for independent reasons what a correct (or incorrect) answer on a C.T. can even suggest in relation to the presence/absence of a cognitive ability or structure, then this "supplementary" function of justification is also undermined. And, following from this point, it will be seen that there are better reasons than Brainerd's for being circumspect about what the evidence of justifications can tell us on the question of presence/absence of a rational competence here.

McGarrigle and Donaldson will be faulted - along with just about everybody else - for an oversight in relation to this last point. But given the soundness of Brainerd's argument within his framework of assumptions, they cannot be faulted for deciding to eschew the evidence from justifications. In addition to quoting Brainerd's point that the justification criterion is too strict, they point out:-

The attempt to elicit justifications would have involved the child and the experimenter in further complex interaction, the characteristics of which could have influenced the child's subsequent behaviour in a number of ways.

But Neilson et al (1983a), arguing with what they see as empirical support, believe that "the most crucial" methodological criticism of the work of McGarrigle and Donaldson is that they "did not request the children to justify their arguments in conservation tasks." Their claim is that, if these authors had done so, they would have had to acknowledge the likelihood that the majority of the extra successes obtained in the Naughty Teddy condition represented false positives. With what seems misplaced confidence, they take the "supplementary role" Brainerd assigns to justifications (see above) to be to "offer a means of checking the validity of arguments given to explain the child's success," and particularly applicable to modified task procedures. 1:2

In a forerunner to the Neilson et al (1983a) paper, Dockrell et al (1980) are more cautious. They acknowledge that their method of "asking
for justification ... further complicates the interaction between the experimenter and the child." Also they accept that the phenomenon of "uncoupling" invoked by McGarrigle and Donaldson (see above) may contribute to the child's difficulty with Piaget's standard tasks. However, they object to the strong conclusion that these authors arrived at supposedly on the basis of their results: i.e. "that the achievements of the concrete operational stage are as much a reflection of the child's increasing independence from features of the interactional setting as they are evidence of the development of a logical competence". On the basis of justifications they find that there is still reason to believe that Piaget's negatives may be true negatives and that the extra positives obtained in modified C.Ts represent false positives. They conclude:--

What is required at this point is a clarification of the interaction between the child's use of behavioural strategies and his complete or partial understanding of the logical requirements of the conservation task.

Brushing over the possibility that what is required before this is a re-examination by all concerned of just what are "the logical requirements of the conservation task", Neilson et al (1983a) proceed with this project of "clarification". They conclude from the evidence of inadequate justifications, in tandem with the evidence about the child's "behavioural strategies", that a large proportion of "conservation successes" on the Naughty Teddy task result from a failure in understanding what is involved. They thus represent false positives, and do not refute Piaget's "pessimistic" interpretation of the results of his standard versions of the C.T.

Their hypothesis was that "in passing judgement on the [Naughty Teddy] condition the children were only considering the pre-transformational state of the array". The "logical requirements of the conservation task" on the other hand should be ensured to demand that the subject also considers the final array and, from the information available, works out that there must be the same in each container in spite of the perceptual disparity. In passing, Dockrell et al had tentatively put forward a similar sounding hypothesis, again stressing that success on the Naughty Teddy version resulted from an inadequate and erroneous understanding of the context:-
Teddy may have acted as a distracting agent leading the child to ignore the post-transformational state of the array and to answer the experimenter's post-transformational question (correctly) by mere repetition of his earlier response.

The point is that we have the conclusion: (1) that a non-cognitive explanation can be offered for the child's success on the modified task; and thus, given the "logical requirements" actually present, (2) a mistake in understanding is involved. The two sources of evidence are (1) subsequent justifications, and (2) accompanying behaviour.

Donaldson (1983) argues that, even if we decide to legitimise justifications, the evidence offered by Neilson et al does not do the work required of it. Allowing that justifications could discriminate between true and false positives, they fail to show, as would be required, that false positives are any more likely to occur on the modified, Naughty Teddy task than on the standard C.T.

If false positives are to account for the [modified/standard] difference then ... we ought to find a smaller proportion of the [modified] than of the [standard] correct responses adequately justified. It will not be enough to show that the [modified] condition yields many correct but inadequately justified responses - that is many false positives." (her emphasis)

Neilson et al admit that "no [such] significant difference is ... observed in the proportion of justifications". The alternatives we are faced with then are (1) that justifications may after all be to some degree reliable, but show "that false positives abound in both conditions" (Donaldson, 1983), or (2) that the evidence from justifications is unreliable and McGarrigle and Donaldson's claim that their results show that the young child has the logical ability in question stands unrefuted. Donaldson prefers the latter, while I prefer a third alternative. That is, I would remain dubious about the evidence from justifications, and would suspect (and seek independent evidence for the view) that there is likely to be something "false" about correct answers whenever they occur. Whether it be on classical or modified C.Ts., such a view is sustainable because of the shared odd features. (see later).

But for the moment we might allow that Neilson et al's "false positive" challenge to McGarrigle and Donaldson's results can be seen
off. However they are obtained, the claim that they suggest that young children can do the reasoning required is not refuted by the justification evidence. Donaldson now turns the argument back to what can be considered the independent "false negative" charge that, given the way Piaget's negative results are obtained, the conclusion on the basis of them that the child lacks the reasoning competence in question is unwarranted.

McGarrigle and Donaldson's original hypothesis addresses the way the results from the standard task, viewed as an interactional setting, are obtained, and specifically the fact "that the experimenter's actions in the classical situation [may] lead some children to an interpretation of the question that does not accord with the one the experimenter intended, [and thus lead them] to answer the wrong question" (Donaldson, 1983). Donaldson now proceeds to argue that Neilson et al's study of justification does not yield any evidence that is even relevant to the original hypothesis. The latter state that "whatever the reason for the children's performance in [the standard] condition ... it is unlikely that it is related to the interactional setting of the task in the fashion suggested by McGarrigle and Donaldson." The reason given for this assertion is that only very few of the children who fail on the standard task - at least those of nursery school age - justify their wrong responses by referring to the experimenter's actions. Neilson et al infer from this that these children were not attending to the transformation and that therefore McGarrigle and Donaldson must be wrong in saying that it is the way that the transformation is arranged to come about that misleads the child, rather than the perceptually "seductive" consequences alone, as Piaget suggests.

It is at this point that Donaldson invokes the "attention/conscious attention" distinction (see above). She says:-

[Neilson et al's argument] assumes that young children are aware of the factors which influence their interpretation of an utterance. Yet awareness of one's own mental functioning is known to develop over the period we are considering.

Donaldson can be criticized here for introducing what might be yet another developmental red-herring into the debate about the significance of C.T. results - "literature [that] is murky enough as it is" (Brainerd, 1974). But her point stands that nothing that Neilson et al produce in the way of evidence from children's justification confronts
McGarrigle and Donaldson's claim that Piaget's negatives may be false negatives. Nor does it refute their further claim that their own positives - from the modified condition - are true positives.

In the end, however we are to decide on these issues, there is now sufficient reason to suppose that the evidence from justifications cannot decide them for us. To establish consensus on this point we might offer the by-now familiar point of view, but this time quoting Neilson et al (1983b), themselves. They found that the results they obtained, albeit from a different experiment:

indicated that caution must be exercised in deducing the type of strategy the children used to generate their judgements from the justifications they offered for those judgements.

This leaves Neilson et al reliant on the evidence as to children's "behavioural strategies" alone. Apparently the only evidence of their own that they offer is the above-cited (and replicated in my own empirical investigations) "frequent attempts of children in the (modified) condition to revert the array back to its original state following Teddy's actions".

Apart from disrupting McGarrigle and Donaldson's procedure, and making it difficult or impossible to test their hypothesis on the basis of such subjects (they retrospectively recommended the rejection of the results from these subjects), this observed conduct in itself does not contradict their hypothesis. Indeed it might have been a predicted by-product of their "accidental" design, should the child be successfully tricked into thinking the alteration to the array really was an accident. More important, it is impossible to infer from it that a mistake in understanding has occurred (unless we say that the child shows by it that he/she mistakenly understood that the alteration of the array was an accident!). When an accident is seen to occur one natural and logical response is to set things right, and as they were.

This leaves us with the conclusion that the authors draw from examining the way the child attends, justifies and behaves. This is that the child, wrongly, "only considers the pre-transformational state of the array" (Neilson et al, 1983a), or wrongly answers by a "mere repetition of his earlier response" (Dockrell et al, 1980). On the one hand, as will be seen, there is the basis of a real insight here. On the other, if this is how the child succeeds on modified tasks, we might
ask, on what grounds the authors decide that a mistake has been made? Given that the experimenter asks a question, arranges that something irrelevant occurs (along with all the other irrelevant things that will be going on) and repeats the question, what could be more logical than "merely" repeating the earlier answer. It may indeed turn out that "only" considering the pre-transformational state of the array is already doing more than is required (see chapter 6).

But now the point to be noted is that there is no obvious reason why this should not also apply to standard, Piagetian versions of the C.T. Thus rather than their conclusion suggesting that an error has been made by the representative young child in getting the modified C.T. right, it may provide an indication of a better, more parsimonious account of how anyone might be viewed to get any C.T. right.

Further, if conserving responses are able to be interpreted along these lines, it will require a reappraisal of non-conserving responses. Certainly, at this point, we may be starting to move towards a resolution of the "false negative - false positive" controversy, but in doing so we may have to turn the whole debate on its head, questioning assumptions shared by all the disputants.

A consideration of the implications of such points must attend on further evidence as to the nature of the C.T. and what actually happens when subjects conserve or fail to conserve. But it can be seen that we are drawn back to the descriptive problem raised in the last chapter. If it is still not clear how we are usefully to describe the sort of situation that the C.T. comprises, and not clear exactly what we mean by "failure" and "success", "negatives" and "positives", we cannot yet be in a position to pronounce one way or the other on the issue of "false negatives" and "false positives".

Until we have reassessed and spelled out the key features of the context of the C.T. we cannot be confident that we properly understand the subject's performance. And until we understand this we cannot know to what extent a correct judgement is rationally justifiable, and to what extent an incorrect one is not. In turn, until we knew this, we could not - even in principle, be able to evaluate elicited justifications of judgements.
F. Interim Summary and Conclusions

I have considered just a few examples drawn from the multitude of versions of the C.T. resulting from over three decades of research on the topic. And I have examined just some of the different ways of trying to interpret C.T. results. But I have said enough to establish, or remind those who knew it already, that we have on our hands here a muddle of epic proportions. By the selective line I have taken, I have provided a perspective on some important features of the muddle – one that will be put to use below.

Piaget can certainly be criticized for using a task design and methodological criteria that loaded things in favour of a "pessimistic" interpretation of the young child's performance, and thus in favour of his theory. But the attempts to interpret his results more "optimistically", and involving the proliferation of "modified" designs, merely fuelled the debate and posed additional problems – notably the problem of "false positives". In turn the subsequent attempts to take "precautionary measures" in order to allow the discrimination of "true" positives from "false" ones, merely highlighted the difficulty and perhaps impossibility of their enterprise. Thus each new wave of C.T. research seems only to plunge us further into the difficulties, throwing up new developmental issues without resolving what is generally seen as the original problematic one: Why does the young child fail on a task such as "P", while older subjects succeed?

Piaget, of course, did not see this as problematic and assumed it was because the child could not do the reasoning. The "optimists", having generally sound, independent reasons to suppose that the child could, said it was, in the first place, because it was not fair on the child. The way to control for false negatives was therefore to attempt to remove the misleading element in standard versions. But it was seen that, to do what they intended, these investigators (eg. McGarrigle and Donaldson; Light et al) actually had to incorporate an additional deceptive element. The experimenter in any bona fide C.T. intends to bring about a transformation of the appearance of the array, although neither how it comes about nor those perceptual consequences should alter the way the question is to be answered the second time it is asked. The "optimistic" experimenter intends to help the subject with the first bit alone. He/she thus pretends that the transformation was
unintended and incidental to the game they are playing. If the deception is successful the child, as intended, will believe that the occurrence of the transformation was irrelevant and ignore or disregard how it came about. The "danger" now is that the subject will not keep separate the act of transformation and its consequences, and thus will disregard the latter also. Given the nature of the C.T., this happens to be a good strategy for getting the correct and approved answer. But by employing it the subject by-passes the need to work out that, despite the intended appearance to the contrary, there must necessarily still be the same amount in each container. A positive obtained in this way is therefore called a "false positive".

McGarrigle and Donaldson talk of the "incongruity" of the experimenter's non-verbal (transformation) and verbal (questions) behaviour on the standard task. The non-verbal message: "the transformation is relevant" leads to the misinterpretation of the second question, and to the non-conserving response. The Naughty Teddy modified C.T. represents an attempt to bring about a "coupling", as for a normal interaction, by lessening the opportunity of the non-verbal element to give the wrong message. The false positive controversy comes about because of the possibility that such an "accidental/incidental" procedure results in no message for the subject, i.e. none that stands out from the other, equally irrelevant, background events. In this case we might state the charge against McGarrigle and Donaldson as follows:— Rather than achieving a better coupling with the modified C.T., they achieve their results through a complete uncoupling. The transformation event is sufficiently detached or "uncoupled" from the point of the game for the subject to disregard it — along with everything associated with it, including its effect on the array.

It seems that while Piaget may trick young children into giving the wrong answer, the optimists' "incidental" modifications may trick the child into giving the right answer to this question. Turning to the latter problem, that of false positives, the two main ways of controlling for false positives were seen to be (1) to make sure that the subject does attend "properly", and (2) to make sure that right answers, especially those obtained through the optimists' deceptions, were accompanied by adequate justifications. But the difficulty now is that, given the nature of the task, it is not clear what sort of "proper" attention is required and what an adequate" justification should consist of (see chapter 8).
Some of the difficulties, and a great deal of the literature, have arisen from procedural inadequacies that left too much room for alternative interpretations. But more important has been the failure by all concerned to challenge some important shared assumptions. In their haste to resolve the questions relating to the interpretation of "interesting" errors and, subsequently, "interesting" right answers, investigators have overlooked that prior to the problem of interpretation there is a descriptive problem. This was suggested in the last chapter and can only be confirmed by this one.

Only when an interrogation of taken-for-granted, shared assumptions is followed through (as it will be in the next four chapters) will we be able to determine the developmental significance of the mixed bag of results obtained from standard tasks (like "P") and modified C.Ts (like "O").

Nothing that has emerged in the variety of work considered above convinces us what that developmental significance is. This leaves open the question as to whether the results have any developmental significance.

G. A Pointer to the Way Ahead: an Alternative View of the C.T.

The above review confirms that the C.T. lies at the vortex of a first class interpretative muddle. If, as I claim, the source of the problem is to be found at the descriptive level, the first thing that is needed is a new way of looking at the events and circumstances that constitute a C.T. And I will discuss below one piece of work in the literature that I see as going some way towards the sort of fresh perspective required.

All of the above-mentioned authors involved in the false negative - false positive dispute, from Piaget on, can be seen to share the view that the significant event of the C.T. is the transformation of the array. And this is no less true of those who rightly made the point that it is crucial to view such experiments as social interactions if the child's performance is to be fully understood. Thus McGarrigle and Donaldson, and Light et al were specifically concerned to show that factors altering how the child S perceives E's intentions can influence
what sort of sense that child makes of the transforming event and its consequences.

Thus the outlined dispute has tended to revolve around:—

(a) the child subject's non-conserving judgements about the transformed array in standard C.Ts. (negatives or false negatives?).

(b) the effect on the subject's performance of altering the way the array is transformed, or the apparent reason for transforming it, in modified versions (false negatives --> positives?).

(c) the successful subject's attention to the transformed array in modified C.Ts (positives or false positives?).

(d) the successful (especially) and unsuccessful subjects' justifications of their judgements about the transformed array (positives or false positives? negatives or false negatives?)

The question that arises now is: just how crucial is a consideration of the subject's view of (1) the transforming event, (2) what is transformed, and (3) the consequences of the transformation, in explaining the C.T. results? Something useful may yet come out of a consideration of the false negative - false positive debate. And, in the light of these current doubts, what I now suggest - and will aim to substantiate in later chapters is the following:—

(1) the main thing of value to come out of the false negative controversy has been the false positive controversy.††

(2) the main thing of value to come out of the false positive controversy is the insight that the subject can get away with and thus is best off disregarding both the transformation and its consequences in the post-transformational array. (See chapter 6, section B)

(3) the main thing of value to come out of the insight that the subject is best off disregarding the event of the transformation and its consequences, is that the investigator too, in setting about trying to explain the mixed bag of C.T. results, may be best off disregarding the arranged event of the transformation and its consequences. (See chapter 6, sections E and F)

But if we ignore the event of the transformation, what events remain that we may not be able to ignore in explaining the results? We are left with the fact that the experimenter asks the subject two identically worded questions.

This takes me to a piece of work that I have omitted from consideration so far. One reason for doing this is that it involves a
research context that does not qualify as a bona fide C.T. and so is best viewed separately. Another is that it suits my purposes to save the best till the end. While it pre-dates the McGarrigle and Donaldson (1975) paper and the ensuing wave of controversy, I see it containing the germ of an insight that could have made it the more productive line to pursue. In turn this might have forestalled much of the muddle.

Rose and Blank (1974)

The other authors mentioned focused on that to which the questions refer: i.e. those aspects of the C.T. related to the array and its transformation. The concern here was with what the child makes of this event and the changed appearance of the array that is the consequence of that event. Rather than doing this, Rose and Blank focus on the questions asked and on the fact that Piaget's experimental design (subsequently to be incorporated into the design of "modified" C.T.s) involves a question being asked and then asked again. That is, irrespective of what they refer to, the questions are acknowledged to be events that are part of the context responded to. Rose and Blank's concern is with what the child makes of the event comprising the repetition of the question. They accept that there are other events going on, but note that the events intervening between the questions are arranged to be irrelevant to the answering of the question second time round.

As well as being concerned with the subjects' point of view, they also consider the investigator's perspective. They ask in what way the latter may be misled by what he/she intends to arrange into a blindness about what he/she actually has arranged:

While the tasks used in such studies of necessity must be embedded in a context, the investigator's focus is rarely on the context but rather on the underlying cognitive process that he wishes to study. Therefore, when the child fails such tasks, it is easy to assume that there is a lack of the intellectual skill which the investigator has posited is central to the task. It is rare to consider the possibility that the child may be offering a valid response in terms of the contextual cues with which he is presented.

Consequently, while the authors continue to suppose that the C.T. holds the key to the resolution of a significant developmental issue (see also
Rose, 1973), they refrain from using the terms "failure" and "success" in their analysis (the above passage excepted). In this they seem to be implying that the "error" in the C.T. may be wholly justifiable even if, for some reason, characteristic of the young child.

Their thesis is that, given that in the C.T. things happen but that it is arranged that nothing significant changes, the mere fact that the question is repeated leads the child to change his/her original and approved answer to the question. Dockrell et al are wrong when they suggest:

According to Rose and Blank the child's difficulty in the classical conservation task stems from the fact that in a short period of time the same question is asked twice by the experimenter, thus leading the child to infer that his original answer to the question was wrong and to change his judgement ... accordingly.

This form of argument is what Gold (1985) calls the "failure to communicate a change of mind" position, and was proposed by Bryant (1972, 1974) to explain failure on number C.Ts. Gold produces evidence that such an account of C.T. errors by young children in untenable. And Rose and Blank imply that the child (justifiably) supposes that the repetition suggests that something relevant may/must have happened, rather than that the original answer was wrong. Thus:

The request for 2 judgements ... is taken by the child as a cue that he should alter his first judgement so as to acknowledged the change he has just witnessed" (p.499). And (p.500) "the second question itself seems to suggest that a new judgement is in order [i.e. not a revised judgement]."

(my emphasis)

In presenting their reasoning, the authors explicitly suggest the child (justifiably) takes the repetition as evidence that something has changed:

In the normal (nonexperimental) course of events ... one would never ask the identical question twice if a significant change had not occurred in the material that was being observed15.

Rose and Blank's response is to arrange a context in which the "establishing equivalence" question (Q1) is now omitted. They used a number conservation task and obtained the predicted increased incidence of "conserving" responses to the post-transformation questions. And they concluded that their findings suggested (a) that Piaget's
procedures may underestimate the reasoning competence of the child, but that (b) rather than there being a mistake involved, a "possible linguistic misinterpretation of the child", the non-conserving child on the classical C.T. was able to be seen as responding rationally on the basis of a valid interpretation of the episode that was actually arranged for him/her.

With respect to standard C.Ts, Rose and Blank, like McGarrigle and Donaldson, go further than merely pointing out that Piaget's conclusion - that the child lacks conserving ability - may not be justified on the basis of his negative results. They argue from their positive results that the subjects involved have the competence in question, hence also exposing themselves to the "false positive" criticism. However they acknowledge that "it might be argued that the revised task does not require the same processes as conservation". On the other hand, if it does require the same process, they maintain that "it might be expected that the child would now show transfer to the standard conservation test" (see the training procedures of Gelman and Gallistel, 1978). They demonstrated that such "transfer" occurs. But while this might suggest that the "same process" is involved, it does not actually show that this is conserving ability. That is, it still remains to be shown that the reasoning competence in question is involved in getting the right answer on this (and other) "modified" C.Ts, and/or the original, standard C.Ts. And - albeit inadvertently - Rose and Blank present us with the framework to argue that it is not.

There are legitimate criticisms of Rose and Blank's work, the most obvious objection being to their statement that "one would never ask the identical question twice if a significant change had not occurred". This is patently untrue, and - if it is supposed to imply that the young child is not familiar with true repetition of questions, e.g. as a check on whether something is remembered - highly dubious (see below).

Neilson, Dockrell and McKechnie (1983b) criticize the findings on the grounds of replicability and generalisability, and make a valid point (see below) about drawing conclusions here on the basis of atypical number C.Ts. Also occurring is the similarly valid point that it is not repetition on its own that leads subjects to change their
answers. But in making this point they go off on a wild goose chase that leads to the collapse of their challenge.

Their main criticism is that Rose and Blank do not "explicitly identify what the contextual variable is that misleads the child" in standard C.Ts, and, more specifically, whether it is "simply repetition of the conservation question per se, or repetition following transformation of the array ... "They turn out to be similarly deficient in the first respect, and confused in the second.

The first point to be made is that there is a confusion here over the word "repetition". And Rose and Blank's initial warning about the possibility of confusion on the part of the investigator between what he/she intends to arrange and what he/she actually arranges, seems to have gone unheeded (see also Chapter 8). What is quite evident is that Rose and Blank are suggesting that the non-conserving subject gets the wrong answer because he/she (justifiably) does not hear the "repetition" as a repetition. What they do say influences the child to change his/her response is the occurrence following the question (Qi), of an identically-worded question (Q2). That is, the very fact that the question is identically-worded to the one that shortly preceded it serves as a cue that the words have changed their meaning. Further, while Rose and Blank do not bother about the transformation, possible problems it might present, and influence it might have on the child's performance, they do not consider the question asked in isolation from the context. Being an intelligible question it is not context free, but refers to something, i.e. something present or potentially present. Rose and Blank imply that because of the nature of the arranged context, there exists the legitimate possibility that the answer to Q2 could have been different from the answer to Qi. The non-conserving response may be a "valid (justifiable) response in terms of the contextual cues with which he is presented". For Rose and Blank the important contextual cues are (1) the fact that an "identical question [is asked] twice", and (2) the fact that the circumstances allow for the possibility that "a significant change had ... occurred". Whether what allows this possibility is the (actually irrelevant) transformation, or (the actually irrelevant) anything else is immaterial. All that needs to be conceded is that something relevant could have happened (as is the case - see later).

Thus what Rose and Blank might say here in response to Neilson et al, is that it is an identically-worded question, not a repetition that
the subject responds to. And it is an identically-worded question in a context - the sort of context that, amongst other things, makes "not-same" a valid response - not an identically-worded question "per se".

The third point, in passing, is that it is difficult to see what Neilson et al mean when they call the "repeated" question a "contextual variable" of the C.T. episode - if we accept that the repeating of the question has been a criterial feature of any bona fide C.T. (as much as the fact that an irrelevant transformation occurs). To call the fact that a repeated question is used a "variable" of a C.T. is like calling the fact that a football is used a variable of a game of football. Rose and Blank might be subject to the same criticism if they saw the first question merely as an "optional extra" of the C.T. But they were suitably cautious about claiming that their one judgement task should be taken as a bona fide C.T.

The fourth and final point here is that everything that Rose and Blank say suggests that for Neilson et al to ask "what [is] the contextual variable that misleads the child?", is to miss the point. Given that the non-conserving answer is able to be seen as a valid response, if being "misled" implies a "failure" i.e. that must lead to some rational contradiction in terms of the information available - then it is Neilson et al, not the child here, who have been misled.

Neilson et al are concerned that Rose and Blank make no attempt to deal with the question of "what changes occur such that as the child gets older he is no longer susceptible to such contextual cues [concerning the experimenter's repeated question] and can perform successfully in the standard task". Rose and Blank assume that the results from Piaget's standard C.T. do have some developmental significance. But, from what I have said above, their developmental concern could only be with how it happens to be here that one valid and justifiable way of responding tends to give way, with age, to another valid and justifiable way of responding."

Put at this level, it is perhaps not surprising that Rose and Blank do not bother with the issue of a specific progressive developmental process here. One can take their main point to be a cautionary tale in experimental design. Given the design of the standard C.T., there are alternative ways (for anybody) to attend to it, thus alternative ways of describing the critical features, and thus alternative ways of interpreting the elicited performance. The task is a bad one because it is ambiguous from any rational point of view.
In following through their critique, Neilson et al first try to show that it is not "repetition per se" that leads the young child into changing his/her response on a standard C.T. To different groups they present (1) the pre-transformation array, and (2) the post-transformation array, both alone, asking in each case the two identically worded questions of the C.T. in sequence. As predicted they find that the repeating of the question here does not lead to the child changing his/her judgement. Arguing for Rose and Blank as above, however, all they would say is that they never claimed that \( Q_2 \) is a repetition of \( Q_1 \) "per se". Rather it is a case of "repetition" in a context that allows for the possibility that something relevant might have changed. In the fixed-array conditions they would then suggest that unlike the standard C.T and their "revised version", the context did not allow for the possibility, or at least did not do so to the same extent. These fixed-array "controls" cannot, therefore, shed light on the contrasting findings of the two non-fixed-array conditions.

Nevertheless Neilson et al then go on to compare young children's performance on Rose and Blank's 1-judgement task with their performance on a 1-judgement fixed, final-array condition. Omitting the transformation in the latter, their aim - having supposedly already shown that it is not the repetition that is crucial - is to show that it is, after all, something to do with the transformation that is the critical aspect in bringing about the erroneous change in response in standard tasks. As predicted, they find that "the correct response rate in [Rose and Blank's] 1-judgement condition is almost identical to that in the fixed-array condition." This suggested to them the conclusion that in Rose and Blank's version "the children were simply ignoring the pre-transformational state of the array and basing their judgements on its static post-transformation form".

Given that Rose and Blank held that the facilitatory effect was due to only asking one question in the non-fixed array condition, and that Neilson et al failed to refute this in their first experiment (which only referred to fixed arrays), there is no reason why the former authors would not have predicted this result in the second experiment. Further, they might not have unduly objected to Neilson et al's conclusion - if ignoring the pre-transformational state was facilitated by the omission of the first question. The dilemma facing Neilson et al is that (1) if the child who fails on the standard task can succeed on the revised version while only attending to the final array, then it
might seem that the child is showing that he/she is quite clever, especially in those tasks where counting strategies are unavailable (see Piaget's "compensating operations"). This, of course, would support Rose and Blank's claim that Piaget's procedure underestimates the intelligence of the child. The other alternative is that, (2) the event of the transformation does not necessarily have to have the intended consequence of being potentially misleading with respect to the approved answer to Q2. If so, the "misleading" aspect of the standard C.T. must lie elsewhere. And Rose and Blank need only point to the form of the questioning again (At the moment I am permitting them to turn a blind eye to the apparently conflicting results of the sort of 2-question "modified" bona fide C.Ts considered above).

Now Neilson et al are generally intent on seeing to what extent Piaget can be defended against the challenge of the "optimist" camp and the latter's claim that his procedures underestimate the child's reasoning ability. Further, they are suggesting that it is something to do with the transformation "per se" that causes the young child difficulties. Arguing on their behalf, one way out of the dilemma might be to point out that Rose and Blank's findings only apply to number C.Ts and that on a number C.T. the right answer can be got by mere inspection of the final array and adopting a counting strategy (see "Postscript" below). This could apply to both the above 1-judgement conditions and would explain the increase in "conserving" responses without a rational conserving competence having to be invoked. Hence once again we are dealing with false positives. This might all be very compelling, given the doubts about number C.Ts, and given that indeed Neilson et al found poor replicability of Rose and Blank's finding when other C.Ts were used.

Unfortunately: - (1) the evidence they depended on came from elicited justifications - a source that even they, in the end, agree is unreliable and which I have argued above should be jettisoned, especially in the context of the C.T.

(2) their evidence led them anyway "to dismiss a counting explanation of the facilitatory influence of the one-judgement procedure" (my emphasis).

Thus the dilemma - totally unsatisfactory for them - remains, and Neilson et al's particular challenge collapses. But it has allowed a number of important points to be made. And Rose and Blank may still be
wrong to suppose that their results show that Piaget underestimated the reasoning ability of the young child.

A Link to the Previous Discussion

I am directly concerned with bona fide C.Ts. Even if Rose and Blank's "revised version" does test for a reasoning competence, and the same one that the standard C.T. is supposed to test for, it is not here to be taken to qualify as a bona fide version. However, my interest is in the bearing their work might have on the interpretation of the results of the two sorts of bona fide C.T. considered. With respect to the standard tasks, their evidence is supposed to suggest:

(1) that the young child has the rational competence in question, and
(2) that the subject gives the non-conserving response because he/she is justifiably influenced by the "repetition" aspect.

But, remembering the other sort of C.T., this raises the question: why - in Rose and Blank's terms - does the young child tend to succeed in modified tasks, like the "Naughty Teddy" and "0" versions, where there is also repetition? We might here expect them to generally concur with McGarrigle and Donaldson that the Naughty Teddy version presents a more natural, less potentially misleading, interactional setting than Piaget's version. There would just (?) be a difference in emphasis with respect to the description of the significant events of the episode. Thus McGarrigle and Donaldson focus on the events concerning the array. They suggest that the incidental/accidental aspect makes the non-verbal component (the transformation) less potentially misleading in relation to the verbal component (the questions asked). Rose and Blank, by contrast, focus on the events constituted by the asking of questions. They would suggest that something about the modified context makes the verbal component less "misleading" in relation to the non-verbal component. That is, they would presumably have said that in the modified C.T. it is less likely that the repetition will be heard as a "repetition" signifying a relevant change. Both would argue that it would seem to comprise a fairer test of reasoning ability than the standard version, because the young child, suspected to possess the ability in question, now gets the criterial right answer ("same").

But we have seen that McGarrigle and Donaldson run into the false positive problem and we can anticipate that an interpretation in Rose
and Blank's terms might do the same. The upshot of Neilson et al's (1983a) criticism of McGarrigle and Donaldson's design was that the choice is not necessarily between a potentially misleading transformation in the case of the standard C.T. and a non-misleading transformation in the case of the modified C.T. Rather it is between two potentially misleading ways of transforming the array. That is, the choice is argued to be between a transformation that may bias the results in favour of failure (raising the possibility of false negatives), and a transformation that may bias the results in favour of success (raising the possibility of false positives). In the latter case the procedure may not only enable/trick the subject to disregard the way the transformation comes about, but encourage him/her to ignore what comes about, i.e. the perceptual consequences of the transformation.

Similarly, pursuing Rose and Blank's focus on the questions, the choice may not be between a misleading and a non-misleading form of questioning in the ("Naughty Teddy" or "O") modified C.Ts, but between two potentially misleading forms of questioning. And a further examination of the two papers by Neilson et al (1983a and 1983b) allows us to identify the problem here, and consider its implications for making sense of the two contrasting sets of results.

In the former work, Neilson et al accept McGarrigle and Donaldson's claim that it is something to do with the transformation that can be considered the crucial component in determining whether the child will fail or succeed on a bona fide C.T. But rather than agree that the way the transformation comes about in the naughty teddy version makes it a fairer and better reasoning task with which the child can cope, they argued that the child can get, and shows evidence of getting, it right just by attending to the pre-transformational state of the array. In the latter paper, the authors - still focussing on aspects related to the transformation - addressed Rose and Blank's study. This time they claimed that the child can and may get the "revised version" right, again not by virtue of possessing the competence in question, but by just attending to the post-transformational state of the array.

The first point to be noted here is that there is no necessary reason why either of these strategies should not be applied to any bona fide C.T., including standard tasks, where a subject might also only bother to attend to the pre-transformational array, or to the final array. Secondly, while we saw that there was a good deal amiss with Neilson et al's analyses when considered separately, something, at the
same time odd and useful, emerges when we combine their respective conclusions. They are trying to argue that the crucial aspect of the standard C.T., and one that the representative young child shows he/she does not understand, is something to do with the change of the array from its pre- to its post-transformational state. Now it is not just that they fail, with their explanations of success on the naughty teddy and Rose and Blank versions, to pin-point what exactly the crucial aspect is. Combining their conclusions actually suggests that the general hypothesis can be contradicted. Getting the right or wrong answer may be nothing to do with the transformation. What is indicated is that a subject can and may get the right answer on a bona fide C.T. by ignoring "everything" in the way of events intended to be significant; i.e. (1) the pre-transformational array (viz. 1983a), (2) the post-transformational array (viz. 1983b), and (3) the transformation (viz. 1983a and 1983b). While this is presumably an unintended conclusion, it is evidently a true one given that (a) a C.T. can only proceed if the initial "establishing equivalence" question is answered correctly, and (b) that, if it is, a subject can get the right answer by ignoring or disregarding all three aspects. All that is necessary is to recognize the question as the one already asked, and remember the approved answer.

In such a case of "negligence" this would seem to leave the "interrogative events" not ignored (if the subject answers the questions we presume that he/she has noticed that questions are asked). And this takes us back to Rose and Blank's focus on the form of questioning, and the fact that repetition occurs. Only now we would want to ask how could repetition influence a subject to give the right answer on a bona fide C.T. The response here is surely obvious. If the identically-worded question is heard as a true repetition, rather than as a "repetition" suggesting that a significant change has occurred, then we might expect the subject to repeat the first, approved answer — given that everything else (initial and final arrays, transformation) is ignored or disregarded.

Q2 is evidently not heard as a true repetition by the non-conserving child in the standard C.T. (ruling out Bryant's "change of mind" hypothesis\(^9\)). But it could be by the successful child in the modified version. In the earlier paper by Dockrell, Neilson and Campbell (1980) the authors are more disposed (I suggest correctly) to attempt "to reconcile McGarrigle and Donaldson's findings with Rose and
Blank's", rather that the other way round, i.e. with the emphasis on the questions rather than on the array and its transformation. They say that to do this with the naughty teddy findings,

teddy must be seen to have facilitated conservation responses by providing a reason in the child's eyes for the experimenter asking the same question twice: namely that, given teddy's unruly and noisy behaviour, the experimenter had forgotten he had asked the question, or had forgotten the child's answer to it.

This therefore may be considered a spelling out of why the form of questioning in the modified task is no less potentially "misleading" than that in the standard C.T. While the latter might lead the child with the reasoning ability in question to contradict what his reasoning should have told him/her, the former might lead the child - with or without the competence in question - to not even notice that there was the occasion here for exercising that reasoning ability. It is presumably mis-leading because the experimenter arranged it like this, and has not "forgotten he had asked the question"; nor "had [he] forgotten the child's answer."

While there is the basis of a breakthrough here, where Dockrell et al go wrong is:-

(1) to imply with such an interpretation that there is necessarily some sort of mistake, or irrational oversight on the part of the child here - in anybody's eyes. After all the question, Q₂, is a repetition of the identically-worded question, Q₁, given that it has been arranged that nothing relevant intervenes (or that only irrelevant things intervene). Thus there is no need for the exercising of that reasoning ability.

(2) to suppose that it is necessarily something to do with teddy that facilitates hearing Q₂ as a true repetition, "by providing a reason [sic] ... for the experimenter asking the same question twice".

I have already pointed out that the subject can disregard teddy, what teddy does (the transformation), what teddy does it to (the initial array), and what the result of teddy's actions is (the final array) - and still get the right answer merely through attending to the form of the questioning. But the form of the questioning as well as being ambiguous, is the same as in the Piagetian version. So, apart from teddy, what else that might make a difference is there to attend to, present in the modified but absent in the classical C.T.?
At this point I am in the danger of getting ahead of the argument and the sort of evidence required to properly substantiate it (see chapters 4, 5 and 6). What is evident is that there is a shift of emphasis away from the question of the "falseness" or "trueness" of negatives and positives, and correspondingly away from the involvement of reasoning competence in determining performance on different versions of the C.T.

Corresponding to this shift of emphasis, the general lesson to be extracted from the quoted approach of Rose and Blank is that there is something odd about the basic design of the C.T. and that the characteristic form of questioning may be viewed as making a key contribution to this oddness.

I am prompted, therefore, to try to link observed performance on various C.Ts to - in the first place - features of those tasks, rather than to the presence/absence of particular abilities. When attempting to trace out the possible path of influence of such features, it can now be suggested that it may be profitable to link them to their likely direct effect on the way the questions are taken. This may be more productive than to try to link them to any direct effect they might have on how the array, the transformation and the consequences of the transformation are viewed and understood.

G. Conclusion and Post-Script

There is now even more reason to suppose that the indicated confusion over how to interpret C.T. results merely reflects a deeper descriptive problem. How "negatives" and "positives" have been interpreted has depended on what investigators have chosen to view as the significant events and circumstances of a C.T. The disputes, sampled above, have been about what particular results do show. But rather than showing that 3 - 5 year olds possess or lack a reasoning ability, they show that we have on our hands a muddle and that this is largely a consequence of the oddness of the C.T.

Now an adequate description of the significant events and circumstances of a C.T. will be one that indicates what, given its nature, any bona fide C.T. can show. And, as we are in doubt, this is the more basic question and the one to be addressed first. So it is to
this question that I turn now, having determined to try to resolve, rather than - with equally good reason - simply abandon the muddle.

Various pointers as to the way ahead have been able to be made merely on the basis of this selective literature review. What is called for at this point is empirical evidence - or at least a fresh approach to the mass of evidence that already exists. The next four chapters are therefore primarily concerned with arriving at an adequate framework for describing: (a) what C.Ts are to be taken to consist of, and (b) what subjects are to be viewed to be doing when they participate. Only then can the move be made to explaining the mixed bag of success/failure results obtained from young children doing different C.Ts.

The assumption that I will proceed on is that it is not "the absence of a simple control [that] discredits the whole conservation enterprise" (Bryant, 1982 a, p.5; my emphasis). If I am right in the points made so far, it may be something to do with the basic task design that has ensured that the enterprise stands discredited.

Post-Script

One circumstance, possibly contributing to the muddle but having nuisance value rather than anything else, is that an unduly large amount of the false negative - false positive debate revolves around the results of number C.Ts. In other sorts of C.T. (e.g. involving liquid, powder, pasta shells, plasticene etc) the correct judgement ("same") on Qa cannot be arrived at merely by inspection of the post-transformational array. Simple counting or measuring strategies are ruled out, the reasoning task that the subject is intended to be left with being of the form (see tasks "P" and "O") Given that there was the same amount in containers A and B, and given that the experimenter (or a naughty teddy, or whatever) merely poured the contents of B into the large container C, then - irrespective of the appearance - there must be the same in A and C.

It may be that the necessity of these premises, at least from any subject's point of view, can be challenged (see later). But if we allow that they cannot, then such tasks would seem to test for the competence in question; i.e. conserving ability, involving knowing that quantities do not change capriciously and requiring something like a reversibility operation (see Chapter 1). However this is not necessarily the case for
a number C.T. If the subject can and tries to count, he/she can get the right answer merely by examining the final array (one bunched and one unbunched row of counters) and without having the competence in question. And if the subject cannot but tries to count, he/she can get the wrong answer while having the competence in question.

Number conservation tasks differ from other conservation tasks such as length, weight and volume in that it is potentially possible for the child to correctly judge the post-transformational equivalence of the array empirically, without reference to its pre-transformational state, by counting or subitizing.

[Neilson et al, 1983 b] Given the direction my empirically-supported analysis takes, it will be seen by the end of Chapter 7 that the particular characteristics of different types of C.T. need not bother us unduly. The emphasis will be on what they all have in common. And it is what they have in common that allows a single-factor level of explanation of the results, including those of number C.T.s., to be offered.

NOTES

1 With respect to the "Naughty Teddy" version (see below), that the game was "going somewhere" - beyond the "establishing of equivalence" of the rows - is implicit. Otherwise teddy's intervention would hardly be able to be presented (as it is) as an unwanted interruption.

2 The wording of the last part of this sentence is significant both for the "accidental or incidental?" discussion below, and for points to be made later. "Not relevant to what he meant" can be replaced by incidental to what he meant," on the one hand. On the other I will take issue with the "move on", and will suggest that an alternative description here - especially in the light of the post-transformation (albeit unacknowledged) "where were we?", question (Q3) that occurs, or is implied, would be that he moved back to the initial question.

Also to be noted is the confusing oscillation, in the next sentence, between the investigator's perspective ("experimental material") and the child subject's supposed point of view (the free agent "naughty" and "swooping" teddy "messing up the game"). If the C.T. is to be asked to make some developmental point we might criticize the introduction of a child-like incorrect view of the overall events, as premature. (Stuffed toys are not "naughty" and are not intent on sabotage).

3 However McGarrigle and Donaldson could conceivably decide to let the results speak for themselves in showing that for some reason Piaget's task underestimates the child. If the transformation is still
as intended by the experimenter, then they could argue that the child is showing the presence of the ability in question even without making it easier - at least in the way they were aiming at. This, of course, still leaves the problem of accounting for the original failure, even if it apparently strengthens the "optimist" position.

Thus in task "O", to keep this modified version as close as possible in line with the standard task "P", termination of the episode occurs after O has been answered.

Does this mean that it is possible that something might have changed? If there is information to suggest this, we will have to be clear that there is also sufficient information present to rule out this possibility - having arisen.

For example, Light et al go on, "it may transpire that an important aspect of the 'transition to operativity' is the establishment of that degree of personal autonomy or detachment which enables the child to separate the meaning of words from the meaning of the [social] contexts in which they are uttered".

Such conduct was a frequent observed occurrence with 3-5 year olds in my own replications of this modified task. The significance here is that while a subject who does this presumably must have "registered" the final state of the array at some point, he/she may then choose to disregard it.

A large part of the present work revolves around the question of what the investigator should attend to, and how he/she should view and describe the significant features of the C.T. in order to best explain the obtained findings. Since there is room for doubt here, caution needs to be exercised in prescribing what the subject should attend to and how he/she should attend to it.

In terms of Brainerd's (1973) classification of types of criticism of Piaget's findings, to be found in the literature, this approach falls loosely under the heading of "stimulus criticisms", which "concern methods of task presentation, and ... include the usual sources of Type II error in studies of children's reasoning" (false negatives). The other approach (i.e. (b)) roughly coincides with his "response criticisms" and "concern the specific behaviours which are used to infer the presence of cognitive structures". He points out that a main point of contention has revolved round the usefulness of justifications, and the issue I will focus on is the role of justification in avoiding Type I errors (false positives).

However, leaving aside justifications, one of my points will be that great caution should be exercised in applying such a rigid distinction. I argue that investigators have been misled over the basic description of the "stimulus" when any bona fide C.T. is presented. Until we are clear what a subject responds to, we cannot be confident that we have described the subject's "response" accurately - or at least usefully (see Chapter 8).

If our aim is to gain access to how the subject's experience of the array is in the first place organised, we need to be careful with evidence as to how it is re-organised (at the request of the experimenter). And we can be doubly dubious about evidence pertaining to the re-organisation of that re-organization - i.e. that resulting from a request for justification here.
Brainerd (1974) is dismissive of the frequent citing of "Piaget's conjecture that some sort of linguistic experience plays a role in the emergence of formal-operational structures." He points out that "Piaget drew a sharp distinction between this conjecture and the unacceptable (to him) proposition that these structures are linguistic in any sense."

Presumably because (1) it is modified procedures that tend to elicit success, while standard procedures tend to elicit failure in the age group concerned and (2) it is, supposedly, only a right answer (success) that can be adequately justified.

Into the nature of the task, not into the young child's mind (see Chapter 6).

More generally, I will suggest (see chapter 5 and 6) that the main thing to come out of the preoccupation with explaining the child's "interesting errors" on standard C.Ts. is that at some stage the investigator must consider, or reconsider, the question of what exactly is involved in success on any C.T. The optimists' project is obviously a help here as it has given us instances of success to consider.

Hence posing a problem for Rose and Blank's interpretative framework (see below).

This is untrue (see below). The important point that I extract is that asking an identical question twice without an observable relevant change occurring, and with information as to the questioner's intentions withheld, introduces a critical ambiguity.

We must presume that Rose and Blank would accept that giving the right answer is also a "valid response". But we are entitled to want to know, if the wrong answer comes about "validly", in what way(s) - on their conception of the significant aspects of the C.T. - the right answer can come about validly (see below).

Having suggested that the repetition is a "contextual variable" we might have expected Neilson et al to provide more information about the way the questions were asked, suspecting that, for example, a memory check repetition is not normally asked in the same way as a "repeated" question after something significant has been arranged to change (see Chapter 6).

Even if we do not rule this out, it can be argued that a subject who suspects he made a mistake on Q₁ does not hear Q₂ as a true repetition. Something significant is still suggested to have changed, even if it only the evidence for answering the one question asked.
PART II - EXPLAINING THE RESULTS: THE ARGUMENT
PART II - EXPLAINING THE RESULTS: THE ARGUMENT

CHAPTER 4

CHALLENGE I - FROM THE QUESTION OF WHAT PARTICULAR VERSIONS OF THE C.T. DO SHOW, TO THE QUESTION OF WHAT ANY VERSION CAN SHOW

A. Introduction

Happily or unhappily - depending on your point of view - Piaget set the ball rolling by giving us the original classical versions of the C.T., exemplified here by task "P". The robustly replicable, supposedly interesting result that his theory required, was obtained: the representative young child gives the wrong answer. Piaget designated the C.T. a test of concrete reasoning ability and interpreted the child's wrong answer as the product of a lack of logical competence. But, in employing his C.T. to show that young children could not reason, he set himself up for contradiction: that is, for others to use C.Ts for the opposite purpose of showing that young children can reason.

Thus the "optimists" came along with independent and often compelling reasons for supposing that the child of, say, 4 or 5 years possesses the rational competence in question. And, in exploiting the C.T. context for their own ends, they designed other versions, exemplified here by task "O", where the representative child who failed on "P" now gets the right answer. But, in taking this to indicate the presence of reasoning ability, they in turn set themselves up for contradiction.

As we saw, the next wave of research suggested that success on the modified C.Ts could not be taken to indicate the presence of reasoning ability. Hence classical versions like "P" might be the more reliable indicator after all. The concern is still with the question of whether or not the young child can reason. But, yet again, we find that the disputants set themselves up for contradiction. And now the challenge is one that has repercussion for all sides in the dispute.
For one thing, something of a paradox is suggested. This will resurface in chapter 6 and is that if the "task" is such that the subject can get it right without having to do the envisaged impressive rational processing, then he/she cannot be faulted on rational grounds for doing so. More significantly here, if it is argued that task "O" cannot be viewed as a test of reasoning ability, what grounds are there to suppose that task "P" can - given how much "P" and "O" have in common (see below)? And, if neither "P" nor "O" are able to be viewed as reasoning tasks, one might ask: what are we doing trying to challenge or defend Piaget (or the optimists) over the issue of presence/absence of reasoning ability, on the basis of C.T. results?

I have pointed out that the conservation controversy has been about what particular versions of the C.T. do and do not show. That is, the problem has been seen as one of choosing between alternative interpretations of the agreed behavioural evidence (successful or unsuccessful performance), obtained from versions of the task where, in each case, the design features can be checked and agreed about. All the results are generated from versions of the task seen - at least by the inventors - to involve reasoning. And, where there has been doubt about this, notably in relation to the optimists' modified tasks, precautions have been taken - or suggested - in an attempt to ensure that they do test for what they are supposed to test for. However, it was seen in chapter 3 that the project of taking precautionary measures to this effect may be destined to run into trouble. Now if, because of the nature of the task, it is to be doubted that we can take the necessary steps to ensure that we are dealing with reasoning tasks, then all interpretations of performance in terms of the presence/absence of reasoning ability may be invalidated. As anticipated in chapter 2, and reinforced by the review of literature in Chapter 3, the first step towards resolving the interpretative problem would now seem to be to locate and identify the underlying problem at the descriptive level. It is therefore advisable - at least temporarily - to suspend the search for better interpretations (and, correspondingly, the search for better versions of the C.T.) and re-examine how we are to describe the significant aspects of any C.T. episode.

The argument so far has, therefore, all led up to what I term "challenge one". It culminates in the proposal that we should turn away from the debate about what particular versions of the C.T. do show, and
go back to the more basic question of what any version of the C.T. can show. With this shift of emphasis, we disregard (a) whether we are dealing with a "classical" or "modified" C.T., (b) how the subject actually performs, and (c) who/how old the subject is.

It was assumed, apparently by everyone concerned, that C.T. results can show - providing appropriate precautions are taken - something with a bearing on the developmental issue of the presence/absence of reasoning ability in young children. Now that it is in doubt that the conservation experiment can shed light on this particular developmental issue, whatever precautions are taken, we might ask: what developmental issue can the C.T. shed light on. But such an orientation would once more set itself up for contradiction. For, as we shall see in Chapter 7, it becomes questionable whether the C.T. can shed light on any developmental issue.

While some of the above researchers (see Chapter 3) might in the end be prepared to let the C.T. go as a test of a specific reasoning competence (see Donaldson, 1982, p. 48), they are not prepared to let Piaget's version go as some sort of discriminating test, and definitely not prepared to entertain the idea that the results may have no developmental significance. The possibility is not considered that the C.T., because of its nature, may not be able to do any work in relation to developmental theory.

It will be argued that it is up to "developmentalists" to establish that there is a developmental issue at stake, rather than up to me to prove that there is not (see Chapter 7). But, given the weight of opinion opposed to such a conclusion, I will concede that my claim needs to be carefully examined. In the meantime, if there is substance to it, then we might have expected that each successive stage of the conservation enterprise would be marked by the fact that - as I have pointed out - the investigators involved set themselves up for a challenge from some other quarter while leaving the original developmental (?) question, posed by Piaget with the results of the standard task, unanswered. And, as we saw in Chapter 3, the overall result has been a big muddle.

It can be noted that at each stage of this story of criticism and counter-criticism, those doing the criticizing generally praised the insights and/or ingenuity of those they criticized. And everyone, of course, praises Piaget for giving them such a "provocative and important" developmental question that is a "joy" to grapple with, as
well as seeming "answerable and ... very close to a solution" (Bryant, 1982 a, p.5). The dispute, therefore, has been a relatively friendly one. If it had been a little less "friendly" perhaps we would not have been left with such a muddle. That is, if it had been recognized that some fundamental assumptions could have been challenged, we would not be left seeking a strategy to clear up the muddle.

However, articulating "challenge one" is the first step in formulating the required strategy. It allows us to press forward towards providing an answer to the initial question, Question A: why does the young child fail on "P" while succeeding on "O"? That is, why does the young child say "not-same" on "P" while giving the opposite response ("same") on "O"? In chapter 2 I pointed out that posing this question before the "developmental question" (Question B) allows us to keep the developmental issue - at least temporarily - at bay. What also emerged was that we must bear in mind the possibility that Questions A and B are misleadingly posed. In that chapter, examination of Piaget's project layed the ground for doubts as to whether the crucial C.T. evidence could support his or any other theory of progress. The element of oddness that emerges from examination of the optimists project enhances these doubts and gives us an idea of what to expect when we come to contrast and, more significantly, compare tasks "P" and "O". It is through identifying what the two versions have in common that we will be able to pose the more general question, i.e.: Given what C.T.s have in common, what is the most parsimonious, psychologically-relevant and empirically-supported general account of that which determines whether a subject, irrespective of his/her age, will succeed or fail on any version of a bona fide C.T.?

This is Question C. It will be seen that adequate answers to Questions A and B can eventually be derived from the general level of explanation yielded.

B. Initial Doubts About "P" and "O": 1st Empirical Note

To the extent to which Piaget can be seen to have set himself up for contradiction through the strong claim that his C.T. is a reasoning task and failure on it shows that the young child cannot reason, then the optimists' project of "turning failure into success" might be seen
as justifiable. But we immediately want to know what sort of a context is the C.T., that has apparently been able to provide such contradictory evidence. What is therefore sought is a potentially helpful general descriptive statement about the essential nature of any C.T.

1st Empirical Note

Everyone who has actually done research with C.T.s has probably, at one time or another felt that there was an ineffable oddness about what they were doing (e.g. Hughes, personal communication). In initially carrying out various replications, including "P" and "O", I was no exception. When the child gets the wrong answer (notably here in the case of "P"), I, like other apprentice C.T. researchers, was struck by how strange it is that a bright 4 year old can be so easily fooled into thinking that something had happened, when it was so obvious that nothing had happened that could have made a difference to the amounts being compared. On the other hand, on those occasions where the young child gets the right answer (notably here in the case of "O"), the strangeness lies not in the subject's response, but in having to recognize that one has arranged an episode where nothing happens. Indeed it is frequently found to be too embarrassing to admit to the child that "that's all there is to it - you can go now". The successful child generally seems to expect that there will be a "point" to it all (see Chapter 5) and the investigator more often than not finds him/herself compelled to pretend that there is, or may be, a point to it after all. Alternatively the experimenter will attempt to divert the child's attention by moving hastily on to something less point-less.

My project can, at least in part, be viewed as an attempt to make "effable" this strangeness. And it starts from the descriptively significant acknowledgement that the essence of any C.T. is that nothing relevant to what is asked about actually happens. An alternative way of putting this is to say:- only irrelevant things happen - including the irrelevant transformation, among all the other irrelevant things going on in the vicinity (E's mannerisms, traffic noise, etc, etc).

Bearing this fact in mind, and its possible significance in relation to the question: what can any C.T. show (viz "Challenge One"), let us look closer at the classical C.T., the take-off point for the optimists' project and, indeed, the whole conservation controversy.
C. What Can the Standard Version ("P") Show?

The group of researchers I have labelled for convenience "the optimists" were concerned that a task like "P" could not show what Piaget intended it to show. While they accepted that success (i.e. in older subjects) indicated that the subjects involved could reason, they challenged the view that failure (i.e. in younger subjects) indicated that these subjects could not reason.

There is no problem in accepting their doubts about "P". They justifiably make the point that "P" is not a good reasoning task because the young subject, while possessing the competence in question, may be deceived in some way by the artificial nature of the context, viewed as a child-adult interaction. A potential result of this is that the subject will be misled into believing that something relevant was intended to change and has changed, when it has not. Successful performers on "P" - that is older subjects - show themselves to be less susceptible to the deceptive component.

Piaget would have no quarrel with the view that a subject fails through succumbing to a deception, and succeeds by overcoming it. But the "deceptiveness" of the context he intended, and would have in mind, is solely that relating to the appearance of the materials constituting the transformed array. It is this misleading appearance that the subject must "see through", not - as the optimists suggest to be in the first place required - the potentially misleading perceived intentions of E. We can pass over the problems surrounding the view, shared by both Piaget and these critics, that the appearance of the post-transformational array is necessarily deceptive or problematic, requiring reasoning if the problem is to be overcome (see Chapter 8). What remains here is the supposed effect of the potentially deceptive perceived intentions.

In the 1st "Empirical Note" I pointed out that it seems odd and surprising that anyone, including the young child, should suppose that the amounts - having been the same - become unequal when all E does is just pour the contents of container B into C. But it starts to become less odd when the following is acknowledged:-

(1) that perhaps the child might not believe that what happened was "just" pouring. The question then is: how justifiable, or at least how intelligible, is such a belief? It becomes more intelligible when it is noted,
(2) that in general here the child is encouraged to expect that something interesting will happen. He/she is invited to "come and play a game" and told to "watch carefully" prior to the transformation. More specifically, though speculatively, the child may be familiar with adults doing conjuring tricks and may expect one in this sort of context. Further,

(3) that, given the materials at hand and the E's portentous manipulation of them, it is possible that something interesting could indeed have been arranged to happen (see Smedslund, 1961; also chapter 7 here) to make the answer to the second question (Q₂) different from that to the first (Q₁).

Points like these (especially the first two) provide the background for optimist-oriented criticism of Piaget's task as a sensitive measure of the presence/absence of reasoning ability. Thus McGarrigle and Donaldson (1975) emphasized the potential deceptiveness of the latter's non-verbal conduct. However, they were not content to merely draw attention to potential flaws in Piaget's "pessimistic" interpretation of the resulting non-conserving responses. Being optimistic, they wanted to argue, and seek supporting evidence, for the opposite interpretation. But, while arguing that Piaget's task was a bad test of reasoning ability, they went along with the view that there was a reasoning task in there somewhere. If this assumption is wrong, we can expect all sorts of problems to arise. Exposing them will help us with Challenge One. So let us examine the optimists' project in relation to the C.T., viewing it in terms of the two available strategies for "turning failure into success", linking the flaws that emerge in each case.

D. Turning Failure into Success: Doubts About the Optimists' Project

There's no success like failure, and failure's no success at all
(Bob Dylan, 1965)

Adding "nor is success" to this quotation, I will argue that this is the sort of conclusion that analysis of the optimists' project leads us to, in relation to the task of interpreting performance on C.Ts. That is, instead of achieving their aim of turning apparent reasoning
failure into actual reasoning success, the optimists merely suggest to us that the wrong answer on any bona fide C.T. can always be viewed as potentially too impressive to be dismissed as "failure". Conversely, success may always be able to be viewed as totally unimpressive.

The wrong answer may not indicate the absence of a reasoning ability. More than this, it may not be able to indicate the absence of any ability. And, not only may the right answer be insufficient to indicate the presence of a reasoning ability, but it may not be able to serve as an indicator of any ability - beyond whatever ability is involved in seeing that the question should be answered and that the answer should come from one of the offered alternatives. And these abilities are already presumed to be possessed by all subjects able to participate in a C.T.

In relation to "Challenge One", it will be noted that this section is primarily concerned with what the C.T. cannot show. But it will help us in tackling the main question: what can any C.T. show us, and how are they best viewed to achieve their (success/failure) results.

(a) First optimist strategy: keeping "content" constant, analyse performance and context for a new "form" that allows failure to be now seen as success

The aim here is to re-interpret, preferably with empirical support, the child subject's wrong answer to E's question on a chosen task as the right answer to a different question. Here the "content" of the cognitive task tends to be left as it is. What is sought is a new sense-making logical "form" that is compatible with the observed performance and that allows that performance to be viewed as rationally intelligible. While further observations and experimental evidence may be invoked, a conceptual re-working might be sufficient. Whatever, the original empirical finding (the wrong answer) is not challenged, and the aim of any further experimentation is not to elicit the right answer (for this, see "second optimist strategy"). It is: (1) to confirm that the question answered is not the one intended by E, (2) to attempt to identify what question the subject answers, (3) to argue that the asking of this question is supported by the context and that, from a certain point of view, it is a justifiable way of taking what is asked, (4) to
establish that the subject answers this question correctly and may do so by virtue of a consistently applied concept.

Gareth Matthews, the author of "Philosophy and the Young Child" (1980), in a paper in preparation, entitled "Intellibility and Error", provides a challenging exposition and application of this strategy. While it temporarily takes us away from the C.T. per se, the value of considering his approach lies in the fact that it dovetails with the initial phase of optimistic empirical approaches to the young child's performance on the classical C.T. The added bonus is that the closer we look at the cognitive developmental task chosen as an illustrative example, the more it begins to resemble a C.T. 2

Matthews sought a methodological basis for attributing "special concepts" to young children and formalised his strategy in the explicit terms of an "Intelligibility Principle":-

Other things being equal, we are to attribute to a child all and only those concepts such that attributing them enables us to make the most sense of the child's regular, predictable behaviour.

In effect this states that: if we can interpret the child's (or, presumably, anyone's) regular and predictable observed responses - including those that look like errors - in terms of reasoning successes rather than reasoning failures, than we are under an obligation to do so.

He applies this principle to the case of Piaget's (1970 b, p.122-132) finding of the young child's ostensible reasoning error on his "Tunnels Task". In the crucial condition of this experiment, replicated by me (paper in preparation), though apparently not by Matthews, the subject is placed in front of two tunnels of different lengths. At the entrance of each tunnel ("start") a doll (or toy car), attached to a stick that runs through the tunnel, is placed poised to enter the tunnel.

![Diagram]

The child (S) is first questioned about the relative lengths of the tunnels. When S agrees that A is longer than B, he/she is asked to say
the word (e.g. "Go") to set the dolls in motion. At this point the experimenter (E), by pushing the sticks, moves the dolls through the tunnels in a way designed to ensure that they re-appear at the respective exits simultaneously, where they stop. The S is asked the standard question, "Did the boy doll go faster, or did the girl doll go faster, or did they go at the same speed?" Appropriate controls are applied, by varying (1) which doll goes with which tunnel, and (2) the order of the clauses in the question. The other condition involves the same procedure but with the tunnels removed, the dolls starting from and finishing at the same points as in the "tunnels-on" condition and, again, both starting and finishing simultaneously. The combined procedure that produces the result that discriminates the youngest ("stage 1") children from older subjects involves running the tunnels-off condition first, followed by the tunnels-on condition. Putting the tunnels in place constitutes what in the C.T. would be called "the irrelevant transformation of the array". The young child tends to get the first (tunnels-off) question right and the second (tunnels-on) question wrong. While the results of my replication were much more variable than Piaget's original results, the 3 - 5 year old commonly says "same speed" on the latter.

Piaget interprets such a result in terms of a reasoning deficit on the part of the young subject. The S, he says, is unable to co-ordinate the two facts, (1) tunnel A is longer than tunnel B, and (2) there is the same duration between starting and finishing, to conclude that Doll 1 goes faster than Doll 2. In both conditions the answers given are the result of overriding but different "perceptual intuitions". Thus, in the tunnels-off condition, the dominant influence is the intuition of Doll 1 overtaking Doll 2, leading to the right answer. In the tunnels-on condition, the child is perceptually "seduced" by the intuition of simultaneity - of the reappearance and stopping at the respective exits of the tunnels.

Applying his "intelligibility principle", Matthews says that how we view the child's performance changes once we acknowledge that he/she is likely to view the episode in terms of a race. Thus E, from the child's point of view, is to be taken to be asking "who wins?" Now the "special concept" of "winning a race" comes in two alternative forms:- (1) the doll that gets to its goal first wins, or - where there is no obvious goal - (2) the doll that gets ahead and stays ahead wins. The application of this concept leads the child to the right answer in the
tunnels-off condition (Doll 1 gets ahead and ends up ahead), but to the "wrong" answer in the tunnels-on condition (Doll 1 and Doll 2 arrive at their respective goals - the tunnel exits - at the same time). The presence of the tunnels limits the child to applying the "goal" aspect of the concept. Both these answers can therefore be interpreted in terms of the rational application of a "special concept" rather than as the product of perceptual intuitions.

While Matthews does not actually say this, extrapolating from his argument, the child may have the concept (speed as a function of distance and duration) in question. In other, less misleading, circumstances he/she might be able to apply this concept appropriately.

Defence of Piaget

As is the case of the C.T., Piaget protects himself from criticism by adopting strict criteria for attributing the presence of the rational competence in question. The charge that the child does not understand the question does not wash with him, for understanding the question is part of the task. The child must not only show that he/she can use concepts in a rational manner, but must be able to show that he/she knows when and in what context - including the linguistic context - to use them. The subject here is not being asked about the winning of races, but about relative speed.

Criticizing Piaget (in relation to experimental design): 2nd Empirical Note

One can criticize Piaget (a) for his somewhat arbitrary and post hoc choice of what "perceptual intuitions" are likely to be operating on the part of the child in a specific, arranged situation, and (b) for the way he discriminates between the role of immediate ("intuitive") perception and that of the intellect - the former being seen as something deceptive and to be overcome by the operation of the latter (see Appendix II); also, (c) for his "pessimistic" interpretation (see Defence of Matthews, below) and (d) for his neglect in relation to the fact that the arranged experimental setting is a social interactive context (e.g. see Chapter 3, section C). Yet all such criticisms - as
in the case of the C.T. — just tend to converge on a criticism of the essential design features of the experimental context. And it is the design, as always, that produces the results his theory of progress is seen to need.

The main point here must be that Piaget, purporting to be investigating the child's understanding of movement and speed, employs on the crucial (tunnels-on) condition a test where nothing happens in the way of visible movement. All the movement takes place out of sight of the subject, who is left only with the sudden disappearance and reappearance of the dolls. With the movement taking place out of sight, this introduces any amount of interesting possibilities, including some that would make the designated wrong answer the right answer. And children often show themselves well equipped to identify such possibilities. For example, in my replication, one 4 year old said that Doll 2 went faster because the road inside its tunnel (shorter tunnel B) was "wiggly". Another little girl said that the lady doll (in that case Doll 2 in tunnel B) went faster because she had to stop inside [the tunnel] to change into her running shoes that was in the bag (in this case, the girl-doll, unlike the boy-doll, did happen to be carrying a satchel), and then went much faster". A 4 year old boy told me, in a condition which involved toy lorries instead of dolls, that the lorry in the shorter tunnel went faster taking into account that it had to stop for petrol ... etc.

Of course E arranges that no such interesting possibilities actually occur. And in this respect the tunnels task can be compared with the C.T. where it is also the case that, while E arranges that nothing relevant actually changes, he/she also arranges a context where various things — including those that would turn the wrong answer ("not same") into the right answer — could happen. The 2nd Empirical Note relates directly to the 1st Empirical Note, and makes the resemblance to the C.T. explicit. It is:- Between the events and circumstances constituting the first array asked about and those constituting the second array asked about, nothing relevant (or only irrelevant things) happen(s). The first question, Q1, is asked and, having been answered correctly, then asked again (as Q2). And while it is arranged that nothing happens to make a difference to how Q2 is answered, something relevant could have happened. The implications of this convergence and a suggested testable hypothesis as to how to empirically "turn failure...
into success" (i.e. applying optimist strategy 2) on the tunnels task, are mentioned in the final chapter.

Defending Matthews

In the first place, Matthews' speculation that the young child is likely to view the context in terms of a race receives appreciable support from the empirical evidence. More importantly he is to be supported on the grounds that Piaget's optimist critics are always to be supported. That is, he, like McGarrigle and Donaldson, makes the valid point that Piaget tends to load things against the young child getting the correct answer with his experimental design, and is likely to underestimate the child's intelligence by his "pessimistic" interpretation of those incorrect answers.

Criticizing Matthews

(1) In the first place, a simple empirical test, while not proving Piaget to be right, suggests that Matthews' explanation of the child's "same speed" response in the tunnels-on condition will not do. If we are to attribute a special concept, correctly employed, to the child it should at least show some degree of generalisability. Thus, in a comparable situation presented as a race, involving parallel movement of the racers and the asking of the same question, the child - on Matthews account - should (a) see it as a race, (b) view the question about relative speed to be one asking "who won?", and (c) base his/her answer on the concept of winning a race; i.e. either (i) which one reaches its goal first, or (ii) which one gets ahead and stays ahead.

I arranged what was explicitly a race between individual subjects and myself, from one side of a room to the other. The child started the race with "ready, steady, go", and after the race I asked "Did I go faster than you, or did you go faster than me, or did we go at the same speed?" We started simultaneously and I controlled my pace so that (a) neither of us ever got ahead of the other, (b) we arrived at the "goal" of the opposite wall simultaneously.

On Piaget's account of the results of the tunnels-on/off context we would expect either the perceptual intuition of arrival-and-stopping
"simultaneously" or that of "no-overtaking" to determine the child's response to the question. In this case the right answer ("same") would be predicted. There again, so would the wrong answer - that is, so long as we could think up an alternative and appropriate "perceptual intuition" that could have been in operation. However, on Matthews' account, with the application of the special concept of "winning a race" being even more encouraged than in the case of the Piagetian task, we could only predict the right answer.

What the majority of the 3-5 year old children said was something to the effect of "I did, because I ran and you only walked", the justification being as often as not elicited spontaneously. While the content of justification was correct (though I did have to walk pretty fast), its application to either a judgement about relative speed or one about who won the race obviously was not. Needless to say, it was not difficult to dig out the sort of direct "perceptual intuition" that could have been operating here: the child could feel that he/she was rushing and could see that I was not.

(2) Linked to this, we might say that Matthews does not give due attention to the explanatory scope of Piaget's notion of perceptual intuitions, and the way they may influence the young child's conduct. Piaget is to be criticized for (i) neglecting the fact that what perceptually strikes us provides the very occasion for acts of intelligence (including those of adults), and (ii) for applying his perceptual intuition explanation in a post hoc manner. But Matthews takes a too rigid and narrow view of what might be allowable here.

For example, he says that where one doll is seen to catch up another, but does not overtake, there would be no (supposedly) crucial perceptual intuition of "overtaking". On Piaget's account, he says, the young child should say "same" or "neither" to the question. The S actually tends to say that the one ahead goes faster. While this might be taken to support Matthews "conceptual" account, his reasoning against Piaget here is strange indeed! It implies that, in the absence of actual overtaking, the child would, for Piaget, be left without a perceptual intuition to influence (or "seduce") him/her. Of course all Piaget need, and would, do is invoke another perceptual intuition, e.g. that of one being ahead of the other. And if the child had said (correctly) that the one behind went faster, Piaget could invoke the "intuition" of "the one behind starting to overtake it", etc.
So, Piaget may be too hooked on the absence of concepts being correctly applied and on the presence of perceptually seductive influences in interpreting the young child's conduct (see Chapter 8). But Matthews, as a direct result of his perceived need to air his Intelligibility Principle wherever possible, becomes too hooked on interpreting conduct in terms only of the application of rational concepts, and gives too little attention to how the influence of contingent, perceived details might override the "appropriate" application of a concept, leading the child into what can only be described as error. However, we can understand and talk about such error because it happens with adults too. Yet this is no basis for withholding the attribution of rationality. In a way Matthews is succumbing to "intellectualism" even more than Piaget.

(3) I am now homing in on a direct attack on the "Intelligibility Principle", and thus on the application of what I have called the "first optimist strategy for turning failure into success". But, as the first of two final points before the nub of the criticism, we can note that Matthews in no way seriously challenges Piaget on the general design features of his tunnels-off/on task. My main criticism here was that the tunnels are arranged not to make a difference, but they could make a difference because all movement (and the crucial information associated with that movement) is hidden from view. Recognizing this fact we have to recognize that the task may not lend itself to being used to demonstrate either the absence or presence of any ability to reason about observed movement (i.e. relative speed or winning a race, etc). Through this "optimist" project to turn failure into success, it is implied that the only alternative to identifying an observed response as failure is to view it as a success. It seems to be overlooked that there are ways of describing conduct other than in terms of either "failure" or "success". And the fifth criticism (below) indicates that here we need to look for such an alternative. Indeed to use such terms already represents an interpretative move - i.e. away from description. This implies that the description of what the subject is actually observed to do (say "same" or "not-same" on either the tunnels- on/off task or the C.T.) necessarily represents a complete and adequate description of S's whole response to the episode. This is not the case, as there may be more to describe, and alternative aspects of S's observable conduct that may be emphasized.
(4) We may suspect that the leap from an account in terms of failure to one in terms of success may, in these contexts at least, represent an overreaction - even if partially justified by the fact that Piaget may be seen to "set himself up" through the combination of his task design and "pessimistic" interpretation (see above). And there is a very general symptomatic consequence. Piaget's developmental tasks, insofar as they seem to successfully discriminate between young children and, say, adults, serve as what can be viewed as a divisive ploy. They enable Piaget to talk about the former almost as if they were *pathologically deficient* in some way. By contrast, and through their (over) reaction to Piaget's pessimistic interpretations, a consequence of the optimists' strategy in general is that we find them adopting a *condescending* tone about the young child. We often come across condescension of the "in their own way they are complete, creative, little social individuals" type. And by showing that they remain preoccupied with explaining the observed "errors" in terms of some sort of failure (e.g. in communication ability), while praising Piaget for the ingenuity of his divisive ploy, it is not hard to spot the source of such condescension. This is the view that Piaget's tasks necessarily show that something interesting and of developmental significance - i.e. that any truly complete individual needs - is missing. But the final, main point suggests that where the Intelligibility Principle (or something akin to it) can be applied to a Piagetian test, it is probably not worth applying it. For the opportunity to apply it may suggest that there was, given the task design, never any significant cognitive developmental point to be made from the results of the test.

(5) The specific consequences of applying something like an "Intelligibility Principle" to turn failure into success

The first thing to note is that we are apparently licenced to apply Matthews' Intelligibility Principle to any instance of observed "regular and predictable behaviour". That is, through it we are enabled to argue that any stable response pattern is actually the product of the successful, rational application by the subject of a "special concept". Now, if we perform a tunnels task or C.T. with a newborn baby it is conceivable that we might predict that regular gurgling and dribbling behaviour will be able to be observed. But we would probably be unwise
to make the attribution of a special concept being appropriately applied.

The problem here is that we can always find some level of describing behaviour - and we may want to include the behaviour of "choosing to say nothing" here - that will result in it appearing "regular and predictable". Taking a more relevant example (see Chapter 6), if we investigate the responses of subjects over the age of three when faced with the questions of a Piagetian task, we can predict that the S will regularly display "attempting-to-answer-a-question" conduct. We may want to conclude that the subject is successfully applying the "special concept" entailing the recognition that "this is a context where someone asks a multiple-choice question, and I answer from any one of the available alternatives". But this does not tell us anything very useful about the subjects ability to understand that which the questions are intended to refer to.

This might suggest that the recognition by the investigator that the Intelligibility Principle can be applied to interpret behaviour in a given instance is worthless. While it should not do so, we must be wary of exactly what it does tell us in given concrete instances, like the tunnels task and the C.T., where it can be and has been applied. Simplifying matters, we can say that for these tasks there is a choice between the right answer ("not-same" in the tunnels task, and "same" in the C.T.) and the wrong answer ("same"; "not same"). The application of the optimists' strategy here tells us (correctly) that both right and wrong answers are compatible with the presence of reasoning ability. That is, so long as the S answers one way or the other - and does so regularly and predictably - the opportunity to apply the Intelligibility Principle (or something like it) tells us that, on the evidence of the answer given, the attribution of reasoning failure is impossible.

Certainly this allows us to conclude that erroneous answers may be too impressive in terms of the processing that generates them to be dismissed merely as the products of reasoning failure. By interpreting them thus, Piaget may be underestimating the child's intelligence. But the important point here is: what then can we say about right answers? For if, as seems to be the case, we have made it impossible to interpret wrong answers as reasoning failures, we are left only with potential reasoning successes (indicated by both right and wrong answers). But then the term "success", as well as failure, has surely become inappropriate or, at best, analytically useless. So, while not
challenging the appropriateness of invoking the Intelligibility Principle here, the consequences are that the notion of success has become so devalued that we must view the "successful" right answer as no better than the "unsuccessful" wrong answer. Now a reasoning task on which, in a straight choice, the wrong answer is as good as the right answer obviously cannot serve its purpose as a discriminating reasoning test. And examination of the two tasks in question should tell us what is wrong with them, and why we might need to look around for some way of describing the results other than in terms of the evaluative failure/success labels.

Leaving the C.T. till later, but bearing in mind that the main points here are all applicable, let us remember what was noted about the tunnels-off/on task. What is wrong with this task as a test of reasoning about movement and relative speed (or, for that matter, about winning races) is that in the crucial condition all information relating to movement (and details about the actual race) is hidden from sight. There is not enough information made available to the subject. It is this that makes it a hopeless reasoning task and allows us the freedom to interpret the wrong answer as representing a reasoning success if we are so inclined. The reason we can do this is because the wrong answer is as justifiable, as the right answer. But, in the absence of the required information, the only completely justifiable answer is "I don't know [who went faster]". In addition to the reasons why Doll 2 could have gone as fast, or faster than Doll 1 ("wiggly" road in tunnel B; Doll 2 stopped in the shorter tunnel [B] and then went much faster) it could easily have been arranged that the doll that appeared at the exit of tunnel A was not the one that entered the tunnel, but, for example, an identical one attached secretly to the stick and positioned already half way down the tunnel.

In such a case it is never too hard to think up reasons why the wrong answer could in fact have been the right answer. More interesting, and ultimately more profitable, is the consideration of what we need to assert is involved in a subject giving the right answer. With the placing of the tunnels obscuring relevant information and allowing for various interesting possibilities, the subject most likely to get the right answer will be the one who does not bother about the tunnels. The subject can get the right answer by ignoring or disregarding the tunnels, together with the corresponding interesting possibilities introduced. Far from calling for reasoning ability, it
would seem that negligence here could be the prerequisite for "success". This corresponds to the prediction that, where there is a choice between what has been arranged to be a right and a wrong answer, and where it is appropriate to apply the Intelligibility Principle to the wrong answer, the subject who gets the right answer will be able to be seen to do so totally unimpressively in cognitive terms. The last step here is to check what happens when subjects are seen to "fail" and when they are seen to "succeed" (if and when they do). What I noted was that (1) the subjects who give the wrong answer, seem to "fail" rather impressively (see above), and (2) the subjects who give the right answer seem to do so rather unimpressively (often inattentive and uninterested, unwilling or unable to give/think up justifications). Such observations are entirely in line with the above discussion.

Matthews, like the other optimists, seeks to turn the child's observed failure into success. And he achieves this through the application of his relativistic "Intelligibility Principle". But where it is appropriate to apply it, it rapidly seems to become inappropriate to talk of success and failure. That this conclusion is unacceptable to Matthews is shown by the fact that he remains preoccupied - like other optimists - with the interesting problem that Piaget's tasks seemed to pose: that is the problem of producing a progressive-developmental explanation of the young child's errors. He is not prepared to accept the conclusion that follows as a logical consequence of his argument - that the wrong answer may be as good and justifiable on these tasks as the right answer. In the end, therefore he turns back to invoking some sort of failure on the part of the child (e.g. in communication ability), to explain those results.

Coming back to Piaget's standard C.T., the conclusion is that, if an Intelligibility Principle, or something like it, can be applied in the service of the optimists strategy of "turning failure into success" - as it can for a task like "P" - then there is no point in bothering to apply it. If it can be applied then the first thing it suggests is that the task may be useless as an indicator of the ability in question. Matthews is interested in errors and might suggest that it is a step forward to show that the wrong answer is compatible with the ability being present, and therefore the ability in question might be present. I would respond that we already know that arriving at wrong answers, and making mistakes is compatible with the presence of reasoning ability. More specifically, of course, reasoning ability might be present in the
young child. That was where we started and was, ostensibly, the reason we - and Piaget - needed a good test to decide whether it was or not. The ease with which the Intelligibility Principle can be applied, not to mention an examination of the features of those tasks to which it is applied (Piaget's tunnels-off/on task and standard C.T.), merely suggests that these tasks are bad and are not likely to be able to do the work required of them.

However, the issue of explaining wrong answers on such tasks is a fraught one, and I will be more interested in right answers. The value of this discussion of the "first optimist strategy" may be in that it gives us a hint of what to expect when we consider those versions of the C.T. where the right answer is elicited from the young child who gets Piaget's standard version wrong. And this takes me to the "second optimist strategy" for turning failure into success, and - once more - to their modified C.Ts.

(b) Second optimist strategy: keeping the "form" of the task the same, alter the "content" to turn failure into success

As in the approach discussed above, those adopting this strategy argue that the young child's wrong answer on a particular Piagetian task may not represent a reasoning failure, being compatible with the presence of the reasoning competence in question. Thus the implementing of the "first optimist strategy" for viewing failure as success tends to be an initial phase of their enquiries. But, rather than a dogmatic application of an "Intelligibility Principle" through which it is argued that we must reinterpret the child's "regular and predictable" wrong answers in terms of some sort of reasoning success, this group of researchers attends to and relies on additional empirical evidence. They seek to demonstrate that the child can do the reasoning involved. Thus the crucial move lies in actually obtaining the right answer on the same sort of task as the original, Piagetian version that elicited the error.

Here every attempt is made to retain the "form" and logical requirements of the original task. At the same time the content is modified in such a way as to ensure that what is suspected to be the deceptive component, allegedly "loading" the results towards failure in
Piaget's versions, is as far as possible eliminated. Their claim is that the standard versions achieve their results through being artificial and unfamiliar to the child. Thus these optimists help the child to actually get the right answer by adjusting the content and procedure by means of which it is presented, in a way that makes it "more meaningful" and "more familiar" to the child. If the subject has the rational competence in question, he/she will be more likely to exhibit it in a context that is "natural". If/when the desired success result is obtained from the child who failed on the original version, this allows the investigator to return to the latter result and offer a full explanation without invoking reasoning failure.

There are many illustrations of the application of this strategy other than the plethora of examples of modified C.Ts, some of which were noted in Chapter 3. The following two examples spring to mind:-(a) Hughes (1975) used a modified context to challenge Piaget and Inhelder's (1956) "mountain task" results (see Donaldson, 1978, pp. 19-25). Young children commonly fail on the latter, allegedly through an egocentric inability to take up another's point of view of a given situation. But even the youngest children tested tended to get the modified "policemen" task right, and Donaldson (pp.23-24) points out:

[The 'policemen'] task ... is in some ways very like the 'mountains' task ... [but in the latter] it is difficult to avoid the conclusion that the children ... do not fully understand what they are supposed to do. By contrast ... in the 'policemen' problem, a situation has been found that makes sense to the child ... The point is that the motives and intentions of the characters are entirely comprehensible, even to a child of three. (her emphasis)

Here for a start then the modified version is seen as having the same form and logical requirements, but having more meaningful and familiar content.

(b) McGarrigle, Grieve and Hughes (1978) obtain success on their modified Class Inclusion experiment, where Piaget (1952) obtained failure. In an example of the original version, Piaget puts before the child a row consisting of, say, 4 black toy cows and 2 white ones, and asks the question "Are there more black cows or more cows?" The modified task differs only in that all the cows are laid on their sides ("asleep"), and the question asked is:- "Are there more black cows or more sleeping cows?" It would seem even harder here to dispute that the
two compared versions have essentially the same "form", and again the point made is that the modified task is more meaningful and less potentially misleading. And the child, possessing the competence in question, is enabled to show that he/she possesses it. (See final chapter for further discussion and critique of both versions.)

Coming back to the C.T., the crucial adjustment with respect to "content" is seen by the optimists to lie in emphasizing to the subject that the transformation event is incidental to the episode as planned by the adult [E].

In the previous section I showed that the "first optimist strategy" faces a problem in applying something like an "Intelligibility Principle". Where there is the choice between a right and a wrong answer, the attempt to reinterpret the child's "regular and predictable" error as a reasoning success encounters the following dilemma. On the one hand, (a) they end up seeming to have to acknowledge that the child's wrong answer does represent some sort of failure. In this case they have to convince us that the failure - e.g. in failing to understand E's question - does not reflect a fundamental intellectual incompetence. And Piaget, with his strict criteria, defends himself quite well in this point. On the other hand, (b) they seem to end up having to admit that it may become illegitimate to talk of any sort of failure on the part of the subject here. And, with the attribution of failure becoming impossible, it would seem that it is also wrong to assume that either right or wrong answer necessarily represents some sort of "successful" understanding.

Thus the optimists still have to convince us (1) that the right answer reflects some sort of achievement (and we saw in the illustrative example chosen by Matthews, that the evidence suggested the opposite), and (2) that there is some sort of failure (e.g. in linguistic/communication ability) still to be explained. The dilemma reappears when we critically appraise the second optimist strategy.

**The optimists' cleft stick**

We might have made the straightforward inference, from the fact that "0" yields the right answer ("same") from the same representative
young child who gets "P" wrong (saying "not-same"), that (a) the child
can do the required reasoning and that therefore Piaget's interpretation
is wrongly pessimistic, and, (b) "P" is an artificial context and a bad
test of reasoning competence, while "O" is less artificial and therefore
a better, more sensitive test. But I said enough in Chapter 3 to
suggest that things are not as simple as this, and that such "straight¬
forward" conclusions leave various questions unanswered.

Again the optimists face a two-pronged attack. On the one hand, it
might be disputed that "O" is the same sort of task as "P". In this
case the result of "O" may not, after all, be able to tell us anything
useful about the "P" result. Thus we would be no closer to resolving
either the question of whether or not the young child has the logical
competence to get "P" right, or that of why he/she gets "P" wrong.

On the other hand and more significantly, since I am taking both
versions as bona fide C.Ts, we may want to accept the optimists' claim
that "O" is essentially the same sort of task as "P", the key difference
lying in the result obtained. But the optimists project here is based
on the claim that "P" is a bad test of the child's reasoning ability.
So now they have to convince us THAT "O" IS NOT ALSO A BAD TEST, AND FOR
THE SAME REASON - NO MATTER THAT THE OPPOSITE RESULT IS OBTAINED.

In general, what initially was seen to be wrong with "P" was that
there is a choice between a right answer ("same") and a wrong answer
("not-same"), but both right and wrong answers are compatible with the
presence of reasoning ability in the child subject. However, the very
fact that we are able to apply the first optimist strategy - or
something like the Intelligibility Principle - for turning failure into
success meant that, in the end, we were left only with what we were
obliged to view as success. But this devalues the notion of success in
relation to the possible results of "P". And it implies that, while the
wrong answer may look cognitively too impressive to be dismissed as
failure, the right answer may look, in cognitive terms, very
unimpressive. At the moment we only have the failure on "P" of the
representative 4 year old to consider. But if this C.T. turns out to
resemble the above tunnels-off/on task, as I suggest it does, then
examination of the way "P" achieves either of its possible results
should confirm this. This, in turn, will suggest that both right and
wrong answers on "P" are compatible with both the presence and absence
of reasoning ability. In this case it may be totally inappropriate to
talk in terms of "success" and "failure".
Now we do have elicited right answers on "O". And the claim being considered at the moment is that "O" is essentially the same sort of task as "P". From a consideration of the - albeit inadvertent - consequences of the optimists' approach to the results of "P" alone, the prediction here then is that "success" will not look much like cognitive success. And, if nothing else, what the "false positive" charge tells us (see Chapter 3) is that the manner of the child's success on a modified task like "O" may be totally unimpressive. If sufficiently unimpressive, the right answer would not be able to viewed as the product of any sort of reasoning competence and would be compatible with the absence of reasoning ability. We await on further evidence as to what is and what is not involved in getting the right answer on "O" (see Chapters 5 and 6). But at the moment it is beginning to look like the claim that "O" is essentially the same as "P" may be justified, and thus what can be said about one also applies to the other.

At this point, therefore, the following strong statement is suggested: Both right and wrong answers on both "P" and "O" are compatible with both the presence and absence of reasoning ability. If so, and if it applies to any bona fide version of the C.T., then we may suppose that something about the nature of the C.T. makes it not just a bad test of reasoning ability, but the worst possible.

E. From What a C.T. Cannot Show, to What it Can Show: a "Null Hypothesis" and the Role of Deception

The main issue addressed here seems to relate to what C.T.s cannot show. But, in accordance with the "challenge" that gives this chapter its title, we still need to address the question of what any C.T. can show. Correspondingly, we seek a minimal level of explanation as to what determines whether any subject will fail or succeed on any C.T. (Question C), if this is not to be the presence/absence of reasoning ability on the part of that subject. At some point empirical evidence will be required. In the meantime we can continue to attempt to characterize the crucial feature(s) of the C.T. Only then will it be possible to suggest how it achieves its results, if is no longer to be viewed as a reasoning test.
(a) A "null hypothesis"

It would seem that an answer to Question A, (why does the young child give one answer on "P" and the opposite answer on "0"?) must be in terms of some critical difference between "P" and "0". But, from what has emerged from Chapters 2 and 3 and what has been said so far in the present chapter, I prefer to switch the emphasis of the investigation to what they (and any bona fide C.T) have in common. Thus I suggest that:

(a) there seems to be something wrong with each task when considered separately; but,
(b) we might expect that what is basically wrong with one task will be what is basically wrong with the other - even though they generate opposite results; and that
(c) the different versions of the C.T. considered achieve their different results in basically the same way.

Going to the extreme, it is now possible to advance a "null hypothesis", stating that there is no essential difference between "P" and "0". This "hypothesis" is not only obviously wrong, assuming that the tendency to give one answer on one version and another on the other reflects some crucial difference in the contexts, but it is also somewhat fictional - at least in the form of an explicit hypothesis supposedly having guided my initial enquiry. But it provides a useful device in laying out my argument.

A consequence of this, and a crucial step forward in the investigation, is that we see that it may not be justifiable - or at least may not be very useful - to consider the right and wrong answer on a C.T. as "opposite".

What the young subject (or his/her cognitive apparatus) makes of the episode and what is asked is, of course, the central psychological question in terms of explaining the various observed results. From the investigator's point of view, the subject tends to be seen as faced with the task of choosing between the right answer ("same") and the "opposite", wrong answer ("not same"). What was intended by Piaget to make the difference as to which answer is chosen is the presence of a reasoning ability. But we are coming to the conclusion that the C.T. (i.e. both "P" and "0") may be a hopeless test of reasoning ability. In this case (a) the distinction made between "right" and "wrong" answers may only have limited usefulness; and (b) we need to look beyond the subject's reasoning competence for a factor that might help explain
his/her answer. The focus now will be on properties shared by C.Ts., rather than on properties shared by the minds of young children.

What seems to be wrong with both versions is that while nothing relevant happens (see 1st empirical note), the subject can never have enough information to know this. It is undisputed — indeed intended — that the required information to answer Qz is not to be found in the post-transformation array (though note problems with number C.Ts where counting is possible). The required information is intended to be in the preceding events and circumstances. My claim is that this information is in fact also insufficient, whichever version is considered and however the transformation is effected. More specifically, and focusing on what is seen as the significant event: while the transformation is arranged so that it does not make a difference (like the tunnels in the tunnels-off/on task, above), something could have happened whereby it (like the tunnels and what they may hide) would make a difference to how the question is to be answered the second time it is asked. In this case there may not be enough information available for the subject to answer on purely rational grounds — with any response other than "I don't know". But this is not one of the alternatives offered to the subject.

We can bear in mind the context, the information available and the information withheld from the subject. On the one side we have the rationally faultless "I don't know". On the other there are the equally unjustifiable — and therefore, in rational terms, equally wrong — answers "same" and "not-same". It thus becomes apparent that the "psychological distance" between giving the correct ("same") and the incorrect ("not-same") answer may be small. At least it may not be sufficient to usefully talk of these available alternative responses as being opposite.

We might still want to argue whether it is as justifiable for a subject in a C.T. to suppose that something relevant happened (to make the answer to Qz "not-same") as for him/her to suppose that nothing happened (and that the answer to Qz is "same").

However, (a) the fact remains that, in "P" it is not just that the E's actions/words might suggest that there are interesting possibilities whereby the answer to Qz could be "not same". There are such possibilities; (b) Given the aim of the optimists to retain the essential form of "P" in "O", there are the same "interesting
possibilities" (plus, perhaps, a few more when rampaging teddies, etc. are introduced) in the latter; (c) blatantly misleading elements do exist in both tasks. We can confirm that saying "watch carefully" etc., in "P", is likely to suggest that something relevant will have happened, and we only have to look at "C", or a task like McGarrigle and Donaldson's "Naughty Teddy" version, to see that deception is still a key feature, with the child intended to believe that the transformation occurred "accidentally/incidentally", when it patently did not.

If it is found that it is impossible to exclude a potentially deceptive component while retaining the form of a bona fide C.T., then the subject has every right to (correctly) take the view that a deception is involved, and that the events and circumstances might be other than they appear to be. (How they do actually appear is a key question still to be addressed.) This is simply because things in a C.T. always are, and are intended to be, in some way other than they appear. The crucial, shared deceptive component common to all C.Ts will be spelt out below.

A successful deception of course goes undetected. But the point here is that a subject of a C.T. is always justified in suspecting that deception is involved (simply because it is) - even if he/she does not know what sort of deception to expect, or where it occurs. A consequence of this is that even the most rational subject is always justified in giving the opposite answer to that which he/she may have arrived at by the most faultless logic, applied to the unhidden events and circumstances of the episode. As long as deceptive components that must be inherent in any C.T. can be identified, we can view it as confirming that the task is a useless test of reasoning ability.

While raising the issue of deception rounds off the discussion about what - significantly - C.Ts cannot show, this also marks the take-off point for addressing what they can show. Thus I now turn to a consideration of the role of deception in relation to the question of how C.Ts may, rather than may not, be taken to achieve their results. This is irrespective of whether the result obtained is a non-conserving "failure", or - in the end, more interestingly - a conserving "success".
(b) The role of deception

The main point made by the optimists' critics in relation to a task like "0" corresponded to the "false positive" charge. That is, getting the right answer here may not involve reasoning ability. On the other hand, the main point made by Piaget's critics in relation to a version like "P", and the basis for the initial "false-negative" charge, was that the task may be unfair and loaded against the young child. That is, it is claimed that the non-conserving result here is obtained by deception.

Applying the "null hypothesis", the following assertions are arrived at:

(a) Neither "0" nor "P" require reasoning ability to get the right answer.
(b) Both "P" and "0" not only involve deception, but achieve their - albeit "opposite" - results in the same way and through the same deception. That is, they share a common, crucial source of deception, merely by virtue of being both bona fide C.Ts.

The first point has been discussed above. So let us address the second point - not just to confirm that the C.T. is a useless test of reasoning ability, but also as the first step towards providing a level of explanation of the C.T. results.

(c) Towards identifying the critical source of deception common to both "P" and "0", and all bona fide C.Ts

It was noted that the essence of a C.T., as arranged, is that nothing relevant actually happens. At the same time it is arranged that the subject never has enough information to know this or work it out. This leaves the only truly rational answer to Qa as "I don't know". Yet this is not one of the offered alternative responses; and it is seen that subjects, whatever their age and presumed logical competence, rarely answer in this way. It would therefore seem that all subjects may be somehow deceived into believing that they have enough information about the situation to give one or other of the answers.

However, (a) we still need to characterize what sort of a situation it is for the subject, and what information he/she can be assumed to, and be seen to, attend to. Further (b) we are committed now to viewing
the C.T. not merely as one where there is not enough information, but also as one where some of the information present is misleading. Now if a central diagnostic feature of a C.T. is that nothing happens, we might explore the possibility that all subjects are deceived in relation to this. And this looks promising, as we might expect that a subject, at the start of any situation like a C.T., presumes that something - and something relevant to what is going to be asked - will happen. What I suggest then is that any C.T. can be described as: an episode where nothing happens, but where the subject not only does not and cannot know this, but - on the contrary - is deceived into wrongly supposing that something will happen. Further, this deception can be seen to be directly involved in determining his/her answer to Q2.

In terms of accounting for failure on any C.T. (taking "P" as the example) this does not seem particularly contentious. We can see that the subject, expecting something to happen, might suppose that something had happened that calls for a different answer to Q2 than the one already given to Q1. The difficulty is that there are various things ("interesting possibilities") that could have happened to change the answer to the question at its second asking, and thus various possible sources of change in circumstances that the subject could attend to and be influenced by. Thus there are several alternative explanations of why a particular subject in a particular version of the C.T. might be deceived into giving the wrong answer; e.g.:

(a) S might decide that what has happened is new evidence that suggests that the initial amounts, in containers A and B, were not, after all, the same, and that Q1 was answered wrongly; or (b) S might believe that what happened was that something was added or taken away during the transformation, or that (c) something related to the nature of the stuff being used happened - perhaps through being poured (see example in Chapter 7) - to make the right answer "not same", etc, etc. We know from the literature (see chapter 3) that a plethora of explanations of failure emphasizing a deceptive component, have indeed been offered, and that, given the unresolved confusion here, it is liable to be unproductive to continue on this tack.

More puzzling and interesting is the question of how a subject, through expecting something to happen, could be deceived into the right answer. And this may turn out to be the more productive line to pursue - if only because the subject who answers correctly, and believes he/she has enough information to do so, presumably does not think that anything
has happened. At least, therefore, we do not have to consider here the actual different possibilities held out by particular versions of the task. This is because the subject, in successfully answering \( Q_2 \), is patently not sufficiently influenced by any of the alternative possibilities to change his/her answer. But we want to know what a subject who gets the right answer is influenced by. Why does a subject who - like the failure - expected that something would happen, correctly, but without sufficient information, not believe that anything relevant happened. It can be seen that the particular problem faced here is that of reconciling how a subject who, on the one hand, correctly does not think that anything relevant happened, is, on the other hand, to be viewed to be deceived into this; and to be seen - like the non-conserving failure - to be deceived through initially expecting that something would happen.

I will show that the problem posed here is not as paradoxical as it might appear. Far from the attempt to resolve it being profitless, it takes us another step towards arriving at a general level of explanation of why any subject succeeds or fails on any bona fide C.T.

The direction that the investigation must take, including its empirical aspects, is marked out. Thus it is necessary to move away from the preoccupation with explaining C.T. failure (wherever it occurs), towards a concerted attempt to understand what exactly is involved in C.T. success (whenever it occurs). In the end, therefore, just as the main value of the "false negative" controversy lay in the fact that it gave rise to the "false positive" controversy (see Chapter 3), so the main value of the criticisms by the optimists of Piaget's "loaded" standard tasks lies in the fact that through them we are provided with versions of the C.T. where the subjects that I am primarily concerned with (3–5 year old children) do succeed. And we can now proceed to look closely at the manner of this "success". Amongst other things, this is to determine - given the doubts that arose above through examining the logical consequences of the optimists' approach - if or to what extent the term "success" is ever an appropriate term for describing the right answer in a bona fide C.T.
With the focus on "P" and "O", I turned from the fraught question of what particular versions of the C.T. do show, to that of what any bona fide C.T. can show - that is, irrespective of (a) the actual result obtained, (b) who/how old the subject is, as well as (c) the version of the task employed. Thus the emphasis is on the features that all C.Ts share, rather than as in Chapter 3 - supposedly crucial differences between versions.

Viewing the initial problem now as one of description, not interpretation, the sort of questions to be posed are as follows. How are we to describe the essential nature of a C.T.? And what will this entail when we eventually come to interpret the result obtained?

The first impression from looking at C.Ts in operation was that there is something ineffably odd about these episodes. I suspected that the source of this strangeness lay largely in the fact that the essence of any C.T. is that nothing relevant happens (see 1st Empirical Note). And a consideration of the two "optimist" strategies for turning failure into success reinforces the suspicion that a context with this feature may not make a good test of any reasoning ability.

It was additionally noted that there was a lack of information. In both "P" and "O" there is not enough information in the final array alone for the subject to work out the right answer to Q. The required information is supposed to lie in the preceding events and circumstances. But, while the essence of both tasks is that nothing relevant happens, it is the case that something relevant could have happened. In other words, there is not enough information here either. We might continue to insist on trying to assign a role for reasoning ability somewhere in the C.T. But the only option left to us now is to see the reasoning component as lying in the correct answering of the preliminary "establishing equivalence" question, Q, from the information available. However: (a) even here, in tasks like "P" and "O", there is not enough information and thus various reasons why the answer might not be "same"; (b) neither the "pessimistic" nor "optimistic" participants in the dispute would want to claim that this preliminary phase is the crucial point where reasoning is called for; accordingly, (c) the answer to Q is always assumed to be correctly
known, the bona fide C.T. in fact being unable to take place unless the right answer to $Q_1$, is elicited, assumed, or given to the subject.

The conclusion that therefore follows is that: both the right and the wrong answer on both $P$ and $O$ are compatible with both the presence and absence of reasoning ability. This suggests that the bona fide C.T. is not just a bad test of reasoning ability, but the worst imaginable. Given Piaget's theoretical rationale, we might start to have doubts about his acclaimed "experimental ingenuity" at this point.

But the C.T. is "ingenious" insofar as Piaget obtained the results he saw himself as needing. Thus we still want to know how his standard tasks achieve their failure results, as well as how "modified" C.Ts achieve their success results. It is now noted that C.Ts are not to be characterized merely by their lack of information; they also contain misleading information. in other words we can try to explain the respective failure and success performances elicited as the products of deception.

I again employed the "null hypothesis" (the assumption of no difference between $P$ and $O$) to point out that we may need to question whether it is appropriate - or at least useful - to view right and wrong answers on the C.T. as "opposite" in terms of their psychological significance. And - persisting with the hypothesis - this paved the way for the following inference concerning the role of deception. If we (a) go along with the optimists and see the failure of the young child (on $P$) as a product of deception, and if (b) we accept that $O$ is essentially the same as $P$, then (c) we may be entitled to suppose that both tasks achieve their results in the same way, and therefore to (d) view the success of the same representative child (e.g. on $O$) as the product of the same deception.

Now, supposing the key property of a bona fide C.T. resides in the fact that it is arranged that nothing relevant happens, the following suggestion was put forward. Given that we can assume that all subjects in a task/game like the C.T. episode expect, at least at the start, that something having some relevance to what will be asked, is going to happen (or might already have happened?), then we might anticipate that here lies the sought-for "universal" source of deception that characterizes the C.T. This might be able to be seen as the source of both failure and success results.

The problem that remains is how to explain the elicited right answer (when it occurs, on any C.T.) as the product of this deception.
At this point what is required is a rigorous attempt to understand what exactly is involved in *success* on any C.T. Acknowledging the confusion and controversy that surrounds the interpretation of both failure and success (see Chapter 3), this is the more legitimate way round. For until we are sure what is meant by the term "success", we cannot be sure that we are justified in equating the wrong answer with "failure". If not, we cannot be sure we are asking the right questions about either sort of result.

The last three chapters all contributed to doubts as to widely held assumptions about (a) the nature of the C.T., (b) wherein lies its interest, and (c) whether we are asking the right questions. With "Challenge 1" I have made the first step to clarify what can and cannot be asked of the C.T. And what has emerged is the suggestion that any subsequent explanation of C.T. results should, at least in the first place, view both performance "failure" and "success" as a product of deception against a background of lack of information.

The empirical evidence that I draw on in the following chapter is intended to provide support for this claim. And by allowing a better understanding of what is involved in success on any C.T., it moves us a step nearer a parsimonious level of explanation of all C.T. results.

**NOTES**

1 The answer to Question C finally emerges in Chapter 6, section G (b).

2 It was this fact, and the remarking of the close relationship between the approaches of Matthews and McGarrigle and Donaldson (1975) that in fact drew me to a consideration of the C.T. in the first place. My research project had begun with a focus on tasks used to investigate the young child's understanding of relative movement and speed.

3 Indeed it is quite possible (see Chapter 7 for an example) that there is not the same volume or amount in containers A and B at the start.
CHAPTER 5

CHALLENGE II - FROM THE PREOCCUPATION WITH EXPLAINING INTERESTING FAILURE, TOWARDS UNDERSTANDING WHAT IS INVOLVED IN SUCCESS.

A. Introduction

In the previous chapter, the emphasis has been on describing what the investigator does in arranging that a C.T. takes place. The main point that came out was that he/she arranges an episode where nothing relevant happens, but where something relevant could have happened. As such, it does not look like a test of reasoning ability. So how is the C.T. to be viewed to achieve its result, whether this be success or failure? I pointed out that, besides involving a lack of information, there is misleading information present. I am therefore committed for the moment to view either sort of performance on a C.T. as the product of deception against a background of lack of information.

In this chapter I attempt to shift the emphasis to the question of what the subject does in participating in and responding to a C.T. In particular it was seen that we need to understand how a successfully conserving subject "succeeds" on a C.T., given the properties common to any bona fide version. This is found to present a greater challenge - at least at the general level of explanation sought - than the case of failure. The reason for this is that I am also committed - through the "null hypothesis" - to seek an account that tries to view both types of performance as the product of the same deception and the same lack of information.

The value of having broadly considered C.T. failure is therefore now to be seen in the outcome of suggesting where to start looking for a specific factor, independent of reasoning ability, that can bring about successful performance on a C.T. And the value of the optimists' project lies in the fact that it has given us examples of C.T. success to consider - e.g. with task "O". But, in the light of the generality of the problem posed by Question C (why does anyone succeed/fail on any
C.T.?), it is not necessary to confine the investigation here to "modified" C.Ts, nor to young children - nor, indeed, to performance alone. And in the presentation of relevant empirical evidence below it will be seen that, alongside the shift of emphasis from failure to success, there is a corresponding shift of attention from what children do in C.Ts, to what adults do. At the same time there is a shift away from the performance (success/failure) aspect, to other features of observable conduct associated with the C.T. episode.

A note on the selection of subjects

This chapter concentrates on some empirical evidence concerning a comparison of how successes "succeed" with how failures "fail". And a word of warning about the evidence obtained from the C.Ts is in order. I have made much of the essential feature of a C.T. being that nothing significant happens. Even though it is always the case that, by the time the final question is asked, something interesting could have happened, this feature means that there is always the danger that, as a "game" that the young child is invited to participate in, it will seem rather dull compared with other games that the child will be familiar with. As such, it is always possible that the subject will not be very interested in the events and circumstances arranged; nor in the questions that refer to those events and circumstances, and the answers he/she gives to those questions. And if the subjects concerned had happened to have been involved in such tasks, or tasks like them, before - as was often the case for my samples taken from the university psychology department nursery - there is additional reason to expect an uninterested or blasé reaction from them.

There was evidence that this was sometimes the case - irrespective of which version was employed and whether the subject ended up getting the right or wrong answer on Q₂. One symptom of this state of affairs is that the child frequently makes a game out of some aspect of the episode. For example the subject may make E pour the contents of the beakers (in "P" and "O") back and forth at the "establishing of equivalence" phase; and one sometimes gets the impression that this would go on ad infinitum if pressure is not applied to induce the child to agree to say that there is the "same in each" container (A and B).
In terms of its arranged, essential elements, of course, the C.T. does make a minimal episode, and has the ingredients of an inordinately dull "game". The subject has every right not to show an interest in the aspects that E requires him/her to attend to, every right not to bother relating his/her answers to what is "actually" and visibly going on in relation to the materials in front of him/her, and the E's manipulation of them. Also, the subject has every right to be casual about the questions asked, to have little faith in the answers he/she gives and to believe that one answer is no better than another.

However, every interpretation of any reported C.T. result, to have had any interest for cognitive developmental psychologists (whether "pessimistically" or "optimistically" inclined), has had to start from the assumption that the subject is bothered enough to try to answer in a correct or approved way. We might then suppose that they at least have dealt with subjects who have been co-operative in this respect. I am therefore justified - if I want to continue to address their findings - in setting out to do the same. I therefore refer only to those subjects whom I considered satisfied these criteria.

Obviously if such selective sampling resulted in the disregarding of a greater proportion of subjects doing "P" than doing "O" (or vice versa), or a greater proportion of failures than successes (or vice versa), it might have biased the findings. Fortunately this was not the case, neither the version used nor the result obtained being a predictor of "unsatisfactoriness". So, while it may well be significant that a number of subjects are inattentive in, and casual about their response to, a situation where nothing happens, I felt at liberty to choose as my sample of successes and failures those whom I decided were sufficiently bothered about their answers.

B. A Further Crucial Lack of Information: the Temporal Aspect

I have suggested that a key and universal source of deception may lie in the fact that, at the start of any C.T., it can be presumed that the subject expects that something relevant will happen, while E has arranged that the opposite is the case. The immediate problem is to identify the particular lack of information that allows this particular
deceptive component to have as its consequence the eliciting of the right answer.

If a deception is to be invoked to try to account for instances of "conserving performance": firstly, a mistake (or incorrect belief) on the part of the subject must be identified - and one that could be directly involved in the process of coming to the answer "same". Secondly, it must be able to be viewed as a mistake that could be made by a potentially rational subject. This is because "same" is the right answer and we would want to avoid ending up trying to argue that the successful generation of the right answer is evidence for a rational failure - at least until we knew more about what is involved in correct responding.

In relation to these points it can now be noted that the subject, expecting something is going to happen, does not know when "it" is going to happen. Not knowing what the intended "point" of the episode is, he/she cannot know when to expect it and, more generally, does not know how long the episode will last.

This allows us to view the right answer as the product of the posited deception. For, if the subject does not know how long the episode will last, then he/she might be seen as saying "same" (or "still the same") while still under the influence of the original deception that something will happen. That is, it becomes possible to argue - so long as appropriate evidence is forthcoming - that the subject says "same" because he/she is (a) initially tricked into believing that something relevant will happen during the episode and is (b) additionally somehow tricked into believing that the episode will last longer than it actually does. The effect of the combined deception would be to trick the subject into believing, as he/she answers Q2, that the anticipated "something" lies ahead. Believing for this reason that nothing relevant has happened, an answer of "same" to Q2 would be the likely outcome.

The inference here would be that non-conservers are not tricked in the additional way, and would be likely to hold a correct belief about the likely duration of the episode. Also, it would now be predicted that removing the temporal deception component will turn would-be "conservers" into "non-conservers".

This latter aspect will be dealt with later. In the meantime the initial empirical project required is clarified. Three points need to be confirmed:
(1) that subjects of bona fide C.Ts start off expecting that something significant and relevant will happen, without knowing when.

(2) that this belief is the product of a feature of deception and withheld information common to all C.Ts.

(3) that successful subjects answer the second question, Q2, correctly while (incorrectly) believing that the episode is not finished and that something relevant will happen.

These points are followed up in the next section.

C. Evidence from 3 - 5 Year Old Subjects (see also Appendix Ia)

(a) Success on "O"

3rd Empirical Note:

(1) Subjects participating in "P" and "O" asked at the time and/or interviewed later, did start off expecting that something interesting to do with the containers and contents on the table in front of them would happen. They were not clear as to what to expect, a variety of opinions being offered; e.g. (a) the liquid in the containers was going to be drunk by us or someone else (on one occasion, when the C.T. followed the above-mentioned tunnels task, it was "for the boy and girl [dolls] to drink after their race"), (b) "you're going to magic it". Another child suggested I was going to make the water turn blue (apparently he had seen a trick like this at a birthday party), etc. When opinions were not offered there was still no doubt that they expected that something significant would happen/change. This at least was the case for the selected subjects.1

(2) Subjects for tasks "P" and "O" are invited to "come and play a game" or to "come and do something interesting", etc. This can be presumed to play a role in influencing them to (wrongly) believe that something interesting/significant is going to happen. In addition, subjects are not told how long the episode will last, how many questions will be asked, and how they will recognise when it has ended.

(3) Task "O" provides the chief source of successfully performing 3 - 5 year olds. Starting with these subjects it is found that they tended to express surprise and/or confusion when I (the experimenter) terminated the episode with something like "Thank you, that's all", and
started to put the materials away. At the point where Q_2 is answered, actions, continued attention to the array, facial expression and tone and style of response likewise suggested that these "conservers" were waiting for something. These aspects of conduct contrast with those of "non-conserving children (see below) and were able to be closely examined using video recordings. Together with subsequent discussions with the subjects, this further suggested that it was not so much that they were, at Q_2, still expecting more to happen. Rather they were waiting for something to happen, and for the game/episode to start. This is the result that Bovet et al (1981), also working with a version of Light et al's (1979) modified C.T., reported for successful children.

The conclusion here is that the representative 3 - 5 year old participating in (and succeeding on) "O" is deceived into being left, at what turns out to be the end of the episode, still waiting for something to happen. And this is made possible by the fact that the subject has no way of knowing how long E intends the episode to last.

A comparison with the representative young child's conduct on "P", where he/she fails, is called for. This shows that the subject here is also led at the start to believe that something interesting/significant is going to happen, without knowing when. However, the effect of the events and circumstances this time seems somehow to be to make the subject believe, at the point where Q_2 is asked, that the episode may be over. In this case the subject - presumably believing "it" has happened - can be predicted to be (a) less likely to answer as a conserver, and (b) less likely to be surprised/confused when E terminates the episode after the answer to Q_2 has been elicited.

Thus the evidence from subjects' actions, manner of attending to the array, facial expressions, and tone and style of the delivered (failure) answer, all suggested that here we are not dealing with a "waiting" response. The subject, in response to E's final question (Q_3), is frequently observed to sit back with his/her attention moving away from the array, smile confidently at E, and - often triumphantly and with some degree of finality - deliver the "not-same" answer. In addition, such a representative subject in "P" rarely seems in the least put out when E then proceeds to terminate the episode.
It is conceivable in the above, however, that the reported differences (see point (3) above) are a consequence of the differences between task "O" and task "P", and not directly related to the fact that in the one case the success result and in the other the failure result tends to be elicited. Fortunately we are always still left with some instances of failure on "O", and, more importantly here, success on "P". So this can be checked. And it turns out that it does not matter much which version of the C.T. is employed. It is the fact of success or failure that is the important predictor as to the above observable features of the young child’s behaviour on being asked Q=2 and, crucially, on encountering the E’s "termination of the episode". Thus, so long as examples of failure and success are forthcoming (the same child may give us both right and wrong answers - in either order - in response to the same task done on different occasions)*, the same, anticipated pattern of results is obtained. That is, successes on "O" or "P" succeed while (through?) being deceived into incorrectly supposing, at Q=2, that something is going to happen; failures on "P" or "O" fail while (through?) being left at the end correctly not waiting any longer for something to happen.

(c) The posited deception as the "cause" of both right and wrong answers?

At the moment we just have correlation. On the one hand we have successful performance accompanied by what we can identify as "waiting conduct". On the other hand there is failure occurring in the absence of "waiting conduct". What can so far be said is that a "causal" explanation of the sort outlined in section (B), and featuring in both cases the key posited deception, is not refuted by the available evidence.

Thus we can now see how the combination of the initial deception - whereby the subject is misled into expecting that something relevant will happen - and lack of information as to the duration of the episode could explain both successful and unsuccessful performance on a bona fide C.T. In the one case S is tricked into wrongly anticipating, at
Qi-, that the expected but actually non-existent "something" is about to happen - in which case it cannot have happened. In the other case S is tricked into wrongly believing at $Q_2$ that "it" has already happened.

But there are plenty of possible objections to, and indeed much that is wrong with, this "explanation" of failure and success. The most obvious and relevant objection at this point is that it may have got things the wrong way round. That is, we can accept the correlations - that success goes with waiting for more and failure does not - but could suggest the following: (a) It is not the subject mistakenly supposing at $Q_2$ that he/she is in an episode that will continue that leads him/her into supposing that nothing relevant has happened prior to $Q_2$. Rather it is the (correct) belief that, up to $Q_2$, nothing relevant has happened that leads this subject to wrongly suppose that the episode will continue; (b) It is not (correctly) supposing that the episode is, at $Q_2$, now over that leads a subject to believe that "it" has happened already. Rather it is the mistaken belief that something relevant has happened that leads this subject to correctly believe that the episode will be (or can be, without causing any surprise) terminated by $E$.

However, three points can be made:

(1) As a by-product, it would still be a significant by-product, telling us something about the nature of the C.T.

(2) There are insufficient grounds for either belief (that something/nothing has happened) in the arranged events and circumstances prior to $Q_2$. And what we do not want to do at this stage is readmit explanations in terms of rational processing (or the lack of it) in relation to the transformation event. The view of the C.T. episode as any sort of test of reasoning ability has been left behind. I am specifically seeking a general explanation neutral to the question of the subject's reasoning competence. All C.Ts have a transformation phase; but they also all have duration, the end point being when $Q_2$ has been answered. With the emphasis on the latter rather than the former property, the episode can still be seen to pose a problem for the subject. But the problem is now to be seen as arriving at a correct belief about the duration. And, given the lack of information, it is difficult to see the intellect being much help in resolving this "problem". Besides, the significance of taking up this perspective of the task is that the correct "solution" here should make failure on a C.T. more likely.
(3) We may be able to check whether or not the lack of temporal information (in combination with the initial deception making the child believe at the start that something interesting is going to happen) is a key influence on performance, or merely a consequential by-product of the belief ("same" or "not-same") governing the performance.

4th Empirical Note

All subjects, in any version of the C.T., receive accurate information from E as to when the episode ends - but normally only when it does end, that is with E's "thank you, that's all", or something similar. It is hardly necessary to say that the way E "terminates" is likely to influence many children in this age range as to whether they believe they were right or wrong. But it may be interesting that, of 17 "conservers" on "O" considered, 2 of them actually changed their answer to the non-conserving response, being particularly attentive to the array as they did so. 1 of the 11 successes on "P" did likewise. And, considering all the conservers together (as is the trend in this and the following chapter), there were various other signs that subjects were frequently having second thoughts about their answers when they realised that the end of the episode had been reached.

But none of the failures on either task changed their answers.

This prompted me subsequently to compare the reactions of successes and failures when, instead of terminating the episode after eliciting an answer to Q2, I asked the children what they would say now if I terminated the episode. The responses to the question "what would you say if I told you that's the end?" divided roughly under 3 types: (a) those that changed their answers, (b) those that wanted to know what we were going to do next, and (c) those who responded in some other way - frequently with "I don't know" - or did not respond at all, except with puzzlement. Of the 12 failures looked at only one subject changed her answer, while 5 responded in the second fashion. Of the 13 "conservers", 4 changed their answers and 2 replied in the latter manner.

I would be the last to claim any great significance of such results, but there is a suggestion that if successes had known that the episode would end when Q2 had been answered, at least some of them might have answered differently (as "non-conservers"). Similarly, non-
conservers might have been more inclined to give the "same" response if they had been sure that the episode was to continue after Q2. In terms of a comparison of "P" and "O": given that "P" tends to produce failures and "O" successes, we would then want to spell out why the former and not the latter is more likely to give the correct "temporal message" to the child.

To go down this path now would be to prematurely attempt to answer Question A, which we will see in chapter 6 does manage to incorporate the temporal aspect as well as another significant but likewise (apparently) unobvious feature of the C.T. But one or two points may be made here. The reasons why a child might want to change his/her already - given answer to Q2 of course are not necessarily related to the reasons the particular answer being considered was given at the time. The obvious step here is to insert the supposedly crucial temporal information before Q2 is answered, keeping everything else the same. One could try to present "O" keeping it intact but adding correct temporal information. Thus E could insert just before Q2 is asked, something like "Right, that's it then. When I've asked you the next question that'll be the end". Conversely a task like "P", where subjects are expected to fail, could be employed, inserting something to the effect that "This game is not finished, but is going to go on after I've asked you [Q2]" Care should, of course, be taken that neither manipulation affects the status of the respective versions as bona fide C.Ts.

The prediction here would be that more would fail on "O" now, and more would succeed on "P". With a handful of subjects there was some indication that the former prediction might be born out, but this line of enquiry was discontinued for the following reasons:-

1) adding the correct temporal information to an intact "O" task just had the effect of making what was being asked sound thoroughly nonsensical and contradictory.

2) The objection to the second manipulation was that it involved telling a lie and adding a further layer of deception to an episode already overlaid with deception. The more levels of deception involved, the harder the task of interpreting responses in terms of the subject's beliefs and understanding becomes.
Conclusion (3 - 5 year old successes and failures)

From a consideration of some of evidence from 3 - 5 year olds we see that it remains plausible that conservers succeed and non-conservers fail for what can be described as basically the same reason. This involves invoking the same source of deception and the same background of lack of information. All subjects initially and naturally expect that something significant will happen before the end of the episode, but are not subsequently given enough information to disconfirm that deceptively induced belief. Additionally and more specifically they lack information as to how long the episode is intended by E to last. At the asking of Q₂, subjects are able to be divided up into (a) those who would not be surprised if this marked (as it does) the end of the episode, and (b) those who would (and will) be. The former tend to fail while the latter tend to succeed.

The tentative conclusion and attempt at a general explanation of success and failure on any C.T. is therefore as follows:— In an episode where nothing relevant happens but where there is not enough information for the subject to know this, the induced expectancy that something relevant will happen without any guidance as to when it will happen deceives some subjects into the wrong answer and the others into the right answer. In terms of answering the specific question (Question A), why a representative young child fails on "P" but succeeds on "O", we need only posit reasons why (a) something in the latter task wrongly and deceptively suggests that there is more to come — thereby correctly suggesting that nothing has happened yet, while (b) the presentation of the former task correctly suggests that there is no more to come — thereby deceptively suggesting that something significant has happened.

While, in the end, it will be found inadequate, there is much that is useful in this "explanatory" framework. Not least is the emergence of the fact that something critical may be happening, not just at the time of asking Q₂, but in and through the asking of this second question. At the moment we are committed to using the two tasks in order to find an answer to the more general question: why does anyone fail or succeed on any C.T? (Question C). In the above we have been concerned only with 3 - 5 year olds, and it is conceivable that — for whatever reason — the above sort of explanation will only find support from observations concerning this age group. For example, while I have
made the point that failure on C.T. does not show that the young child cannot reason and success does not show that he/she can, it remains possible that young children cannot reason, and that the above evidence only applies to subjects who cannot reason.

In particular we are concerned in this chapter with what is involved in success on a bona fide C.T. Insofar as there has been a consideration of C.T. failure (see chapters 3 and 4) this can be seen as having done its work. It culminated here in the suggestion that a deception on the part of the experimenter, and hence a mistake on the part of the subject, may be crucially involved in any instance of successful performance. And from now on the focus remains firmly on the "conserver" and the instances where the correct answer is given. Deception does occur, conservers being tricked into believing (1) that something will happen before the end of the episode, and, additionally, (2) that there is more to come when they answer Q2. Whether this deception can be viewed as bringing about the right answer, as a by-product of what actually brings about the right answer, or something else, we need now to check if the same deception applies to other successfully "conserving" subjects of C.Ts. Given the doubt expressed above, we particularly need to check if the same sort of evidence is obtained from subjects who, we assume, can reason. This takes me to my investigation of successfully performing adult subjects.

(D) Evidence From Adults (Task "P"): See APPENDIX Ib

(a) The reason for the turn to adults

The turn to a group of subjects guaranteed to succeed on the original, definitive version of the C.T., and to a group we might expect to get the right answer in the required way (whatever this might be), is the next logical step. The aim in doing so will be to see how far the view of how a child succeeds on particular versions of the C.T. can be generalised to how anyone is to be seen to succeed on any C.T. It is thus crucial in moving towards an answer to the general question, Question C.
Adults can be presumed to be able to reason. They can also be expected to give the right answer to Q0 on a task like "P". We can check these assumptions. But we can also examine how they succeed on the task. And the specific claim requiring to be tested at the moment is that the representative adult succeeds on "P" in the same way that the representative young child succeeds on "O", and that the sort of rational ability, posited by both pessimists and optimists to be required for a subject of a C.T. to perform as a "conserver", does not come into it.

(b) The immediate empirical aims in doing "P" with adults

Using adults in the project of moving towards the parsimonious, general level of explanation of success, the immediate empirical aims are clear. We want to see:— (a) if the subject is initially deceived into expecting that something significant will happen; (b) if the subject gets the right answer to Q0; (c) if the subject is deceived into expecting at Q0 that the episode will continue/has not really started. Evidence here will include that which comes from, (d) how the subject reacts to the E’s termination of the episode, when Q0 has been answered. The representative adult on "P" and representative young child on "O" can be compared and contrasted in other respects. For example, we can additionally examine, (e) what happens with the adult at the "establishing equivalence" phase, and (f) if giving the subject correct information as to when the episode will end makes the right answer less likely.

(c) Advantages and disadvantages of doing C.Ts with adults

(i) Advantages

Three positive aspects of working with adults stem from the facts that (a) adults can be assumed to be able to reason, whereas for children this would still seem to be open to dispute (b) adults are generally more likely to understand what is asked by another adult, in the way intended, than a young child; and (c) the adult’s performance on a C.T. is generally more predictable than that of the child.
(a) As adults can be assumed to be able to reason, we always have the option of attempting to explain some observed conduct purely in terms of the subject's rational processing of available information. At the same time this does not mean that we necessarily have to accept that such an explanation will be complete. Nor does it mean that we need be committed to seeking such an explanation - that is, if a better, more economical account can be found (as here). Nor again need we be committed to the assumption that an adult will not make mistakes. In the case of both child and adult mistakes, we might suppose that there will be a good reason for the mistake. But we can be more confident of gaining access to that good reason in the case of the adult.

(b) This introduces the communication aspect. The optimists have correctly drawn our attention to the potential communication problem between adult and child in a C.T. When we ask questions, there is usually more reason to suppose that the adult will understand what he/she is asked by another adult, and more reason to suppose that the child may not. And in terms of the answers (right or wrong) elicited, we can ask the adult for justifications, more confident than in the case of the young child that (1) he/she can gain access to, and be prepared to give, the actual reason (even if a flimsy one) that operated, rather than think up an ad hoc reason, for example to please the investigator. And we can be more confident that, (2) the asking for a justification will not be seen as calling for a change in the subject's belief as to what actually happened. We also may, in a context like the C.T., be better able with adults to ask useful hypothetical questions without risking being misunderstood, and with less risk of changing subsequent conduct through asking them. Thus we can intervene before the end and ask a question of the form "what would you say if x?" remaining reasonably confident that the subject will understand that x may or may not be true. And if the subject does modify his/her answer because the question was asked, we may again expect this to show up in a subsequent elicited justification.

(c) There again, if something extra the E does or says is suspected to make a difference to subsequent conduct, we are less dependent on a justification to bring this to light. This is simply because we are more confident, in a situation like the C.T., that we know what the adult would have said in the absence of an intervention in the normal procedure.
(11) **Disadvantages**

The disadvantage of doing C.Ts with adults is that you can lose a lot of friends and/or credibility by it.

It is found to be generally rather embarrassing to involve a subject in a psychology experiment whose outstanding feature is to be that nothing happens. Reactions of subjects at the end ranged from non-plussed tolerance, through confusion, disappointment, boredom, irritation ... to contempt. Of the 8 subjects for whom full transcripts were taken in the task-plus-commentary situation (see Appendix I), the following labels could be assigned in relation to their general emotional response:

**Subject 1: Tolerant confusion** - "I wondered what it was all about. If someone else asked me these questions in a different context I would probably find this person rather childish. But Robert seemed serious and interested in my reaction."

**Subject 2: Irritation** - "Do you want me to write anything I found relevant? [Yes] I didn't find anything relevant [dismissively]"

**Subject 3: Dissappointed irritation**

E: What are your comments [on termination of the episode]
S: Humph! [Disappointed]
E: You sound disappointed?
S: Yes. I was disappointed something more ...
E: Anyway, can you write down just what happened?
S: What, that you made a mess on my kitchen table? That's all that happened.

**Subject 4: Nervous confusion** - at the thought that he might have said something stupid.

**Subject 5: Boredom**

E: Say I did this in a pub? [at the end]
S: You'd expect a trick.
E: Some people say they'd think I was just bored(?)
S: No. I think the person doing the trick wouldn't be bored. But everyone else would be.
"To do an 'experiment' without knowing either the aim or the result was ever so slightly boring ... z z z."

**Subject 6: Tolerance to apparent pointlessness**

E: I wondered if you were disappointed? [at the end]
S: No, not disappointed ... Because I knew it was a psychology experiment, and [therefore] you don't know where it starts and where it stops. Everyone can create different ways of doing things."
Subject 7: Tolerant incredulity -
E: That's the end [after Q3]
S: [amazed] What's the point?
E: ... [To] see that it is unchanged, even if the appearance changes.
S: [incredulous] Does anyone ever get it wrong?
E: Children of four for a start.
S: Do they? Well that's interesting isn't it?
E: Not particularly.
S: No, I suppose not.

Subject 8: Contempt
E: Suppose all this had taken place in a pub with beer ... ?
S: You'd be drunk.
E: Suppose I wasn't
S: You'd be trying to rip me off. Some sort of trick. Anyway it would be a waste of beer ... You're seriously doing a PhD on that!?

Such features of the subject's general response to the arranged events and circumstances can only give rise to doubts on how we are to psychologically interpret the conduct we are interested in. A look at one aspect of the experimenter's conduct reinforces these doubts. The subject understandably wants to know what is going on and therefore tends to want to ask E questions:— e.g. (a) "What sort of a psychology experiment is it/will it be [something to do with psychoanalytic symbols? (subject1); observation accuracy/optical illusions? (subjects 1, 2, 4, 5, 8); compliance/conformity (subject 2)?; eye-contact?; memory?; reasoning task? ... etc, etc]; (b) "How long will it take?" (a crucial aspect in terms of the present discussion); (c) "Is there a trick?/Is there more to it than meets the eye?"

In the first place, the child happens not to ask such questions, and the aim is to keep the episode arranged for the adult as close as possible to the C.T. episode that the child encounters, which means not providing any extra potentially useful information. In the second place, answering questions such as these truthfully may go a long way towards giving the game away — i.e. telling the S that nothing happens and he/she should not expect anything (involving a trick, fancy reasoning, etc.) I was thus understandably reluctant to answer these or other such questions. However, this often is seen merely to make the episode even more mysterious and enigmatic, increasing the subject's disposition to look out for and expect something really interesting.

Also tending to crop up are questions like, (d) "Are/did you [E] make sure that, the glasses [A and B] are exactly identical?"; or (e) "Did you/how did you make sure there was exactly the same amount of
water/powder in each glass?" The truthful answer in each case is inevitably "No". But to baldly own up to this gives the subject licence to suppose initially that he/she is being asked to give an opinion. This may suggest to him/her that it is justifiable to change his/her opinion subsequently - e.g. at the point where Q2 is asked. This, of course, is not at all what has been wanted in the case of the C.T.

In the end, to establish equivalence, what I frequently had to say was "Just assume that there is the same [in A & B]". This is highly unsatisfactory, because: (1) as an hypothetical assumption rather than a fact, the rational subject is again free at any point to decide that the hypothetical assumption he/she was asked to make was, after all, unwarranted; (2) it is not just possible but likely that a 4 year old would not understand what is meant by and involved in "making an hypothetical assumption". If he/she did understand we might well ask what we are doing trying to design or use a test involving this to see if the child can reason concretely. For it would suggest that the child can do much more than this; i.e., in using an hypothetical assumption, showing "formal" reasoning competence. Again it is stressed that we should at least be aiming at giving the same sort of task to the adult that we are accustomed to giving to the young child. It is therefore undesirable, though often necessary, if we we want to proceed beyond the preliminary phase of the experiment, to add something to the procedure in the former case.

Overall, the best I could do was to ask the subject, when I invited him/her to participate, to refrain from asking too many questions - including why he/she should refrain from doing this.

However, all these problems associated with doing C.Ts with adults, rather than discrediting the evidence, were in the end seen as an important contribution to the evidence. The above aspects reveal the strangeness and unsatisfactoriness of the C.T; as both a reasoning task and a social transaction. And they do so in a much more emphatic way than emerges from just contemplating what is (and what is not) involved, and from doing C.Ts with children (see 1st empirical note).
I persuaded 14 adult subjects "to help me with some work I needed to do for my psychology PhD". When asked what it would involve, I told each person that "it is important that I don't tell you anything about it other than that, "it will involve me asking you some questions in a certain context". To overcome reluctance, I frequently had to say something like "You can trust me. It won't involve any pain or unpleasantness" and/or offer some reward: usually "I'll buy you a pint". As mentioned, I tried to avoid answering questions as to how long it would take. When pressed, I was suitably vague: ("Don't worry. Not too long. And you can stop any time you want.")

The first phase involved doing a straight version of task "P" with six of the volunteers. The subject(S) is seated in front of a table on which stands the initial array - 2 half-pint identical glasses filled to what I had estimated to be the same level, and an empty pint glass set to the side. On three occasions the substance used was water, and on the other three "Coffee-Mate" powder was used. The following "standardised transcript" captures the main points of the ensuing dialogue with the subjects (subjects A to F), but is illustrated here for the case of water:

E: What do you see in front of you?
S: Three glasses: two half-pint ones with (what looks like) water in, and an empty pint glass.
E: Is there more in this one [A], less in this one [A], or is there in this same in each [of A and B; this is Q1]
S: They look (about) the same ... [in 4 cases, appreciable difficulty in getting the S to "establish equivalence" - even more so than for the young child. I usually had to eventually indicate that I wanted them to agree that they were the same so we could continue. On one occasion I ended up saying "Just say that they are the same"!] ... O.K. I'll say they're the same [Big decision, as if they were risking something].
E: Now watch carefully [I pour the contents of B into pint-glass C] Is there more in this one [A], less in this one [A], or the same in each [or A and C; this is Q2]
S: [without hesitation] Still the same [even, "waiting" tone of voice, attention remaining fixed on the array, ready for the next move]
E: [I paused, refraining from saying anything until the S looks up at me. After a period of silence one S said "unless you mean there's a few drops left in that glass (C)"?]
That's it. That's the end. [pause for the S's reaction]
S: [S looks immediately back at the array. Baffled silence. Looks back at me (for me to explain myself and/or for a clue from my expression which remains neutral)]
You're joking/Is that all?
E: You seem surprised/disappointed [whichever is more appropriate]
S: Yes I was expecting something more.
E: What? That it would continue?
S: Yes, or that something would have happened. Did I miss something?
E: [Avoiding answering this] When you answered the question after the pouring [B into C] were you waiting for something else to happen with the beakers and water.
S: Yes
E: If you had known before you answered the second question that that was the end of the episode, would you have answered differently?

[This usually merely caused confusion as (a) the S tried to remember how many questions there had been (remember establishing equivalence often involved repeating Q1) and what was the difference between them; and (b) the S tried, with difficulty, to imagine having an explicit belief that the episode would end when it did. Some indication that there would have been a reluctance to answer at all - that the S "would not know what to think".]

Subjects' comments indicated that their impression was that they had been misled or deceived in some way (see later)

On the basis of the results from these 6 subjects - amply supported by the findings (fully reproduced - see Appendix 1b) from the other 8 subjects (subjects 1 to 8) on the expanded, task-plus-commentary situation - the immediate aim of this piece of research, listed above as (a) to (f), can be addressed.

(a) Is the subject deceived at the start into expecting that something significant - relevant to the array, and the answering of the questions that would be asked about it - will happen? This was the case for subjects A to F. Support also comes from 7 of the 8 task-plus-commentary subjects (The exception here is subject 6, a psychology graduate claiming not to have had any particular expectations, knowing that "it is a psychology experiment, and you don't know where it starts and when it stops").

(b) Do the subjects get the right answer to Q2 ("same")? 13 of the 14 subjects did. The exception was subject 2, a chemistry graduate, a reluctant participant, suspicious and sceptical from the start: " ... how am I supposed to know it was water in both glasses at the start? ... I can't look at liquids like someone else. Remember I did three years of chemistry. It changes the way you look at liquids, volumes and that." 

(c) and (d) When the subject answers Q2, does he/she expect that there is more to come/that the experiment is still in the preliminary phase? What is S's reaction to the termination of the episode when Q2
is answered? With the possible exception of subject 6, all 14 subjects showed features of "waiting conduct" as they answered Qa. Additionally, again with the exception of subject 6, all subjects showed surprise at the "termination", often followed by an expression of disappointment and/or irritation. This reaction is confirmed when subjects are subsequently asked for their comments. E.g. see subject 3: "I was expecting something more ... When you pour one glass of water into another ... well ... well what!? ... you made a mess on my kitchen table. That's all that happened"; subject 5 (in response to my asking what she would say if I told her that was the end of the experiment): "I wouldn't think you'd achieved anything"; and subject 8: incredulity and dismissive contempt. (See (f) for more on S's reaction to the "termination".)

(e) What happens at the "establishing equivalence" phase? Like the child, but even more so, the adult subject frequently "makes a meal" of the attempt on the part of E to establish the equivalence of the amounts in containers A and B. Whereas Qa is generally answered unhesitatingly, this is often not the case for Q1. A lot of thought goes into answering the question about the initial array. The subject is more inclined here, than at Qa, to admit that he/she has not got enough information and the only correct answer is "I don't know". In the end subjects tend to decide to make an informed guess (subject 8: "Do you want me to use my judgement and make a guess?"); this guess being as much "informed" by the subjects perception that E wants him/her to say "same" (subjects 1, 3, 7 and 8, the first two eventually evidently taking pity on me), as by information in the array (subject 6, in retrospect: "I think there are two parameters to determine how much water is inside. One is the diameter, the other is height ... But when I guess, I would just use one of them"). What is clear is that subjects, at the time of encountering first Q1 and then Q2, are more inclined to believe that the "something interesting" expected is referred to by the former question than by the latter.

(f) What is the effect of giving the correct information as to when the episode is to be terminated? As for the child success, we can first look at the effect of giving the correct information that the episode has ended. That is, I again turn to the subjects' reaction to E's termination of the episode. But this time the specific aim is to see if there is any tendency for the "termination" to bring about a reversal of the subject's answer, as was indicated for the child conserver. It is
seen that on no occasion does the "termination" have the effect of actually making the subject think/say "the answer must have been not same". However the tendency is there, given that the termination produces surprise and confusion. The suggestion is that, if the subject had known that the episode was over when he/she answered Q2, he/she would have shown some reluctance to give any answer (at least without asking a few questions). In other words we are justified in supposing that providing correct information as to when the episode will be terminated will make the right answer less likely, even if it will not bring about a complete reversal.

The obvious step - as for children likely to succeed (i.e. on "C") - again would seem to be to tell the adult subject that Q2 marks the end of the episode, before Q2 is answered. But the problem here was that subjects generally did not believe me.

It is thus established that there are some potentially significant aspects of convergence in the evidence comparing child and adult "conservers". I will now summarise what this combined evidence may and may not be able to tell us in relation to the sought-for general level of explanation of any C.T. performance. It is to be remembered that the central question now is: what is the most parsimonious, psychologically-relevant and empirically-supported account that is able to cover all instances of C.T. success.

E. Review of the Combined Evidence for the "Deception Explanation"

I have deliberately taken an extreme position. I have tried to argue, in the first place from evidence gathered from 3 - 5 year olds, that successes - however and on whatever bona fide version of the C.T. the successful performance is elicited - succeed in the same way. This is the less contentious aspect. More contentiously I additionally suggest that successes succeed in much the same way that failures fail. Both results can be seen as the consequence of the same source of deception, against the same background of lack of information. Seeking a parsimonious, single-factor explanation, I therefore attempted to identify a single, potentially critical piece of always-missing
information. I saw the crucial detail as lying in the fact that the subjects do not know how long the episode will last. It was potentially decisive as it had to be compensated for in one of two ways. Thus, at any point during the arranged episode, the subject can be viewed as having to decide that "this is likely to be the end" or, alternatively, "there is likely to be more to come". The emphasis is on explaining the elicited response to Q₂, and the decision can be viewed as needing to be made on hearing this question. If it turns out that we are able to see the subject's answer to Q₂ depending on this decision, then we will have avoided bothering ourselves directly with what the subject makes or does not make of the arranged events and circumstances concerning the array. The attraction of this mode of explanation of the C.T. results therefore lies in the fact that it would allow us to by-pass that which the conservation controversy revolves around.

The conclusion from the combined evidence was that subjects who get C.Ts right usually turn out to be subjects who are deceived into incorrectly expecting that the episode will continue after Q₂ has been answered. Conversely, subjects who get the wrong answer are likely to be those correctly believing that there may be no more to come after Q₂. At the moment the only evidence quoted in the latter cases comes from young children (see Chapter 7 for adult C.T. failures).

The question remains as to which way round are we to view the direction of influence in relation to successful performance. Does the subject get it right in a particular case because he/she is deceived in that instance into wrongly believing that there is more to come? Or is S tricked into wrongly believing there is more to come because - for whatever reason in that instance - he/she correctly believes that nothing relevant has happened yet?

The successful conserver is tricked. The adult subject commonly states after the episode has been terminated that he/she believes he/she was deceived - whether this was into making some stupid mistake (e.g. through inattention), into making some understandable mistake (e.g. through some sleight of hand on the part of E), or merely into having agreed to participate in something that was a waste of time. The important point in relation to the issue of deception is, however, that at the point of being required to answer Q₂, successful subjects have been tricked into believing that there must be more to come. In turn, this is because they have been led to expect (like non-conservers) that
something relevant would happen at some point, when in fact nothing would happen.

But the evidence suggests that this incorrect "temporal" belief may be able to do no more than reinforce an already-held correct belief that the approved answer to Q₂ is, as for Q₁, "the same". It seems that the existence of the deceptively induced belief on its own is insufficient to fully account for successful performance. What is implausible here is the view that the subject bases his/her answer on available "duration clues" alone. Even if our concern is with the perceived duration, at some point we must address the question: the duration of what? It is a feature of a C.T. that information as to when and where it will end is withheld. But we are no longer able to sustain a consideration of the possible significance of this feature in isolation from other properties that characterize any bona fide C.T.

This means that attention must shortly turn to the arranged events of the C.T. What goes on from the setting up of the initial array and asking of Q₁ through to the answering of Q₂? The notable event would seem to be the transformation, and it will now be necessary after all to properly consider how the subject may be taken to view this as well as any of the other events and circumstances characterizing the C.T., in instances where successful performance occurs.

But, apart from the above analysis of C.T. performance solely in temporal terms being unduly and artificially narrow, it is also psychologically illegitimate. This is so at least insofar as the fact that a deception - and hence a mistaken belief - has tended to appear bound up with the would-be explanation.

F. The "Deception Explanation" as Psychologically Illegitimate

The main criticism on the grounds of legitimacy is that the "deception explanation" is just not psychological enough. Such an explanation tells us more about the investigator who designed, and the experimenter who uses, the task, than about the participating subject. This follows from the fact that the main point being made here is that it is an inherent part of the nature of the C.T. to involve deception. It is a property of the task that has as its consequence the fact that
any participating subject will be deceived into wrongly supposing that something significant will happen at some point during the episode.

This fact is significant in that it suggests that both "non-conservers" and "conservers" are going to be found to be in error somewhere along the line. In turn, this assumption is a useful one given that the primary project has become that of understanding how best to view what is involved in success on any C.T. Specifically, it needs to be spelled out how, in spite of insufficient information, the subject who "conserves" in any particular case — including the presumably rational adult on "P" — is able to arrive at the right answer believing he/she has got enough information.

The clearly defined task was then one of identifying a mistaken belief on the part of the conserver that (a) would have its source in the above deception that applies to all subjects (S led to believe that something relevant will happen when the essence of the C.T. is that nothing happens or changes), but that (b) would be likely to influence the subject's performance on Q₂ in the direction of success rather than — more straightforwardly — failure. It was certainly, therefore, a helpful move to go looking for a mistake consistently made by all successful conservers, irrespective of the version of the C.T. being considered. But, in the end, the fact that both non-conservers and conservers make mistakes is merely, from the point of view of arriving at a psychological understanding of what subjects do and say in the C.T., an interesting diversion.

We do not want to know — at least initially — in what way the subject is mistaken in the beliefs he/she holds. In the first place we want to know what those beliefs are and how best to describe them in order to relate them to observed conduct. Specifically, we want to describe the belief, or set of beliefs, that needs to be invoked to understand why the subject answers "same" to Q₂ in those instances when he/she does. What we describe should receive support from the evidence and make intelligible other aspects of observable conduct — for example the conserver's response to E's termination of the episode. One conclusion from the above analysis is that an adequate understanding at this level has not yet emerged. We need to consider more than just the perceived duration.

The more general point here is that the "deception explanation" refers to and makes use of information about the events and circumstances of the episode that is unavailable to the subject. At
least, this is the case if the deception is successful. Yet the subject must be presumed to respond on the basis of information that is available to him/her. Admittedly we do not want to frame an explanation of the subject's observed conduct only in terms of the subject's chosen beliefs, which conceivably might bear no relation to what E thought he had arranged. On the other hand we are obliged, in the first place at least, to avoid attempting to adopt an explanation in terms of information available only to the investigator with his/her privileged perspective of the episode.

A compromise is required on the issue of what events and circumstances are to be legitimately referred to in moving towards a psychologically relevant explanation of C.T. performance. In the first place we might see how far we get sticking to information about the episode that the investigator who arranges it and the subject who participates can be assumed to agree about (see chapter 6).

G. The Way Ahead

The focus is now firmly on successful conservers and on establishing a level of explanation of successful performance that (1) embraces the evidence already obtained from the representative successful child on version "0" and the representative successful adult on version "P", and (2) also allows us to derivatively account for failure, if and when it occurs on either version.

Following on from the end of the last section, one belief that E, any successful conserver and a fully informed observer would all seem to be able to share is that the transformation of the array does not result in any difference in relation to the amounts being compared. What the E does is arrange and perform the irrelevant transformation. The intention has been to present the subject with the problem of working out rationally that there is the same amount in the large container, C, as in the small container, A. The question is: what does the subject make of the transforming event and its consequences, and - in spite of the absence of adequate information - how is the successful subject to be best viewed to arrive at the correct solution to the problem?
In terms of providing an adequate general explanation of performance the sort of deception account considered must be rejected. However, the value of such an analysis remains and the implications must be taken into account by any explanation of the C.T. results offered. In this respect, the following points are noted:

1. There was a broadening of the scope of the evidence whereby, for example, the reaction to the "termination" must now be considered.
2. The source of evidence from successful performers was extended to take in that obtained from adults. More specifically,
3. It is now recognized that something significant in relation to the elicited performance in any particular instance may be happening in or around the asking of the question second time round (Q2). That is, there may be other events to consider in relation to their possible influence on how the subject answers Q2, apart from those associated with the transformation. Finally,
4. It is established that mistaken beliefs are held by both unsuccessful and successful performers. This goes some way towards confirming the doubts I expressed in previous chapters as to the appropriateness of the terms "success" and "failure" in relation to any C.T. episode. The analysis in the following chapter merely reinforces this point.

NOTES

1. The possible self-fulfilling aspect associated with the criteria for selecting and rejecting subjects need not worry us unduly. This is because (a) most of the subjects were interested and (b) it did not result in the selection of an unrepresentative ratio of successes:failures (see section A).

2. Given that the ultimate goal is to explain success and failure performances, we might prefer to put this prediction the other way round. But if the key event, as regards a behavioural index, is to be seen as the response to the "termination", then - at least in temporal terms - it is the performance that is to be seen as the "predictor".

3. It is useful to note that, at least the present levels of analysis, it is not important how the E obtains the success and/or failure results - so long as the experiment stays within the general design constraints and remains a bona fide C.T. This is important as it enables us to disregard small procedural variations (additional comments, non-verbal elements, tone of asking the questions) that may exert an influence on
performance. At some point, however, we may want to see how particular variations may affect the "temporal message" being given to the child answering Q2 (see chapter 6 (H)).

"As far as possible I avoided giving a series of C.Ts to a child over a single session. When one subject was used for more than one C.T experiment (especially if they were as closely related as "P" and "O"), I usually employed an interval of at least one week (generally much more) between testings, or testing and re-testing.

"Such instability of the "conserving" answer is entirely in line with a body of evidence quoted by Russel (1982, p.97) that young children do not appreciate the necessary truth of their correct responses. But - significantly - he adds "that even adults do not appreciate conservation as a necessary truth". In the light of what has been said in the previous chapters, this is not at all surprising. Because, unless we are referring to the experimenter's privileged perspective of the C.T. he/she has arranged, the answer "same" is not necessarily true.

"Indeed it can be confidently asserted that there has never been a volume conservation task performed where the right answer to either Q1 or Q2 actually was the supposedly correct response, "same".

"I discussed my work and the notion of "conservation of volume" with another chemistry graduate. He did not regard "conservation" as a useful concept, pointing out that if you add 50ccs of water to 50ccs of alcohol (C₂H₅OH) you get 98ccs of the liquid mixture. "Whatever's conserved there, it's certainly not volume", he commented.

"If it is not successful, and the S recognizes that the attempt to deceive is a component of the episode, this makes the task of interpreting S's answer to Q2 even more of a can of worms.

"See Chapter 6 (section E) and chapter 8 for more rigorous treatment of this topic - relevant to answering Question A, B, and C, and to identifying a general source of error.
CHAPTER SIX

CHALLENGE 3 - AN ALTERNATIVE APPROACH
TO GENESIS: ANSWERS COME FROM QUESTIONS

A. Introduction

In the previous chapter I approached the question of what is involved in success on a C.T. from a deliberately idiosyncratic perspective. The justification was that the strange nature of the context of the task allowed such an approach. Being as far removed as possible from that of Piaget and his optimist critics, the advantage of adopting that perspective was that it also by-passed the main aspect of the conservation controversy, arising from their approach.

A consideration of the deception aspect has served its purpose and is now left behind. I attempted to show that, if the episode is to be viewed as containing a problem, we do not have to see it only, or even primarily, as one of working out what has happened to the array. Instead we might see the problem facing the subject to be one of working out when the episode is likely to finish. I suggested that the subject who arrives at the correct decision at Qz (i.e. that the episode will now finish) would be the one likely to give the wrong answer ("not-same") to Qz, and the one who is in error here would be the one likely to give the right answer. If the evidence had been positively decisive, the correct belief as to the relative amounts would then need to be seen as a by-product of the incorrect belief on the "temporal" problem. In the end, however, this strategy proved inadequate to provide the sought-for parsimonious explanation.

The strategy in this chapter starts from a move to the other extreme. Instead of trying to by-pass the Piagetian - post-Piagetian cognitive framework for interpreting C.T. performance, I attempt to exploit it, to see where it leads. Where it finally leads is to the parsimonious general level of explanation sought. It provides what I
suggest is the most economical, psychologically-relevant account available of how anyone may be viewed to succeed on any bona fide version of the C.T. It enables both Question A and, later, Question B to be answered in a way that receives adequate empirical support. However the answers offered are radically at variance with those offered by both Piaget and his optimist critics, and have implications that will eventually allow severely critical comments to be directed at both "opposing" camps (see Chapter 8).

Three key components may be identified in a Piagetian approach to the understanding of an observed instance of purportedly intelligent behaviour. Firstly, the framework for interpreting it is a cognitive one: the described conduct is viewed as a solution or attempted solution to a posed problem. Secondly, the conduct in question will not be fully explained without reference to its genesis. Finally, the conduct to be explained is not to be considered in isolation, but in terms of a psychological structure associated with it.

Whatever aspect of the living realm Piaget addresses - whether this be some observed anatomical feature peculiar to running-water snails, or say, the observed response of the 7 year old on task "P" - he is committed:- (a) to a view of it as an adaptive response and attempted solution to a problem posed by the organism's environment, (b) to fitting it into a story of progress from less adequate to more adequate solutions, and (c) to viewing it in terms of the overall organised structure into which it fits, and the constraints imposed by this structure.

It is each of these components in turn which I exploit in the service of arriving at a parsimonious level of explanation of the conduct in question - that is, any successful performance on a C.T. Thus, in accord with these guidelines, I ask:-

(1) What is the best way to solve the problem seen as being posed by the C.T., and how does this relate to the evidence as to how "conservers" actually do arrive at the right answer? (The cognitive question)

(2) Where do the right answers come from? (The genetic question)

(3) How are we best to view the psychological structure associated with the giving of the right answer on a C.T.? (The question of structure)

In anticipation of the answers offered to these questions, four key observations that will help the reader comprehend the empirically-
supported argument that follows are made here, and will be elaborated on later. They are:-

(a) that the solution to a problem is that which makes the problem disappear; and sometimes the problem-solver may be in a position to go one step better (see below).

(b) that when the conduct to be explained is an answer, it presupposes - and can be viewed to have its origin in - a question.

(c) that prior to asking about the psychological structure behind, or enabling, some item of observed conduct, a structuralist-oriented enquiry may concern itself with the structure in the observable conduct of the subject - that is in the whole response of the subject to the situation, of which the item of specific interest is to be viewed as just a component.

(d) that the questions asked by the experimenter in the C.T. episode do not just refer the subject to the context to be responded to, but are part of the context responded to. They are themselves events, not a transparent reference to events.

B. "The way to solve the problem ... is to [act] in a way that will make what is problematic disappear." (Wittgenstein, 1980, p.27c)

Successful conservers are deceived. But, (1) they cannot now be said to succeed because they are deceived in the way suggested in the previous chapter, and (2) we anyway seek an explanation that is more "psychological". And I have chosen to go down a cognitive psychological path, to see where it leads.

A strictly cognitive approach views the situation responded to as a problem, and the subject who responds solely as an efficient (or not-so-efficient, as the case may be) problem-solving device. It is therefore not directly concerned with the role of consciously-held beliefs in guiding conduct (as we were in the last chapter). Such beliefs, whether correct or mistaken, tend to be viewed as the by-products of series of mediating information-processing operations within the cognitive apparatus of the subject. At the extreme we see the cognitive scientist resorting to the computational metaphor. He/she identifies the problematic aspect(s) of a situation and the correct or most effective
response to it. He/she then asks how an unconscious, information-processing device would need to be programmed in order for it to arrive, through a series of logical steps, at the correct solution and appropriate output. This is related to the evidence as to how subjects actually do arrive at the correct solution.

Now the adult consistently gets the right answer and can be presumed to be able to reason. A cognitively-oriented approach would probably start from the assumption that the subject must have been given enough information to work out the right answer, and, further, that the successful adult "processes" it in the right (rational) way - whatever this is. The questions that remain, therefore, are, (1) what is the problem posed by the C.T?, and (2) what is the right/best way for a subject, or his/her cognitive apparatus, to solve this problem?

We may choose to go along with mainstream conservation research and view the problem as being posed by the transformation. To arrive at the right answer ("same"), the subject, using the information available, must work out that, whatever the appearance of the final array suggests, the transformation phase (involving here just the pouring of the contents of B into C) can have no effect on the relative amounts being compared. It is therefore irrelevant and the amounts in A and C are the same.

As I have stressed, however, there is not enough information available for the subject - behaving in a purely rational manner - to arrive at the correct solution. He/she cannot know from the final array alone that there is the same amount in containers A and C, and no one would dispute this. This would seem to leave the problem faced as that of working out that what happens during the transformation phase is irrelevant to the question about relative amounts.

Framing the problem in this way, difficulties can immediately be seen to arise for a cognitive approach. The first is that, at the time the transformation occurs, the subject cannot know what he/she will be asked next. He/she therefore is short on guidelines as to what may turn out to be "irrelevant" and "relevant". We are thus dealing with a retrospective view of the transformation phase on being asked Q2. Hence, not only is memory involved, but memory for something that is actually irrelevant.

Let us suppose that the subject is to be seen as facing, at Q2, the problem of working out the relevance/irrelevance of what happened. He/she is then supposed to decide on purely rational grounds that E's
actions (e.g. in pouring, etc) were irrelevant to that which his/her question refers (relative amounts). But we should be uneasy about such a view of the subject's cognitive processing. The transformation of the array, being irrelevant, has no more significance with respect to its effect on what is asked than the rain beating on the window, the traffic noise outside ... etc., that is, than an infinite number of other things present and/or going on at the same time in the vicinity or beyond. If the subject is supposed to "process" E's irrelevant transformation to conclude that it is in fact irrelevant, then we must suppose that the subject also has to process all the other irrelevant circumstances and events to the same end. We can presume that such a processing project will be never done - certainly not within the time given for answering Qz. In this case some potentially relevant events and circumstances will go unprocessed. The occasion for doubt will therefore, in principle, remain.

The difficulty here is merely compounded by the facts that, (1) **everything** that happens is arranged to be irrelevant, the essence of a C.T. episode being that nothing relevant happens, and (2) as we have established, the subject starts off wrongly assuming that something relevant will happen. If we are going to find a problem spontaneously occurring to the subject (or his/her cognitive apparatus), we can presume it would be the question of **what** was supposed to happen during the episode, rather than that of **whether** anything was supposed to happen.

In the end, a view of the subject (or his/her cognitive apparatus) processing the totality of events and circumstances around him/her for their relevance/irrelevance is psychologically untenable. In spite of the indicated difficulties, we have to suppose that Qz should focus the subject's processing capacities on the problem specifically of whether the **transformation** is relevant or irrelevant to what is at issue.

The mere fact that this can be seen as presenting a problem, however, already seems to imply that something that makes a difference to how the question is answered **could** have happened at the transformation phase. It is relatively easy (e.g. see chapter 7; section E) to confirm that something could have happened to make the correct answer to Qz "not same" and, again, the only truly rational answer to Qz "I don't know". If the problem addressed, therefore, is the question of the irrelevance/relevance of the transformation phase, this might be expected to be the response of a rational subject behaving in a purely rational manner.
However, in spite of having insufficient information, presumed-rational adult subjects give the correct answer ("same"). On the cognitive approach how are we to try to account for the observed conserving response that correctly solves the problem, supposedly posed by the transformation and referred to by the question, Qa?

The subject cannot arrive at the conclusion that the transformation makes no difference and is irrelevant to the question about the relative amounts—by purely rational operations on the available information. But we can view the subject "processing" the events and circumstances relating to the array in a way that is faultless on rational grounds, and arriving at a view that has equivalent consequences—at least with respect to the generation of the right answer ("same"). As well as avoiding the above difficulties, the view of the "processing" required, we will see, could hardly be stated in more psychologically economical terms.

It is at this point that it is helpful to make explicit the most basic criterion for establishing that a problem has been "solved." At its simplest, a problem is taken to be solved when it is made to disappear. Whatever view of problem-solving and the processes we take, it has to be acknowledged that the appearance of the correct solution to a problem coincides with the disappearance or dissolving of that problem.

There is more here than merely a criterion for helping us decide whether or not a problem has been solved. There is also an indication of the sort of framework we might choose in order to investigate the psychological processes by means of which problems get solved. Accordingly, as Wittgenstein (above) tells us, we can take a view of the subject (or his/her cognitive apparatus) faced with a problem and being required to act "in a way that will make what is problematic disappear".

Accepting this, we can ask: What, in general but psychologically relevant terms, is the best way of making the problem disappear? Now the most effective, economical and error-minimising way of ensuring that a problem no longer exists is to refuse to acknowledge that a problem ever existed, if there is the opportunity to do so. That is, even better than making haste to make a problem disappear is to not allow it to appear in the first place.

With this view of the process of problem-solving, therefore, the best way of solving a problem is to ignore or disregard it— if the
subject (or his/her cognitive apparatus) can get away with it. And the conservation task is an archetypal example of a situation where the subject can "get away with it".

We can always try to deal with a problem by ignoring it. Frequently, however, the attempt will patently fail to lead to the disappearance of that problem. In this case the problem will either be seen to remain until such time as it is not ignored. Or it will subsequently re-appear in a different but related problematic form. A double-decker bus bearing down on me presents a problem - and trying to resolve this problem by ignoring it is not advisable. More cognitively, I might decide to cut down a large tree ignoring that this might involve more than just randomly sawing through the trunk and running away. Not bothering to reason how and where to cut, I start sawing at random. The saw gets stuck. This leaves me still with the original problem of getting the tree down, but with an additional problematic component - getting the tree down without a saw. Again, I might be faced with the task of estimating amounts of water in various containers and decide to do this on the basis of the levels, ignoring the problem that the glasses have different shapes. Here I face a potential subsequent contradiction. For example, if the amounts are to be measured with a graduated cylinder at the end, my mistakes will be exposed.

But none of this applies to a bona fide C.T. such as "P" or "O". There will be no disastrous repercussions, no recalcitrant problem and no subsequent contradiction* if I ignore/disregard the "problematic" transformation and its consequences, or, indeed, if I fail to even notice that a transformation has occurred. If I gave the approved answer ("same") to Q1, and ignored/disregarded the arranged events intervening between Q1 and Q2, I can only assume that there is still the same in each, and answer Q2 accordingly.

On such an account of the subject's right answer, it would not be correct to say he/she has reasoned that there is the same in each container, and/or reasoned that the transformation is irrelevant. He/she is seen as not even considering the (existing) possibility that there might not be the same. The question of the relevance/irrelevance of the transformation phase is viewed to have not even "crossed the mind" of the subject. If it had, at least as a rational subject behaving in a totally reasonable manner, I would have to acknowledge that I do not have enough information and that the answer is "I don't know".
But even a totally reasonable subject (or information processing device) cannot be expected to attend to everything — especially where everything in the vicinity and beyond is actually of equal relevance to what is asked about, all being as totally irrelevant as the current F.T./Dow Jones index. Thus he/she can no more be faulted on rational grounds for ignoring the possible significance of the actually irrelevant transformation as for ignoring the possible significance of, say, the (actually irrelevant) rain beating on the window at the time. And doing so, the reasonable subject is able to take the (correct) view that nothing has changed, that there is still the same in each of the compared containers, and believe — albeit incorrectly — that he/she has enough information to know this.

From the premises, (1) that the way to solve a problem is to act in such a way as to make it disappear, and (2) that the best way to make a problem disappear is to not allow it to appear, I have arrived at what I will call the "gross negligence explanation" of successful performance. In the end it, like the "deception explanation", and for a similar reason, will be rejected for not providing us with the psychologically-relevant account of successful performance that we seek. Nevertheless, this part of the analysis and empirical investigation not only provides an important step towards the sought-for general explanation of the results, but allows an important lesson to be drawn, with implications beyond the conservation controversy. We can learn something with a bearing on the legitimate application of cognitive approaches in the service of interpreting conduct — whether our concern is with the child or the adult. Here our concern is with both and, in particular, the question of whether the child and adult "conserver" can be seen to succeed in the same way. So, with the "gross negligence" hypothesis in mind, let us again turn to the empirical evidence, starting where we left off in the previous chapter — with the adult on task "P".
C. Evidence for the "Gross Negligence" Explanation

(a) 6th Empirical Note (Adults)

Some of the evidence quoted in the previous chapter is also relevant here. This came from (a) elicited comments about the episode as it was occurring, plus subsequent justifications and impressions following the "termination" point (see Appendix I); (b) observations on the way subjects react to aspects of the episode and manner in which they deliver the (normally correct) answer to Q2.

The main point gleaned from that evidence was that subjects are, at the point where Q2 has to be answered, expecting more and - specifically - waiting for something significant to happen (for the "point"). Termination of the episode elicited surprise, followed by confusion and/or disappointment and/or irritation. The possibility that the initially-anticipated significant/relevant event has happened, and happened between the answering of Q1 and asking of Q2 (as it has, assuming that the transformation is the intended significant event of the C.T.), is generally disregarded - at least judging by observation and by the subsequent comments (e.g. "Do you want me to write down anything I found relevant? ... I didn't find anything relevant" - [82])

The subjects who did suspect something related to the array had occurred tended to presume it was something to do with the initial, "establishing equivalence" phase.

Thus, while the adult subjects doing task "P" do not fail to notice that a transformation of the array occurs, the evidence suggests that they discount its possible significance. And it seems that, rather than subjects concluding from some process of reasoning that the events of the transformation phase are irrelevant and can therefore be disregarded, more support is lent to the view that they do not even consider the question of relevance/irrelevance of the transformation.

What I (as the experimenter) am seen to do at the transformation phase is just pour the water/powder from B into C. I have made the point that something relevant (interesting/tricky) could have happened here, and it is not difficult to get subjects to acknowledge this - in retrospect. This sometimes comes out when the episode is (surprisingly) terminated. Realising there is no more to come, subjects may consider interesting possibilities they previously neglected. At least they
realise that there were interesting possibilities that could make "same" the wrong answer and that these could be associated with the transference of the water/powder. It also sometimes emerged when I asked subjects if they might have had different expectations/reacted differently if the episode had taken place in a pub. There are two significant responses to this request to be noted:— (1) I (that is, E) would be presumed to be bored and doing something aimless and silly (viz. S¹ and S³). That is, nothing that I was doing would be expected to have any significance, and the subject would (correctly) neither suspect— just prior to the "termination"— that anything relevant had happened, nor suspect that anything relevant was going to happen. (2) Some sort of trick would be expected (viz S², S⁵, S⁶). In this case it was not only possible but likely that something interesting would have been arranged to happen, during and through E's manipulation of the array, that would make "same" the wrong answer.

The important point here then is that something about the design of the task "P", taken in conjunction with the context in which it is embedded (subjects participating in a psychological experiment— rather than, say, a Christmas party game or the "trick environment" [S²] of a pub), plus my presumed serious interest (e.g. S¹), appears to lead the adult subject to believe that, whatever is/will be the significant aspect, it is not going to be the transferring of the water/powder from one container to another. Taken in conjunction with a different context and a different (less serious?) sort of interest on my (E's) part, the transformation might have been viewed as the potentially significant aspect.

Such points will become important when I come to consider, in the next chapter, what might be required to get an adult to fail on a bona fide C.T. like "P". But the conclusion here from the available evidence is as follows. While what has been designed to be the significant event of the episode (the transformation) occurs and gets left behind, there is little doubt that its possible relevance and, indeed, the "problem" of whether or not it is relevant, is not considered. From what the subjects were able to show and tell me, the explanation that successful conservers get the right answer by ignoring/disregarding the problem was supported.

Of course there is no guarantee that "what the subjects were able to show and tell me" provides a useful guide to the actual problem —
solving processing of information underlying their correct responding. As we have seen in chapter 3, there are pitfalls in depending largely on retrospective justifications (though see the advantages of working with adults - chapter 5). But even if we are provided with a reliable index of the consciously held belief system at the time of responding, crucial aspects of the working of a "cognitive unconscious" may be missed. It therefore remains possible that the problem posed by the irrelevant transformation is rationally processed rather than ignored, somehow resolved by the decision "irrelevant", and therefore and only then disregarded.

However, among the points that can be made here are the following:-

(1) It is more parsimonious - and therefore more desirable - to try to explain the arrival at the correct answer without recourse to hidden, unconscious processes - that is, if we can "get away with it".

(2) As we are dealing with adults, we might presume that the cognitive apparatus is fully developed. Supposing that the problem is not ignored, we would then want to know why the elicited answer does not acknowledge that there is not enough information.

(3) In the first place we could try to apply the same sort of analysis to those cases where the young child is observed to successfully "conserve" - notably on a task like "0".

With the present concern of trying to arrive at a general level of explanation that links child and adult successes, irrespective of which particular version elicits that success, the last is the most important point. I have already - in Chapter 3 - drawn attention to the body of opinion and evidence (see Light et al, 1979; Bovet et al, 1981; Neilson et al, 1983 a, 1983 b; etc) suggesting that the child's success on a modified C.T. like "O" can be explained without invoking hidden, unconscious, rational processing of information concerning the array and its transformation. Not only this, but the sort of explanation of success that is offered here bears a striking resemblance to the above empirically-supported "gross negligence" explanation of the adult's success on "P".

(b) 7th Empirical Note (young children)

I suggested in Chapter 3 that the main thing of value to come out of the false negative controversy was (a) the introduction of modified
C.1s where the same young child now commonly succeeds, and as an emergent consequence, (b) the "false positive" controversy. The main thrust of the false positive charge is that the successful children in the modified "incidental/accidental" version arrive at the right answer through ignoring/disregarding both the transforming event and its consequences - i.e. by not acknowledging the problem, inherent in the events and circumstances, arranged for them. The criticism of the optimistic interpretation of the results of these tasks - i.e. that they indicate the presence of a reasoning ability - is that the child is able to, and liable to, get them right in the "wrong" way.

The findings of my work with 3 - 5 year olds, doing modified tasks like "O" was fully in line with the evidence these critics-of-Piaget's critics quoted. That is, the representative young child does get the right answer while (by?) apparently ignoring/disregarding the event of the transformation - and its consequences. Again the surprised reaction to E's termination of the episode after Q2 has been answered is seen as especially significant, in particular in relation to the finding of Bovet et al (1981) that the child is left at what will turn out to be the end still waiting for the game to start. This observation is supported by my evidence and would be interpreted in terms of the child having ignored that it might have already started and thus have got beyond the preliminary phase. The subject is seen as having ignored/disregarded that something relevant to the array and what is asked might have happened between Q1 and Q2 (as it might, but actually did not).

(c) Linking the adult (on "P") and child (on "O") evidence

While the form of "O" might be seen to more explicitly present the (actually irrelevant) transformation as an incidental interruption to be therefore ignored as an irrelevance (see section H below), there is nothing to stop the subject treating the (again actually irrelevant) transformation in "P" also as an irrelevance to be ignored or disregarded. The evidence suggests that this is what happens with adult subjects, and may be what happens on those occasions when children succeed on "P", given the observation that these subjects also tend to appear to be, at the end, waiting for "it" to start.
I am in agreement with some of the conclusions drawn on the basis of their evidence as to how young children succeed on modified C.Ts - if not with their undue reliance on justifications for that evidence. But it can be seen that the motivation of the "false positive critics" is very different from mine. In general they are trying to show that Piaget may be defended from the challenge of the optimists: that his procedures and way of interpreting the results of them unduly underestimate the reasoning ability of the child. I am - at least for the moment - merely looking for the best/most parsimonious, empirically supported general level of explanation as to how anyone is to be seen as getting the right answer on any bona fide C.T., taking "P" and "O" as the exemplars. Thus the two specific questions addressed are (1) Can the child be seen to get the right answer on a C.T. in the same way that the adult does? and (2) If so, how are they both to be seen to get the right answer.

As these critics would argue, in cognitive terms this posited way of getting the right answer (i.e. of solving the posed problem) is totally unimpressive. But there again, the gross negligence account of how adults succeed on P is also cognitively unimpressive and for the same reason. We might want to suggest that the child is not capable of rationality. But we do not, applying the same criterion, want to end up denying the adult's rationality on the basis of the manner of his/her correct responding on this strangely empty "task". Fortunately we do not have to. Where the problem is seen as one of recognizing the arranged irrelevance of something and responding appropriately, he/she cannot be faulted for "responding appropriately" by ignoring the problem. But the "problem" in "P" is the same as the problem in "O". Therefore the child cannot be faulted for also making the problem disappear (i.e. solving/dissolving it) in the most economical way on the latter task.

In either "O" or "P" it is as legitimate for the subject to ignore the significance of the (actually irrelevant) transformation event and its consequences as it is for the subject to ignore the (actually irrelevant) rain beating on the window during the episode. From the investigator's point of view, the transformation is supposed to pose a problem. And if the subject does not ignore it, indeed it does. But given the information available, it is one that is incapable of resolution by pure rationality.
The important point is that, even if we are to be dubious about a "gross negligence" account of success on a C.T., there is once more good reason to suppose that the child succeeds on "0" in much the same way as the adult succeeds on "P". While the manner of success in both cases appears to be cognitively totally unimpressive, the fault is more easily seen to lie with the nature of the C.T. (nothing happens) and the way the experimenter presents the "problem". As it stands, the successful subject is able to be seen to "solve" the problem in the best way available.

D. The Paradoxical Consequence of Exploiting the Cognitive Approach

The two aims in the above empirically-supported analysis, as in that of the previous chapter, were:—

(1) to try to establish that the young child succeeds on "0" in the same way that the representative adult succeeds on "P".

(2) to attempt to put forward a psychologically adequate general level of explanation of that success.

The additional specific aim was:—

(3) to exploit the (information-processing/problem solving) cognitive approach in the service of the above two aims, and to see where it leads.

In relation to the third aim, an important general consequence was that we were reminded that the question of problem-solving presupposes the question of problem-posing. That is, whatever the privileged perspective of the investigator might tell him/her about the existence and nature of a certain problem in a certain context, the problem only becomes a problem if/when what was intended as a problem is attended to (at some level) as a problem. Only then may a subject (or his/her cognitive apparatus) be usefully viewed as a "problem-solver".

A lesson from the above analysis is that, if the cognitively-oriented researcher's concern is with a subject's understanding of irrelevant events and circumstances, he/she must be prepared to take the consequences of arranging that the significant events of the experimental context are only "significant" because of their irrelevance. If something (e.g. the perceivable consequence of the
transformation) is arranged to be irrelevant, and is to be reasoned by the subject to be irrelevant, the experimenter is liable to encounter difficulties in ensuring that the subject even notices that there might be a problem related to that event. The subject is then not only liable to ignore/disregard the intended problem, but will, at least in the case of the C.T., get away with it.

The investigator here faces a dilemma. For, on the other side of the coin, any attempt by E to ensure that the subject does attend to the intended problem will be likely to involve manipulating things to make what is actually irrelevant appear relevant. That is, an explicitly deceptive element will have been introduced. I have already pointed out that the introduction of a deceptive elements multiplies the interpretative difficulties.

The tentative conclusion was that young children succeed on "O" in the same way that adults succeed on "P", and that they do so by ignoring or disregarding the significance of the supposedly problematic transformation. It rested on an exploitation of the cognitive approach insofar as a criterion for deciding that a problem has been solved is established (the problem must have been made to disappear) prior to asking: what, therefore, is the best way of solving a problem (i.e. making it disappear)? The response to this - ignore the problem if you can get away with it - proves productive insofar as the evidence as to how C.T. successes do succeed corresponds to this "strategy". And in terms of processing economy, it would seem that such an explanation of conserving responses could hardly be improved upon. However it can be seen that the consequence of the determined application of the cognitive approach here is to undermine the very applicability of that approach.

Such an approach insists on viewing the subject as a processor of available information. Here we are left with a subject who has "processed" the available information by ignoring it. Because he/she can get away with this, the subject is to be seen as totally justified, whatever the actual reasons for doing so in a particular instance might be. If it is claimed that the subject is not justified in ignoring the transformation and its consequences, this might be seen as tantamount to admitting that there is not enough information for the subject to know or work out that nothing occurring during the transformation phase made
a difference to the amounts compared. This is the case. But this too undermines the applicability of the cognitive approach.

Whether we view gross negligence here as justifiable or not-justifiable, the paradoxical conclusion is that the attempt to apply the cognitive approach to the C.T. results considered leads to a denial of its applicability. And if there is a useful insight here it is that, given the degree to which the gross negligence explanation fits the evidence, we can finally abandon the view that the elicited right answer on a C.T. is to be usefully seen as some sort of success. The manner of "success", while faultless in relation to the cognitive demands of the episode, is also — and correspondingly — totally unimpressive.

In the next section it will become clear why the "gross negligence" explanation of "success" will not do. But the above conclusion proves to be a help rather than a distracting hindrance. It can be seen as "liberating" in that we no longer need orient the enquiry towards seeing what "conservers" do right, and "non-conservers" do wrong.

B. The "Gross Negligence" Explanation Rejected, but Pointing the Way Ahead

The unpalatable consequence of the adopted cognitive psychological framework arouses suspicions about the derived "explanation" of success on a C.T. And when we look closer at it we find that the "gross negligence" account must be rejected for the same reason that the "deception explanation" was dismissed. That is, even if the evidence seems to lend support, it is found in the end to be not psychological enough.

I am trying to identify something of significance in the described events and circumstances of the C.T. episode that enables the subject to decide that the answer to Q₂ is "the same", and choose this from the alternative answers on offer in that multiple-choice question. The explanation of success cannot be that the subject ignores the one and only significant event — intended to be the pouring and its consequences. For, if the subject ignores it (as he/she can and may do), then the "significant" event must be viewed as having no
significance for the subject. At least it has no more significance than
the rain beating on the window, or the traffic noise from the street.
And we would be reluctant to say that the subject succeeded because
he/she avoided being sidetracked by the irrelevant (and disregarded)
event of the rain beating on the window during the episode. However, in
a sense this (allowing that there was rain beating on the window) would
be true. And a case can be made, as I have shown, that in a sense it is
ture for the equally irrelevant (and disregarded) transformation.

But, while an explanation in terms of "gross negligence" sounds
unduly negative, we can look for the positive correlate of that
negligence. Thus, if the subject manages to fully participate in the
C.T. episode while ignoring what we thought was its significant aspect,
what else in the arranged episode can we view the subject to be
attending to, and responding to when he/she answers Q2 with the right
answer?

This question will be tackled in the next section with a view to
providing the sought-for general explanation of success (and,
derivatively failure). But two pointers as to the way ahead can be
noted here:-

(1) With the "gross negligence" explanation, what has again emerged
is a crucial perspective disparity between the two participants in the
episode — the experimenter (E) and the subject (S). This takes us back
to the problem of description (see chapters 2, 3 and 4). The difficulty
also raised its head with the problems surrounding a "deception
explanation", discussed at the end of the last chapter. It was brushed
over as it appeared that, while E and S could not be presumed to agree
on the temporal aspect, everyone could at least agree that the
transformation occurred as something to be made sense of. But with the
empirically-supported, posited "gross negligence" account of success, it
emerges that this may not be true. Thus it becomes necessary to deal
again, and more rigorously, with the "perspective disparity" problem.

To the important points of view of (a) the E, and (b) the S, we can
add that of (c) me, the present investigator. The first is important
because it is E who decides and arranges what will take place. The
second is important because it is the subject's conduct that needs to be
explained psychologically. The third is important because it is me who
is trying to provide the framework to adequately explain that conduct.
And I have the advantage of being able to try to embrace both the other
two perspectives in an overall perspective. I may then be in a position
to derive a perspective (i.e. description of the C.T. episode) that eliminates the identified contradictions or discrepancies between the perspective of E and that of S.

So, instead of grappling with the problem that the perspective disparity presents for description (and hence interpretation), I view it now as a feature that should figure in an adequate description of what occurs (along with the other events) when a C.T. takes place. Having made explicit those areas where disparity exists, it should be possible to discern what we are left with - that is, the components of the episode that I, the E and any S can all agree about. As I have shown, it is not just the subject who "fails", but also the subject who "succeeds" who fails to agree with the E about the significance of the transformation. For the former it has a different significance, and for the latter it may have, and seems to have, no significance. Hence we must look elsewhere for a convergence of perspectives (see section F).

(2) While the "gross negligence" explanation undermined the applicability of a problem-solving (i.e. cognitive) approach, it is not quite true to say that ignoring the supposedly significant event and its consequences leaves S without a problem. The evidence from both children and adults indicates that subjects frequently find answering Q1 a problem. But it is one that has to be resolved, or assumed to have been resolved, correctly for the conservation experiment to take place at all. Later E asks S the question, Q2, and at the very least requires an answer, chosen from one of the available alternatives, from S. As E intends, his/her arranged episode elicits a response of "answering conduct", and the S further complies - or behaves as predicted - by answering in an approved way, that is with one of the available alternatives.

Points (1) and (2), considered together and arising out of the rejection of the "gross negligence" account, have the effect of focusing the investigation on the questions asked by E.

F. Exploiting the Notions of "Genesis" and "Psychological Structure"

I have attempted to exploit the cognitive component. With the emphasis still on explaining "successful" performance, I now proceed to exploit the other two identified components of Piaget's approach to explaining an observed instance of intelligent behaviour: the genetic
and structural components. The outcome of this strategy (1) is to confirm that the role of the questions deserves independent consideration. (2) guides me to a productive view of what sort of significance the questions should be given in the general level of explanation sought.

(a) Genesis

A genetic approach to explaining observed instances of successful performance entails that the investigation addresses the question: where do right answers come from? Piaget would insist that, as an instance of adaptive intelligent behaviour, an explanation of it will not be complete unless this question is answered satisfactorily. And a Piagetian, progressive - evolutionary application of the genetic approach involves acknowledging that somehow right answers come from wrong answers. This must be so insofar as - on a "necessitarian progressivist" (Boden, 1979) account - less successful solutions will precede more successful solutions. However, what I have established is that, with respect to the C.T., talk of "solutions to problems" may be misleading, and the terms "success" and "failure" may be inappropriate. Consequently the labels "right" and "wrong" here, while not unintelligible, are viewed to have an application of only limited usefulness. This raises difficulties for the above type of progressivist genetic account.

Fortunately this and other difficulties that a Piagetian genetic approach might encounter can be by-passed without abandoning the genetic orientation. For, in the first place, a right answer - like any answer - comes from a question. If the conduct that is our concern is to be seen as an answer, there must be, somewhere, a prior question to be taken into account. And the "answering behaviour" can be viewed to have its origin in this question. Thus before investigating, for example, how the ability to make relative judgements about volumes and amounts emerges in development, the suggestion is that performance on the C.T. should be directly related to the question that is asked, without which there would be no performance to explain.

This sort of genetic orientation therefore reinforces the point made in the last section, in relation to the cognitive orientation. At some juncture proper attention must be given to the significance of the
questions asked by E, irrespective of what else E arranges and does in the C.T. episode. This leaves the notion of psychological structure to be reconsidered.

(b) Psychological structure

For Piaget the question of genesis is inseparable from that of structure, and the explanation of an observed instance of intelligent behaviour is not to be considered complete until it is linked to a corresponding psychological structure. This associated structure is seen as a cognitive one, residing in the subject's head, and enabling the conduct in question. The question that Piaget would tend to ask is therefore: What sort of structure lies behind the observed instance of intelligent conduct?

Merleau-Ponty (1962, 1965) provides us with a very different approach to psychological structure. This is especially relevant since the very applicability of the cognitive approach in explaining C.T. results has been thrown into question. Merleau-Ponty is not primarily concerned with cognitive processes or structures, the focus of his investigations being on what is presupposed by invoked mediating operations: that is, un-mediated, "pre-reflective" meaning (see Appendix II). Nor would he view psychological structure occupying an internal realm inside the head, enabling adaptive responses to the environment. For him conduct is not to be seen as contingent either on events inside the head, or on events in the external environment, even though physiological and external circumstances and events will provide conditions for, and constraints on the range of conduct that can occur. They are to be seen as "situating" the subject, rather than determining his/her behaviour.

Any "world" that is referred to (even that of the most abstract and exact science) is always a world from someone's point of view (even if this happens to be a widely shared one). And to understand what a subject does, the "environment" with which we are in the first place concerned is the world for that subject. Merleau-Ponty's starting point for psychological analysis is therefore the subject's initial, direct (or as if direct, if we are unhappy with the notion of direct experience) encounter with the particular, concrete situation. And he points out that we cannot talk of this or any level of encounter between
subject and world without allowing that it always will have some meaning for the subject ("We are condemned to meaning"). We cannot talk of the contents of perceptual experience in terms of "raw data" - inherently meaningless units of sensation awaiting the imposition of some organising form or structure (Merleau-Ponty, 1962, pp. 3-12; see also Rojcewicz, 1987 - quoted in Appendix II). Merleau-Ponty insists that the contents and form of experience are indivisible. Whatever level of experience we address in our psychological enquiries, it must always be assumed to be structured in some way - even when what the subject is aware of is confused and unclear. Correspondingly, any change in organising "form" brings with it a change in the "contents" organised.

In relation to conduct, the initial project for psychology is to describe it in an appropriate way. For Merleau-Ponty, psychologically-relevant description involves relating observed behaviour to the structure of experience - the meaning for the subject. And, in the first place, a fundamental psychological enquiry should undertake to relate behaviour to the spontaneous organisation arising from the always already-situated subject's initial, pre-reflective encounter with the world. This initial, "lived" encounter of an involved (as opposed to a detached, cognitive) subject provides the occasion for all meaning. From it all meaning is to be taken to derive (see Appendix II).

The important point here, and that which enables Merleau-Ponty's approach to be compared and contrasted with that of Piaget, is that while we are still concerned with the question of psychological structure, its primary rôle as a conceptual tool is in relation to the project of describing conduct, not explaining it. While Piaget addresses the question of the psychological structure behind and enabling some observed conduct, Merleau-Ponty points out that the prior "structural" question is that of the psychological structure in and conveyed by that conduct.

In relation to the C.T. situation, what is sought is a correct and primary characterization of the sort of conduct exhibited by the subject. In line with the above discussion, we consequently require a correct and recognizably primary characterization of the sort of situation the subject participates in. Now, how the C.T. situation is to be characterized is, as I have shown, not just a matter of disagreement between the E and the S - the latter who, for example, may either not see the transformation as irrelevant, or may, equally
justifiably, not agree that anything significant has been transformed. It is also the occasion for dispute among investigators (see chapter 3). But, whatever else the arranged episode is or might be claimed to be, we can presume that the S encounters it — and that it can be described — as an "interrogative context". It is, in the first place, the sort of situation where questions requiring answers are asked. And the conduct exhibited by the S is — in the first place — that corresponding to a question-and-answer situation.

So once again we are drawn to the emerging importance of E's questions. And now we must explicitly acknowledge that these questions do not just refer to the context to be responded to, but are events constituting part of the whole context to be responded to.

With the C.T. viewed in these terms the project becomes clearer. Whatever aspect of the subject's conduct during the episode we are interested in — whether it is that which needs to be explained or that which is to be used (as evidence) to explain some other aspect — we (1) should not view the various aspects in isolation from each other, and (2) should, in the first place, relate the conduct in question to the subject's whole response to the situation viewed as an interrogative context. The aspect of conduct of primary concern is of course the subject's answer to Q2. But it is now even clearer that this is not to be isolated from what we can observe (or infer) as to the subject's behaviour and expectations before and after he/she answers. In short we are concerned with how the subject comes to, is in and leaves the arranged interrogative episode. In this respect it is significant that I have already dwelt on the fact that the subject of a C.T. comes to the episode expecting that something interesting will happen/be asked (when nothing will), and on the subject's reactions to E's termination of the episode.

The conduct I am currently specifically interested in is successful performance. And so far in this chapter I have made the points: (1) the subject can "get away with" ignoring what were supposed to be the significant aspects of the episode, involving the array and its transformation, but that (2) the subject cannot get away with ignoring that questions requiring answers are asked. In these respects the investigator can follow the successful subject's example.

In the end, in setting about the task of explaining observed performance (success and failure) on the C.T., I am therefore not
content just to acknowledge that the subject's responses occur within an interrogative context. I set out to investigate and explain the subject's conduct in the situation viewed SOLELY as an interrogative context.

(c) The C.T. described solely as an "interrogative context"

Any description of the C.T. episode must correspond to what the experimenter (E) arranges. From what E has arranged to take place in version "P" or "O", he/she knows:-
(1) What the circumstances are and what will happen, in relation to the presented array.
(2) that nothing that happens will have any relevant effect on what is asked about - i.e. the initial and subsequent relative amounts of water/powder in the glasses containing these amounts.
(3) that two identically-worded questions (Q₁ and Q₂) will be asked in sequence
(4) roughly how long the episode will last and that the elicited answer to Q₂ will mark the conclusion of the episode.

The only feature listed here that has a bearing on the episode as an interrogative context is item (3). Therefore this is to become, not just the basis of the description, but the description itself - to the exclusion of all else.

We can also note that this is the only item that the subject can agree with E about. Nothing else in the list can either be taken as given to the subject, or as able to be deduced by the subject, whatever his/her level of reasoning ability. Hence a major advantage of describing the episode in this way is that it eliminates the perspective disparity that has dogged the investigation to date.

The major disadvantage would seem to be that concerning ourselves only with item (3) presents us with an unduly impoverished description of what takes place. This throws into question whether such a description could prove psychologically useful when it comes to explaining performance on a C.T. However, while it may be an impoverished and - I would accept - blatantly biased description of what occurs:-

(a) it does not contain any descriptive inaccuracies, given that we are dealing with any bona fide C.T., i.e. where two questions are asked
(unlike in Rose and Blank's, 1974, version), and where they are identically-worded (see Donaldson's characterization of the C.T. - 1979, p.62-63).

(b) the resulting impoverished nature of the description is legitimizied by the basic design of the C.T.¹⁰

(c) that it is legitimate is not just indicated by the facts, (i) that it is not inaccurate and (ii) that E, S and any observer have no problem agreeing about item (3) in the above list. In the end it is legitimate by virtue of the fact, (iii) that it proves productive from the point of view of arriving at the sought-for parsimonious, psychologically-relevant general explanation of the C.T. results.

G. A General Explanation of the C.T. Results

(a) From description towards interpretation: what sort of interrogative context?

In any bona fide C.T. such as "P" and "O", two identically-worded questions requiring answers are asked in sequence. This description of what a C.T. is to be taken to consist of was the result of the empirically-supported investigation described in the last three chapters. I have presented this view of the task as emerging here from the exploitation of the key components in Piaget's "cognitive-genetic-structuralist" approach. Having, on the basis of this, chosen to describe the C.T. as an interrogative context to the exclusion of all else, the next question is: what sort of an interrogative context are we dealing with?

The simple answer is that it is an ambiguous interrogative context. On the one hand the second question may be justifiably heard as a pointless or - at best - memory-jogging repetition of the first question. On the other hand it may, equally justifiably, not be. In the latter case it will be heard as having a point, suggesting to the subject that, for example, something relevant to answering the question must have happened between the first and second asking of the question, or that he/she was wrong in the answer given the first time it was asked. More generally, if S justifiably does not take Q₂ to be a
pointless repetition of Q₁, he/she must presume that E has a reason for "repeating" the question. Unless E's intended point is seen to be merely to jog S's memory (or confuse S?!), then it is likely to be seen by S to be that Q₂ requires a different answer than the one already given to Q₁. Why this should be, and what E's reason for "repeating" the question actually was, are immaterial - so long as E could have a reason other than to jog S's memory (as he/she does) and as long as the correct answer to Q₂ could be different from the answer already given to Q₁ (as it could - e.g. see Chapter 7, section (E)).

Two identically-worded questions asked in sequence will always present an ambiguous context if the questioner's reasons and intentions in asking them are not made clear to the one being questioned. A similar point is made in the "Prelude" to this work. It can now be recalled that Rose and Blank (1974) focused on the implications of one aspect of the inherent ambiguity. As I discussed at the end of chapter 3, they pointed out that a subject, hearing the identically-worded second question, is justified in supposing that there is a point to it, and that something relevant has happened to make the approved answer to Q₂ different from the one already given to Q₁. What they neglected to point out is that the subject is also justified in supposing that there is no point to it; that is, in taking Q₂ as a pointless (or memory-jogging) repetition of Q₁, and thus in merely reiterating the approved answer already given.

(b) The sought-for general explanation: Question C answered

In section A of Chapter 4 I posed the question, Question C, that needed to be addressed before Questions A and B could be tackled. This was: "Given what C.Ts have in common, what is the most parsimonious, psychologically-relevant and empirically-supported general account of that which determines whether a subject, irrespective of his/her age, will succeed or fail on any version of a bona fide C.T.?”

Adhering to a description of the C.T. episode solely as this sort of interrogative context, the sought-for general level of explanation that can answer this question is as follows: - The "conserving" subject succeeds because he/she justifiably takes Q₂ as a pointless (or memory-jogging) repetition of Q₁. The "non-conserving" subject fails because he/she, equally justifiably, does not.
This fulfils the criteria of parsimony. In terms of the features common to any bona fide C.T., the only property mentioned is the fact that the episode contains a question asked twice. Further, it is important to note that S is always already assumed to have given the answer "same" to Q₁. Otherwise things could not proceed, and hence operate and be viewed as a C.T. In terms of processes that need to be invoked, all that S is therefore required to do is recognize that Q₂ is identically-worded to Q₁, and remember that the approved answer to Q₁ was "same". Such "recognizing" and "remembering" is sufficient to account for both a "conserving" and a "non-conserving" answer, as the reasonable response of anyone. For the S is justified in supposing, (1) that, because E has asked two identically-worded questions in sequence, Q₂ is to be taken as a pointless repetition of Q₁, and therefore requires a repetition of the answer already given to Q₁; or in supposing (2) that E would not ask two identically-worded questions in sequence without there being a point to it. Therefore Q₂ cannot be meant to be taken as a pointless repetition. Hence the answer required must be something other than the one already given to Q₁.

Should we want to try to identify a problem or contradiction here, it is that both subjects are right. Q₂ is a "pointless repetition" in that it refers to the same, unchanged entities and requires the same answer as that assumed already given to Q₁. There again, there is a point to asking Q₂, the point being exactly that it does refer to the same, unchanged entities and requires the same answer as that assumed already given to Q₁. But it is E who has a monopoly on the "point" here—supposing that something interesting might come out of asking the subject the same question twice while making sure that nothing that affects the answer happens.

For me, looking on and concerned with the issue of parsimony, the unintended "point" to be derived from what E does is that each of the responses available to anyone is to be explained in terms of the same fact—that two identically-worded questions are asked. No consideration of what the question(s) might be supposed to refer to is necessary. In terms of the psychological processing called for to get the right/approved answer, the subject does not have to know or bother what the question refers to, or, indeed, suppose that it refers to anything concrete. From the successful subject's point of view:—"E
asks me (S) a question, only proceeding when the approved answer, x, is elicited from me. E later asks me the same question again. Therefore the answer must again be x - whatever the terms in the question refer to, ' and whatever the answer means."

(ii) Psychologically relevant?

It is psychologically relevant in that, as noted above, behaving and answering appropriately (with a "conserving" or "non-conserving" response) is seen to involve "recognizing" and "remembering". More importantly, having employed a description of the "task" that removes the perspective disparity, everyone (S, E and me, the investigator) can, in principle, agree about what is to be recognized and remembered. With the episode viewed solely as an interrogative context, "facts" over which there could be disagreement between S and E are eliminated (see chapter 8 for further discussion).

The general explanation offered is therefore psychologically relevant because it refers only to information available to the S, and events and circumstances that can always be assumed to have made some psychological impact on the S - rather than none. For the explanation of conduct to be properly psychological, it is desirable that subjects be taken in the first place to respond on the basis of what they do attend to/see/know/realise, etc. This is so even in those cases where what they see "does not make any sense", and what they realise is that they "do not have a clue what the right answer is". Here the form of the questioning can serve as a "clue as to what the right answer is" for any subject, in any C.T. On its own, however, it is a wholly ambiguous clue.

(iii) Empirically-supported?

The "fit" between this offered general explanation and the already-quoted evidence is mentioned below, in sub-section (c). The way "conservers" are observed to "succeed" supports, or at least does nothing to disconfirm, such an account. Children frequently succeed on "0" and occasionally succeed on "P", while adults customarily succeed on "P". And they can all be argued to do so by giving a repeated answer to what is heard as a repeated question. The evidence from non-conservers - notably the frequent failures of the young child on "P" and
less frequent failures on "0" - confirms that Q₂ here is not heard as a pointless or memory-jogging repetition. We now need say no more about "failure" than that.

Of course, it is difficult to see how the account of "success" could fail to correspond to the evidence obtained from "conservers". After all the strategy for arriving at the descriptive framework appropriate for such an account was, in a sense, borrowed from the observed "strategy" by means of which successes were seen to succeed. They were able to get away with ignoring the significance of what have been seen as the significant aspects of a C.T. - but did not ignore the questions asked. Again, it is difficult to see how the matching account of "failure" could fail to correspond to the evidence from non-conservers. If a different answer is given to the second identically-worded question, one would assume that it cannot have been taken as a "pointless" repetition of the first.

In terms of additional evidence, it merely needs to be established that the subjects, whether successes or failures, (1) recognize that Q₁ and Q₂ are questions and are worded identically, and (2) remember the answer they gave (or were given) to Q₁ (always "same")."² Given the time scale, it can be assumed that all the subjects concerned are capable of the relevant feats of memory and discrimination. Following what was said on the issue of parsimony, it is not even necessary to establish that the subject understands what the questions mean. He/she only has to understand that they occur and are worded the same.

The only obvious missing evidence is that from adult "failures" on a bona fide C.T. This is supplied in the following chapter and does nothing to disconfirm the offered answer to Question C.

(iv) Generality?

The answer offered to Question C is by definition and design a general one. It starts from an account of "success" on any bona fide C.T., from which the account of "failure" is derived in the simplest way and stated in the most general terms. This involves the substitution of a single negative in the account of "success" ("If Q₂ is justifiably heard as a repetition ... " versus "If Q₂ is justifiably not heard as a repetition ... "). To have a general application, whatever else is involved in a particular version of the C.T., to be a bona fide version the episode must involve the same identically-worded question being
asked twice, with nothing actually happening to alter what is the correct answer.

Just because of its generality, it does not yet explain particular instances of success or failure, on particular versions of the C.T, and relating to particular subjects belonging to particular age-groups. But its value lies in its capacity to direct us to those aspects which will explain and predict particular instances of success or failure. Such specific accounts will be seen to fit comfortably within the framework provided by the derived, albeit impoverished, description of the C.T. as an interrogative context.

Perhaps other accounts of performance can be thought up in particular instances. But given the parsimony, psychological relevance (especially with the removal of the "perspective disparity") and general applicability of this level of explanation, we would want to retain it so long as the relevant evidence did nothing to disconfirm it. Let us therefore look again at the already-quoted empirical evidence.

(c) Implications for making sense of the quoted evidence

With respect to the quoted evidence, the sort of explanation to which this impoverished descriptive framework points looks promising. The subject who takes Q₂ as a mere repetition of the preliminary phase question, Q₁, would be predicted to (1) show surprise when E terminates the episode after Q₂ has been answered, and (2) give an indication that the possible significance of the events of the transformation phase has been ignored. If the subject is to be viewed as reasoning about anything, we could only suppose that he/she reflects on what might be going to happen. The unsuccessful subject, not taking Q₂ as a repetition, would be predicted to (1) not be surprised at the "termination", and (2) show that the possible significance of the transformation has not been ignored. If he/she is to be viewed as a reasoning about anything, we might assume that he/she would reflect on what made the answer to the second question different from the initial, approved answer. And there are plenty of possibilities for the subject to think about, if he/she so chooses.

Such an "explanation" may be (1) supported by the empirical evidence from the subject's observed conduct, (2) the most parsimonious
available, (3) psychologically-relevant, and (4) correct. But, as it stands, it is post-hoc and, as I stated above, does not predict and explain these and other specific instances of success and failure on particular versions. At the moment, the explanatory power of such an account remains to be demonstrated. The sort of question that must now be addressed is: why should the child on "Q", and the adult on "P", hear $Q_2$ as a mere repetition, whereas the child on "P" (what our descriptive framework insists is essentially the same sort of episode) does not? And, with answers to such questions, we may also have adequate answers to Questions A and B.

The latter question - the "developmental question" - will be addressed in the next chapter, while the former will be answered at the end of the present one. Before that I will mention those features occurring in the presentation of particular C.Ts that are now able to be sought out as the factors making it more likely that $Q_2$ will be heard as a mere repetition of $Q_1$. Where these features are found to be present, successful performance will be predicted to be more likely than in those instances of presented C.Ts where they are absent.

H. "Extraneous" Factors That Make Hearing $Q_2$ as a Repetition - and Hence Successful Performance - More Likely

A C.T. episode is now to be described as involving two identically-worded questions asked in sequence, where the second question ($Q_2$) may or, equally justifiably, may not be taken as a mere repetition of the first ($Q_1$, always assumed to have been answered with "same").

This applies to any bona fide version of the C.T. and, indeed, serves now as the generalised description of what the bona fide C.T. consists of. But, as it stands and other things being equal, a subject faced with this wholly ambiguous interrogative context - where something might or might not have happened to make the answer to $Q_2$ different from the approved answer to $Q_1$ - is as likely to "succeed" as to "fail" on $Q_2$. 
To answer Q₂ the subject may bring knowledge or opinion to bear on whether or not anything relevant could have happened to the array referred to by the questions. But because of the descriptive and explanatory framework adopted, the role of this knowledge or opinion is now ruled out of consideration - at least until such time as the adopted framework is found to be inadequate to explain and predict the results of particular C.Ts.

To explain any instance of success on a C.T., I am now therefore committed to confine the search for contributory factors to those events and circumstances extraneous to and independent of those concerning the array and E's manipulation of it. The only crucial events now acknowledged are the questions asked by E. The search is therefore on, in any chosen instance of success, for something (unconnected to the array and the subject's correct/incorrect understanding about it) that might be seen to make it more likely that Q₂ will be taken as a mere repetition of Q₁, than if that feature had been absent.

Where successful performance occurs, we might anticipate one or more of the following features to be also present:

1. something prior to that C.T. - e.g. in the general context, or in what E says to introduce the task - that is likely to make S unsurprised by being asked a question which is followed by its mere repetition.

2. something occurring during that C.T. - e.g. in what E says - that is likely to tell the subject that the second identically-worded question is merely a repetition of the first.

3. something in the way Q₂ is asked that makes it sound like a repetition of Q₁.

4. something, e.g. in what E says, that is likely to wrongly suggest to S that the interrogative context is still, at Q₂, only at the preliminary phase.

The last anticipated feature can be seen to constitute an indirect factor. But it is an important one and worth discussing as: (1) it provides a link with some of the things that were said about the temporal aspect of the C.T. in chapter 5, and (2) together with the second factor (above), contributes to the answer to be offered to Question A, below.
The temporal aspect re-considered:

Qi occurs at the start of the episode. It must be assumed to have been answered with the response "same" if the episode was able to continue and if it was able to be considered as a bona fide C.T. Subjects, when they see that it is to continue, can be presumed to view Qi as a scene-setting, preliminary-phase question with the point they were "told" to expect lying ahead. Now any subject, who - for whatever other reason - justifiably hears the identically-worded Q2 as a mere repetition of Qi, will therefore incorrectly hear Q2 too as a preliminary-phase question (in effect the same question) with more to come and the anticipated point still lying ahead.

Conversely, anything that - for whatever other reason - misleads the subject to suppose, at the point where Q2 is to be asked, that this interrogative context is still at the preliminary phase\textsuperscript{1} and that there is more to come (including the anticipated question with the point) will make it more likely that Q2 is taken as a mere repetition of Qi. Should the subject - for whatever reason - have correctly believed that the episode ends with and culminates in the answer to Q2, he/she would have been likely to (correctly) suppose that Q2 was the crucial, anticipated, point of the question-and-answer situation and hence be unlikely to hear it as a mere repetition of Qi. He/she would therefore be less likely to merely repeat the answer to the preliminary-phase question, Qi.

This is not to try to bring back the temporal aspect in a central explanatory role with respect to performance on any C.T. Reasons for refraining from doing this were given. Information about duration is missing; besides, to consider the property of duration on its own misses the point that in a C.T. events occur even if, as is the case, we end up with only "question-and-answer" events to consider. However it is now possible to view the temporal aspect (i.e. whether or not S presumes there to be still something significant to come at the point where Q2 is to be answered) as a potentially important "extraneous factor" in relation to whether or not Q2 is likely to be heard and answered as a repetition.

The perceived "temporal duration" aspect is so bound up with the perceived "repetition/non-repetition" aspect that it may be misguided and unnecessary to continue to try to separate them. If, in any instance of C.T. success, we instead enquire into the factors that make it more likely that Q2 will be heard as a mere (preliminary-phase)
repetition of the preliminary-phase question, Q₁, then this manages to combine the two aspects without compromising the analysis as one solely in terms of the C.T. as an interrogative context. Question-and-answer episodes always have duration and this one has a preliminary-phase identifiable to anyone, including E and both the successful and unsuccessful subjects.

With this in mind, let us at last address and answer Question A.

I. Question A answered

Why does the representative young child fail on "P" while succeeding on "O"? This is Question A. But it now becomes, in the light of the adopted explanatory framework: why should it be more likely that the subject concerned hears Q₂ as a mere repetition of the preliminary-phase question, Q₁, on version "O" than on version "P"?

Following the trend established in the last two chapters, I focus on the instance of successful performance, whence the account of the instance of failure is derived.

Referring to the potential factors mentioned in the previous section, it can be assumed that the general expectations of subjects coming to each of the tasks is the same. All subjects are taken from the psychology department nursery and invited to "come and play an interesting game". Also we might here assume that the way E asks the standardised question, Q₁, is kept constant with respect to intonation. Observations as to the way C.Ts have been run by previous researchers, however, indicate that this is by no means always the case - further discrediting the sort of interpretations of version-dependent differences that have been offered. Fortunately it is not necessary to bother about these and other potential, but hard-to-measure discrepancies. For it is found that, in "O", E actually says something that can be seen as explicitly telling S that Q₂ is a repetition of Q₁ - calling for a repeated answer.

It is now that we can recall that (see Chapters 1 and 3) that a diagnostic (though seemingly, universally unacknowledged) feature of "incidental" (including "accidental") modified versions of the C.T. is the insertion of a third question, Q₃, immediately prior to Q₂. Being concerned with the C.T. solely as an interrogative context, we cannot afford to ignore this. After the "unintended" transformation that
disrupted or interrupted the "intended game" has been brought about by E (or "Naughty Teddy"), E says "where were we?", or "what were we saying?", or something similar.

"Where we were" was at the preliminary, scene-setting phase. And "what we were saying" was the preliminary-phase question and answer. The response of anyone to such a question could be assumed to be to consider what happened back at the start of the episode. However, it turns out to be a rhetorical question which E proceeds to start answering, reminding S that what happened was that E asked him/her a question requiring an answer: "Ah yes, [I was saying] is there more in A, less in A or the same in each?" E then waits for S to say something. As in any C.T., it would be understandable for S to suppose that there is a point to E asking this question again, and that something deceptive is going on (as it is). But in this instance it is particularly understandable that S should not take Q2 like this, rather hearing it as a memory-jogging reiteration of what E said (Q1). As such, it sounds like a request for S to remind E what the already-approved answer to the question is. Remembering that this was "same", S repeats this answer to E's repeated question.

Thus the conjunction of Q2 and Q2 that occurs in version "0", but not in "P", is seen as constituting an explicit request for repetition of the answer already given to Q1. And taking Q2 - with or without Q2 - as such a request is of course entirely justifiable. After all Q2 is the same, identically-worded question as Q1, and does happen to refer to the same entities as before, even if there is actually not enough information for S to know this.

That the subject involved should, as we have seen, answer Q2 (1) waiting for the episode/game to start, and (2) disregarding both the transforming event and its consequences, is entirely in line with saying that the subject succeeds on "0" through (3) taking Q2 to be a mere repetition of Q1. And we can see why the insertion of Q3 should enhance the likelihood of achieving all three effects.

The fact that the successful subject is mistaken as to the duration of the episode and deceived into thinking it goes on beyond Q2 is now able to be seen as irrelevant to the account as a psychological one. Similarly, the adopted general explanatory framework rules out reference to the array and the subject's correct/incorrect understanding in relation to it (including what S understands is able to be disregarded). Such aspects are merely to be viewed in terms of the background of
deception and lack of information, against which the subject responds. I can now, therefore, answer Question A adequately, while remaining bound by the impoverished description of the C.T. solely as an interrogative context.

While "successful" and "unsuccessful" performances on both versions of the C.T. always occur against the same background of deception and lack of information, and are therefore equally justifiable, the subject involved gives the right answer to Q₂ on "O" while giving the wrong answer on "P" because there is an explicit request for the repetition of the answer "same" present in the former version and missing in the latter.

A major point made in the following chapter is that with respect to performance on "P", we may be able to say no more than that it just happens that the "subject involved" is the 3 - 5 year old. It might have just happened that we were still dealing with an intact version of "P", still referring to an instance of "conserving failure", but that the "subject involved" was an adult. It can happen that it is the older subject who is influenced by corresponding "extraneous factors" to not take Q₂ as a mere repetition of Q₁. As it happened, however, it was the evidence from adult conservers that was central to deriving an account of the minimum that need be involved in any instance of "conserving success", and hence laid the ground for a parsimonious, though - as it turned out - conspicuously trivial answer to Question A. That the derived general explanatory framework was such an impoverished one, and the fact that such a supposedly important question (e.g. see Chapter 3 for a measure of the fuss made over it) can be answered in terms that are psychologically trivial, should have already made us sceptical about the ability of any C.T. to help resolve any significant developmental issue.

NOTES

'See, for example, the behaviourist approach, viewing conduct in terms of discrete, operationally-defined units, limiting itself to observable response output, and eshewing the invoking of hidden psychological structures.
"The solution of a problem - any problem - consists in discovering how to transform an existing state of affairs into a desired one that has not yet come into being" (Donaldson, 1978, p. 15). Perhaps so. But "transforming the state of affairs into a desired one" cannot be seen as a "solution" if the problematic aspect of the original "state of affairs" is not made to disappear.

Solve: break, clear up, resolve, dissolve. (S.O.E.D)
Resolve: melt, dissolve, disintegrate, solve, disperse, dissipate. (S.O.E.D.)
Dissolve: disintegrate, cause to vanish, bring to nought, terminate the existence of, solve, cause to fade away. (S.O.E.D.);
cause to disappear (Oxford Paperback Dictionary).

At least, there will be no contradiction in relation to the amounts I have been asked about - correctly arriving at the answer that there is the same in A and C. As I have shown, the fact that the episode now suddenly ends will contradict my belief that there was more to come.

Apart from the fact that it does not (normally) take place in a pub, I would argue that this is an apt description of the C.T.

Subject 1: "But perhaps it depends on you as well. If I saw that you were really interested, and that this really meant something to you, that you really wanted to know the answer ... never mind if it was in a pub, or here."

An analysis of E's role in the presenting of "P" is given in Chapter 7, section F.

What has been said in the last three chapters raises doubts as to whether "successful" performance can be viewed as any more "intelligent" than unsuccessful performance. But, from what I said at the end of the last section, there can be no doubt that both types of performance are able to be considered as instances of "intelligent behaviour". Right or wrong, the subjects answer, and answer appropriately (that is, "intelligently") from the alternatives available.

It is not merely a matter of temporal succession. Wrong answers give rise to conflict in the subject's understanding; conflict has been called by Piaget "the motor of development"; development (cognitive) culminates in the appearance of correct solutions/right answers. Hence right answers can be viewed to arise from wrong answers.

Likewise, the results of such a task may call for an impoverished (i.e. trivial) explanation (see later).

An obvious exception here would be if the question referred to something that was time-dependent, e.g. if \( Q_1 = Q_2 = \text{"What time is it?"} \) If we assume - along with other investigators - that the right answer on say, the volume C.T. is not time-dependent, this exception presents no problems. If we do not assume this and acknowledge, say, that liquids with time decrease in volume through evaporation, and that the rate of evaporation also depends on such things as movement (viz. pouring) and surface area (viz. the fact that container C has a greater diameter than A), this merely reminds us that there are many interesting possibilities in the episode, and therefore various
reasons why the answer to $Q_x$ might not be "same" - that is, given the information available to and withheld from the subject.

"We would also, naturally, need to establish that S can and does hear the E's questions.

"This does not exclude the possibility for S that what happens between the asking of $Q_i$ and $Q_x$ is something that suggests that he/she gave, or was tricked into giving, the wrong answer to $Q_i$.

"Adding the privileged perspective of the investigator, a C.T. is now to be taken as an episode where it is arranged that nothing relevant happens except two identically-worded question requiring answers are asked in sequence.

"Note that I am still able to disregard events and circumstances relating to the array, and S's correct/incorrect understanding of them, focusing rather on "extraneous" aspects: "extra" things that E does and says."
A. Introduction

The difficulty at the source of the conservation controversy was initially identified as a descriptive one (see chapters 2, 3 and 4). The last three chapters can be seen as an attempt to resolve this difficulty and arrive at a legitimate and useful way of viewing and describing any bona fide C.T. The descriptive framework that resulted promised to provide the basis for an adequate explanation of all C.T. results, and for answering the two questions, Questions A and B, at the heart of the controversy.

Question A might be seen as a testing ground for the sort of explanation that follows from this adopted framework. Such an explanation needs to account for why the subject concerned - the representative young child - gives the wrong answer on "P" but the right answer on "O". Piaget was neither interested in the results of "modified" C.Ts like "O", nor in Question A. He would have correctly claimed that "O" could not be viewed as a test of reasoning ability. In this case his mistake was to assume that "P" could be. The optimists were interested in Question A and sought to explain why the child fails on "P" through showing that he/she tends to succeed on "O", and through then trying to indicate what the essential difference between the two tasks is. One common mistake here was to assume that because a modified task like "O" achieves a higher proportion of conserving judgements it must be a better test of the subject's rational ability than the classical version.

Employing the adopted framework for explaining the findings entails deciding that there is no essential difference between the two tasks. Those researchers who assumed there was can be charged with overlooking crucial evidence - not just that successes on either version succeed in
the same way; but that the reason for that success in any instance can be found in the fact that Q₂ can justifiably be taken as a mere preliminary-phase repetition of Q₁. To explain why the subjects concerned are more likely to "conserve" on "O", we are prompted to seek some "extraneous" event tending to accompany the presentation of "O", but absent from "P", that makes it more likely that Q₂ would be heard as a pointless or memory-jogging repetition of Q₁. Sure enough, this event was identified as the additional, unacknowledged "where were we?" / "what were we saying?" question asked by E. This, not by coincidence, also exerts an influence on the temporal aspect in that it leads the subject to incorrectly anticipate that there is more to come. We have an explanation of why the subject concerned would be more likely to "fail" on "P" than on "O". But it only seems to apply when the "subject concerned" is the representative young child. That is, the explanatory framework does not yet tell us why it should be children that give the wrong answer on "P", while adults "succeed" on the same task. Traditionally of course it has been this "failure" that has been viewed as the significant result to be explained. This was because of what seems to be its indisputable developmental significance. And, while I have shown that it is inappropriate to talk of "success" and "failure", it is the case that there is a marked correlation with age in the manner of responding on a classical C.T. like "P". Thus, it might seem, there is still a developmental - in the broad sense of the term - issue to be resolved. Younger subjects respond one way while older subjects respond in a different way in what is arranged to be the same situation. And to explain this fact is to answer Question B, the "developmental question".

The aim of the present chapter is to show how the strategy and derived general explanatory framework that proved its value in allowing Question A to be answered, also enables Question B to be answered. One problem with focusing on failure was that there are always going to be a variety of possible reasons why someone might get the wrong answer on a C.T.

However, what I have shown is that an initial focus on why a subject (when he/she does) gets the right answer allows this difficulty to be by-passed. Now there is no reference to the array needed, nor to the subject's correct/incorrect understanding in relation to the array. The concern now is only with an aspect of the questions asked.
So, in addressing Question B, it is again the successfully conserving subject—in this case the adult—that will be the primary concern. As for the case of the young child's success on "O", an "extraneous" event or aspect will be sought to account for the adult's success on "P"—that is, for the fact that he/she tends to hear Qₐ as a mere repetition of Qₜ. While there is no explicit "where were we? / what were we saying?" request for repetition here, something having the same effect is anticipated.

Again an important aspect of the strategy for answering the question lies in the challenge to the assumptions embedded in it. But this time, as well as interrogating the description of the findings and wording of the question, there is also a challenge to the truth of the empirical finding at issue. These challenges together culminate in the conclusion that what appeared to be a developmental question is not a developmental question.

B. Why Challenge the Developmental Assumption?

All those—"optimists" and "pessimists" alike—who have sought to make some useful contribution to conservation research, have shared the belief that some developmental issue is at stake. However, what was conspicuous in chapter 3 was that each fresh wave of research merely raised new contentious issues while failing to resolve the original question posed by Piaget's allegedly interesting, supposedly developmentally significant and apparently wholly robust findings.

Given then that a variety of developmentally-oriented approaches have led only to an escalating controversy, it is justifiable to suspect that there is something basically wrong with all this research. And the possibility to be now explored is that what is wrong is that what seemed to be a developmental issue is not a developmental issue at all. If the developmental assumption has all the time been unwarranted, the attempt to mount a challenge to it—however belatedly—must be worthwhile. On the other hand, should the developmental assumption be warranted after all, the challenge should be able to be seen off without undue difficulty.
It will be seen below that the adopted explanatory framework suggests that such a challenge can be mounted. Thus one good reason for challenging the developmental assumption might be simply that it is possible to challenge it. However, for the radical criticism to be constructive, it needs to be pointed out why challenging developmental assumptions, where this is possible, might be a desirable thing to do. Thus I would argue that, other things being equal, there are reasons for preferring a non-developmental explanation to a developmental one. There is a strong case for asserting that this is simply in accord with the dictates of scientific parsimony. If the behavioural results from a range of investigated subjects can be explained without recourse to dividing those subjects into two or more types or categories, we should refrain from such subdivision.

Thus, taking an hypothetical example, it might be hoped that the different toy preferences of 5 year old boys and girls of the same age would be explained either by (1) contingent aspects of their upbringing, or by (2) the predetermination of sex. It can be argued that it is, in the first place, up to those espousing the genetic explanation to establish that such an account is needed. They must therefore show that the pre-determined sex of the subject, on its own, is a better predictor of the the conduct in question than certain events and circumstances contingent on something else (e.g. a parental perception/attitude) which is not directly dependent on the sex of the subject.

It is not so much up to those espousing the "environmental" account to try to prove that - at least in relation to toy preference - everyone is to be expected to start off basically the same, and subject to the same sort of influences on their conduct. With weight given to the criterion of parsimony, the more general is the applicability of some empirically-supportable theory attempting to explain a given range of behaviour, the better. One based on a "null hypothesis" - no essential, relevant difference between the two labelled groups of subjects investigated - must always, other things being equal, have the edge on a theory requiring initially the identification of an essential difference between those subjects. Amongst other things, this recognizes that, while the labelling strategy employed may be useful as a classification of the subjects investigated, it may not contribute to an explanation of what they do - and the observed difference in what they do.
On the same lines I would argue that, so long as doubt exists about the warrantability of the developmental assumption in relation to the C.T. results, it is not so much up to me to prove that the developmental assumption is not warranted. Rather it is, in the first place, for "developmentalists" to establish beyond reasonable doubt that the developmental assumption is required. We can take this to involve showing that the way a subject behaves in a set situation, like "P", is to some degree directly dependent on his/her age. The least we could expect then would be that there is some essential, relevant (i.e. in relation to performance on the task) difference between the young child and the adult.

What the developmentalist might now say is that doubt about this does not exist - since children consistently perform one way and adults consistently perform in a different way on exactly the same task. But, having already suggested that the implied psychological significance of the difference between C.T. "success" and C.T. "failure" can be undermined, I will now show that, even though there still is some age-related difference to be accounted for, the most robust correlation is not in itself enough to banish doubts. The question is whether there is more than an interesting (?) correlation at issue.

C. The Difference Between an Age-RELATED Finding and an Age-DEPENDENT Finding

The shibboleth "correlation does not necessarily mean causation" is frequently encountered by students in the natural and social sciences. And it should be borne in mind here. When, in an arranged experimental context, like task "P", the representative young child is observed to respond differently from, say, the representative adult, we should ask if the child behaves differently because he/she is a child, as a developmental interpretation would have it. The alternative is that the child responds in that way for some other researchable reason. In such a case the fact that in the observed instances these subjects happen to be children is to be seen as incidental. And it is not difficult to demonstrate how a near-perfect correlation with respect to age might lead us astray if we do not remember this.
We might imagine a research project that sets out to investigate if and how the concept of "mother" changes with age. In a pilot study let us say the investigator decides to select and compare two extreme age groups - 10 year olds and 80 year olds - to see what trend might be suggested. He/she asks the members of each group to write a short essay entitled "My Mother". Above all, what is conspicuous is that the younger age group tend to write in the present tense while the older group tend to write in the past tense. Conceivably, the naive researcher might suppose that this is a directly age-dependent effect. Less naively, we would presume that this result is merely a consequence of the fact that the octogenarians' mothers, unlike those of 10 year olds, are highly likely to be dead. That the correlation with age was contingent on this fact, and therefore incidental in relation to the aim of the investigation, might be demonstrated in the following way. A sample of 80 year olds whose mothers are still alive would be selected and compared on the essay topic with 10 year olds who happened to have lost their mothers. It is predicted that the trend would be reversed with respect to the investigated aspect of performance. And it would be the children who now would be found to write retrospectively.

As well as illustrating how an age-related behavioural finding may be mistaken for an age-dependent one, this example gives an indication of the sort of strategy that is to be employed to challenge the developmental assumption in the present case of the C.T. results. Thus I will examine if it is possible to reverse the child-adult results, and what is involved in doing so (see below).

In the example it is obvious that an aspect independent of age and development is likely to be involved in the tense a person writes about his/her mother. Further, it is relatively easy to see what this factor might be and how to test for it. However, in the case of the age-correlated results obtained from a conservation experiment like "P" it is not so obvious. For it seems that, no matter how the subjects are selected, it is always the case that, in the same situation, a young child will be likely to respond one way and an adult will respond in a different way - i.e. with the "non-conserving" and "conserving" answers, respectively. What then can be wrong with the assumption that how the subject responds is in some way and to some degree directly dependent on his/her age, and that therefore some developmental issue (in the broadest sense) is at stake?
D. Re-emergence of the Descriptive Problem: Can We Talk of the SAME Task?

As before, the initial strategy — prior to attempting to advance an interpretation of the result of interest — is to try to make sure that we have got the descriptive level right. In other words (see chapter 2), just as the way Question A was posed was found to be misleading, so too may be the case for Question B. With respect to the former question, a rat was smelled in the reference to "success" and "failure", and in the implication that right and wrong answers were to be usefully viewed as "opposite". Thus, in tackling Question A, it was pointed out that researchers have tended to overlook a key area of similarity between tasks like "P" and "O". In the case of any bona fide C.T., it was noted, we are dealing with an ambiguous interrogative context. For this and other reasons (including the fact that nothing happens, the lack of information and involvement of deception), they are both to be judged on the same grounds, and found to be equally hopeless tests of reasoning ability.

In relation to Question B, what emerges is that — for the same reasons, researchers have tended to overlook a key difference. Again the descriptive framework adopted allows us to spot this and indicates how to act on it when it comes to trying to offer an answer to the "developmental" question". While "P" and "O" are able to be viewed as the same sort of ambiguous interrogative contexts, in tackling the results obtained from task "P" alone we might smell a rat in the reference to one group (young children) responding one way and another (adults) responding in a different way on the same task.

In relation to this suspicion, three points are to be made, the first suggesting that Question B may not be completely intelligible, the next that it might not anyway be worth trying to answer, and the last suggesting that — even if we grant that it is intelligible and worth trying to answer — there is a now-challengeable assumption in what lies behind the asking of it. It is the final point that directs me back to the correlation ≠ causation issue and to an empirical challenge to the developmental assumption.
(1) The analysis in chapter 6 converged on the point that, while "successful" and "unsuccessful" subjects have points of view - each equally justifiable - that overlap in one crucial aspect (both know that two identically-worded questions are asked in sequence), they do not hear and respond to the same question when E asks Q₂.

This suggests that the comparison implied by the wording of Question B may be a bogus one insofar as a subject who hears Q₂ as a pointless repetition of Q₁ (and answers "same") may not be able to be viewed to be participating in the same sort of episode as one who, equally justifiably, does not. But it does not in itself force us to give up the developmental question. The latter just changes its form, to: why, when E sets out to try to present the same task "P" to all subjects, do children consistently see what they have to respond to one way, and adults see things in a different way?

(2) But now we may start to suspect that the developmental question here may not be worth asking. We have already seen that, in terms of the information available to the subject, the only rationally correct - and deducible - answer to Q₂ is "I don't know", an option not made available to the subject. Thus, whatever the difference between children and adults in relation to a C.T. like "P", it does not appear likely to be a useful one for a theory relating to the progressive development of cognition-related abilities.

Even less promising, a case can be made that, insofar as an age-correlated difference is observed, in one important way it is the reverse of that which would be predicted by an hypothesis based on the developmental assumption. And it is now that the temporal aspect, which was the focus of chapter 5, once more assumes prominence.

One feature or property of an episode is its duration. In the present case the subject's understanding about how long the episode will last is, with the adopted general explanatory framework, to be seen as closely bound up with how the subject performs (see chapter 6, section G). While the perceived duration aspect was not sufficient on its own to explain performance, it is now to be seen as a contributory factor.

Comparing two subjects, the one who, other things being equal, is more likely to believe, at Q₂, that the episode is still at the preliminary phase is therefore, for that reason, the one more likely to perform with the right answer. Yet such a belief is obviously incorrect. With respect to duration at least, the subject who is
correct in his/her understanding of the sort of episode he/she is participating in is the one likely to get the wrong answer. The subject whose understanding of what he/she is taking part in is - however justifiable - manifestly at fault here is the one more likely to get the right answer. And in the case being considered, it is the child who can be claimed to be closer to the mark about the sort of episode he/she is in.

I have made a good deal of the fact that in a C.T. the perspective of the participating subject - whether he/she turns out to be a "conserver" or "non-conserver" - does not coincide with that of the experimenter who arranges the episode. If nothing else, it is curious that, on an aspect of the episode (its anticipated duration) that is likely to be important in influencing the response the adult developmental psychologist experimenter is interested in, the young subject's point of view should turn out to be closer to this privileged perspective than the adult subject's.

As is the case throughout this work, my aim is not to elevate the understanding of one group of subjects (children or adults) at the expense of the other. Rather it is to draw attention to the oddness of the C.T. and of what the investigator is trying to do in the service of seeking a developmental insight. Should we insist in arguing that there may be something inappropriate or "childish" (in a perjorative sense) about the child's response to the situation, then we may also have to suggest now - with this overlap in points of view brought to light - that there is something curiously inappropriate or "childish" about the investigator's conduct in organising such an episode, and in the E's conduct in carrying out the experiment. And it is not hard to spot the inappropriate component. In a context supposedly designed to test for a subject's understanding of what happens, the least we might expect is that something does happen in relation to what keeps being asked about.

Nevertheless, the point that concerns us and remains is whether Piaget's conservation task can tell us anything about children and some age-dependent psychological change. I have argued that it is hard to view it as capable of telling us anything about the progressive development of mental abilities. And I have now suggested that, in terms of the consistently observed child-adult difference, in one respect the difference is the reverse of that which would be expected.

(3) But, as it stands, we still have to admit that a consistent, age-correlated difference exists. And we may decide that there is still
a sufficiently interesting issue at stake here to persist in asking what it is about the young child that results in it performing differently from the adult here. That is, we are again drawn back to the "developmental question".

Having laid the ground and suggested that the grounds for comparing the child's and adult's performance on "P" may be dubious, that which I argue is the underlying flaw that has existed since C.Ts came into being can be tackled head-on. As I have pointed out, in asking the question as a developmental question, a correlation = causation assumption can be discerned. And I am now in a position to challenge the view that the child responds the way he/she does here because he/she is a child. The previous section equipped us with a strategy for challenging the developmental assumption in a specific case like this. That is, the empirical strategy now to be implemented is that of seeing if it is possible to reverse the results while keeping "P" intact as a bona fide C.T. The question is, therefore, how robust is the finding of adult "success" on "P".

E. Empirical Challenge to the Developmental Assumption

The adult gets the right answer on "P". In terms of the adopted general explanatory framework, he/she is now to be taken to do so through - for whatever reason - hearing Qz as a pointless repetition of the preliminary-phase question, Q1. This subject therefore is seen to answer Qz waiting for the episode to properly start - that is, get beyond the preliminary phase. The young child customarily gets the wrong answer, and he/she is now to be taken as "failing" through - for whatever reason, but equally justifiably - not hearing Qz in this way. Rather he/she supposes that there is a point to it and that the episode has got beyond the preliminary phase. For this subject it is likely that something relevant to the question will (justifiably) be viewed to have happened between the first and second asking.

There must be two components to an attempted reversal of these results. On the one hand we need to show that the representative young child may, in an intact bona fide version of the same sort of C.T., be induced to "conserve". He/she should be able to be seen to do so in the same way and for the same reason that the adult "succeeds" on the same
sort of task. On the other hand we need to show that the adult can "fail" on a task arranged to have the same properties, and be seen to do in the same way and for the same reason that the young child gets the wrong answer.

Task "0" can be seen as serving the required function of, as it were, "turning the child into an adult" as regards C.T. performance. But what remains conspicuously missing is a demonstration of "failure" by an adult on a bona fide version of the volume/amount C.T. In terms of hard evidence, what we require for the present "developmental challenge" is an example of adult subjects, faced with an intact version of the C.T., giving the wrong answer and manifestly not being left at its termination waiting for the episode to pass beyond its preliminary phase, and for something to happen.

8th Empirical Note: "turning the adult into a child"

The strategy for "turning the adult into a child" with respect to C.T. performance, involved presenting to adult subjects the very same classical C.T. that they would normally succeed on, but now embedded in the context of a game which, amongst other things, would be good fun. The game/experiment may be done with individual subjects, but I will describe here the procedure employed on a single occasion involving the simultaneous "testing" of 29 adult subjects.

These "subjects" were a mixture of undergraduates, post-graduates and staff attending a seminar in the Edinburgh University psychology department. I had let it be known beforehand that there were prizes to be won, and asked them at the start to try to forget about being psychologists and enjoy the good-fun (and possibly rewarding) game that was to open the seminar. Nevertheless, they did know that the topic of the talk was to be conservation tasks, and the majority would have been familiar with what a C.T. involved - though not with my research orientation.

The initial array of a conservation-of-amount task involving Carnation "Coffee-Mate" powder - was presented embedded in a series of five 2-beaker comparisons, all beakers containing the same substance. Shown in the diagram below are the five comparison pairs on display, plus the container (K) used for the transformation phase.
The lighter shading denotes normal powder (i.e. as poured from packet to the container). The heavier shading indicates densely packed powder. Before the arrival of the subjects I had arranged that the powder in containers B, D and F was compacted by repeatedly tapping of the base of these beakers on the bench. The difference between "normal" and "dense" powder was not detectable by cursory inspection. The actual, pre-arranged relative amounts for each comparison is indicated above each pair, the embedded "C.T. pair" being the fourth in the row.

I told the subjects, when they entered the room, that I was going to ask them ten questions and that they were to write down the answers in order. To emphasize the game aspect I offered prizes: 50 pence for the one who got the most right answers and £1 for anyone who got all the questions right. I informed them that the bottle of whisky (also on display) was a "mystery prize" that might or might not be won, without saying more. I also asked the participants not to alter their answers once written.

The first five questions concerned what was initially on display. For each of the comparison pairs in turn I asked the standardised question that was to be used throughout: "Is there more in the left-hand beaker, less in the left-hand beaker, or the same in each beaker?" Having elicited the pre-transformation answers, I then proceeded to the sequence of transformation phases.
Returning to the first pair, I transferred the contents of the right hand beaker (B) into the large beaker, K, asking the standard question again. Having elicited written answers from everybody, I then said "Let's see" and poured the contents of K back into B which was now unable to contain all the uncompacted powder, causing a mess and appreciable surprise among the subjects. Moving to the next pair, I repeated the procedure, the final levels in C and D in this case being now the same. When this had been done for all five pairs and a total of ten questions had been asked, the written answers were examined.

Moving to the results: as predicted, the total financial outlay was restricted to 50p. No one, until afterwards, acknowledged that there was not enough information to answer any of the questions. Therefore no one won the bottle of whisky which was to go to anyone who wrote down "I don't know" to all ten questions. But my main interest was in the answers to the embedded conservation task, involving comparison-pair (4). Of the 21 subjects who answered "same" to Q₁ (as is normal, and was pointed out in Chapter 5, there was a degree of reluctance to "establish equivalence") no fewer than 13 (62%) gave some sort of "non-conserving" response to Q₂.

The main likely objection would be to the contrived "tricky" nature of the manipulation. Thus it might be argued that such a manipulation unduly alters the nature of task "P", which (1) does not normally allow for such interesting possibilities of something relevant happening, and which (2) does not occur embedded in a deceptive context. Such an objection, at least as stated in this form, does not stand up well to scrutiny. We only have to remember and note the difference between the privileged point of view of the investigator and that of any subject on any C.T. to acknowledge:— (1) a task like "P" does always contain interesting possibilities of something—rather than nothing—relevant happening, as any amateur conjuror could demonstrate, whatever materials are in use; and (2) a C.T. is always presented to a subject embedded in some context, and, as I have pointed out, there is always a deceptive component to this context. Further, the child on the so-called "straight" version may well suppose that he/she is in an episode where there are interesting possibilities, and where there may be an intention to deceive on the part of the adult (E). After all, the "straight" version, explicitly occurs in the context of a game which might be a "tricky" game involving guesswork in the absence of sufficient supplied information.
The primary aim of this empirical work was simply to demonstrate that it is not unduly difficult to get normal, presumably intelligent adults to "fail" on a bona fide, intact version of the classical C.T. A recent study by Winer, Hemphill and Craig (1988), involving adults doing — and failing on — weight C.Ts. makes a similar point. In my study, the adults "tested" in the manner indicated more often than not gave the wrong answer, and the way that they "failed" fitted the general explanatory framework put forward in the previous chapter. Thus they answered Q₂ not supposing that it was a pointless, or memory-jogging, preliminary-phase repetition of Q₁. And they were compelled to acknowledge that the conservation task held out interesting possibilities. Whatever the point was, they were satisfied, unlike before, that there was a point in asking Q₂. As there is a point, in this respect at least the discrepancy between the points of view of S and E is narrowed. But, in spite of having made good what was seen as a gap in the evidence, we are still left with what seems to be a dilemma. And this takes the discussion back to the points arising in the previous section.

On the one hand I might insist that it is meaningful to talk of the adult "failing" here on the same task that he/she would normally have succeeded on. In this case, with such variability of results from the same age group on that "same task", it appears too temperamental a context to be useful in the resolving of any significant developmental issue.

On the other hand it can be argued that it is not meaningful to talk here of the "same task". Granted, the embedded C.T. has precisely the same "objective" properties that the "unembedded" or "straight" version has. The same array is used, the same manipulation of it occurs. In each nothing relevant happens except that the same two identically-worded questions are asked in sequence. However, observation and intuition categorically tell us that the situations encountered in each case are quite different. To confirm this we need look no further than the fact that the embedded version, unlike the other, is found interesting and enjoyable.

One response here might be to try to preserve the developmental significance of the findings of the "straight" version, arguing that the results of the above "non-straight" version are unduly contrived, irrelevant to the debate and therefore do not constitute a real challenge. For reasons already indicated, picking out the latter
version alone for criticism will not do. And now it turns out that every point made as to how the "non-straight" version crucially differs from the "straight" version merely reinforces the view that it is the former that more closely resembles the version that the child participates in - and also "fails" on.

If, when the attempt is made to "even things up" while keeping the form of the C.T. intact, the difference between child and adult tends to evaporate, then we have a right to be sceptical about the capacity of the C.T. to do the developmental work assigned to it.

In relation to this point, task "O" can be seen to work to "even things up" in the other direction. Keeping the basic form of the situation intact as a C.T. the child is, in effect, "turned into an adult" insofar as he/she now succeeds on the modified version and may be seen to do so in the same way that the adult succeeds on the standard version ("P"). Again the "objective" properties are the same. But this time the evidence suggests that both (justifiably) hear Q\textsubscript{2} as a mere repetition of the preliminary-phase question, Q\textsubscript{1}, and are left at the end waiting for the anticipated, but non-existent, "point".

**Summing Up:**

To make a developmental point from a specified context there must be seen to be a common ground for comparison. If it is not possible to talk convincingly of presenting the same situation to the children and adults involved, then the comparison implied by the question: "Why do children fail on "P" while adults succeed on the same task?" seems like a bogus one. Conversely, if it is possible to present the same situation to both groups and still talk of it as a bona fide C.T., then we might expect that the attempt would be made to do so. Yet, when the attempt is made to do this, what stands out is that the original difference between the younger and older subjects - with respect to both performance and overall response to the situation - tends to disappear.

Either way the suggestion is that it may be more profitable to turn away from seeking some sort of developmental account of the originally observed difference, looking instead for the "other researchable reason" - even if such a non-developmental account turns out to be a rather trivial one in psychological terms.
F. Towards a Non-developmental Answer to Question B

(1) An hypothesis

The whole of the foregoing analysis gives us the hypothesis to be examined. This is that, while the E may attempt to present the same task to both groups of subjects, (1) E manifestly does not succeed, and (2) the aspect(s) of the arranged episode that E fails to keep constant across the compared groups are sufficient to account for the finding in question - i.e. the age-correlated difference in performance on "P".

The next step then is to compare what E does in carrying out the classical C.T., "P", with 3 - 5 year olds with what E does when he/she tries to do the same experiment with adults. The aim is to decide if, other things being equal, some emergent discrepancy in E's conduct could account for the observed difference in performance on what, in other respects, is the same task and in each case still an intact bona fide C.T. Whatever else E does, and whatever discrepancy is introduced, he/she must still be seen to arrange that in both cases nothing relevant happens except that two identically-worded questions are asked in sequence and that the first (Q₁) is answered with the response "same" before the episode is allowed to continue.

(2) "P"-for-the-child versus "P"-for-the-adult*: the significance of observed differences in what the experimenter arranges

(a) What does E do prior to the C.T., that introduces a discrepancy between what the compared subjects expect?

The child is invited to come and play a game while the adult is invited to participate in a psychology experiment. The 3 - 5 year old, in the case of my own work, was taken from the psychology department nursery where he/she was having fun doing interesting things and playing enjoyable and interesting games. If the child was not convinced he/she was leaving this context to come and play another interesting, good-fun game he/she would have been less willing to come than he/she was generally observed to be. On the other hand, the adult is taken from a context involving the serious business of earning a living, revising for
exams, etc., having been asked to make an entry in a busy appointments diary to come and help me with something connected to my serious business of producing a PhD thesis in psychology. Like the child, however, the adult was to be taken for testing, with the same tape-recorder running, frequently in the same room of the psychology department, with the same materials.

While both subjects are given, through E's conduct, to anticipate an episode where something interesting will happen, the discrepancy in what E does is evident. The child is told it is a game in order to get him/her to willingly co-operate. The adult is told it is a psychology experiment because this is the truth. To even things up we might hope that it would be possible to tell each subject exactly the same. Preferably we would tell each the truth. But there is a problem here. We can presume that the significance of informing the child that he/she is about to participate in a psychology experiment (or in a problem-solving situation) will be lost on this subject. This certainly raises some sort of developmental issue (the child lacking a concept possessed by the adult), but it would be strange indeed if the developmental significance of the C.T. hinged on this point.

The obvious alternative for removing the discrepancy would seem to be to tell the adult that he/she is going to take part in an interesting, good-fun game. But, apart from any reluctance E might have about lying to a subject, there is again the risk of introducing a diversionary developmental issue in the search for an answer to Question B. E will face the problem of later having to match his actions with his/her words. That is, having told the subject to expect a good-fun, interesting game, E needs to arrange something that looks like good-fun, interesting game - for adults. We can assume that the phrase "interesting game" has different meanings for the child and adult. If the adult (as tends to be the case) still performs differently, it could be - somehow - through a difference with respect to what is associated with this concept. Again we can presume that, whatever was intended for the C.T. by way of the developmental significance of the results, it did not include the aim of demonstrating that a "game" for the child is not always the same as a "game" for the adult.
(b) The discrepancy in what the compared subjects get

Both the child and the adult get an episode involving two identically-worded questions asked in sequence, where something relevant to the answering of the question(s) might or might not have happened, and which is terminated by E when the second question \(Q_2\) has been answered.

Observation indicates:- (1) that the child gets what he/she was told to expect: an interesting and enjoyable game; (2) that the adult does not get what he/she was told to expect: a psychology experiment with a serious and interesting aim. What the former subject gets comes up to the requirements of a child's game. What the latter gets does not come up to the requirements of the stuff that psychological insights and theories could be based on. It might be more accurate to say that what the adult gets does not yet come up to the requirements of a serious psychology experiment. For, at the point when \(Q_2\) is answered - actually marking the end of the episode - the subject presumes that the episode is still at a scene-setting, preliminary phase, with the point of the "experiment" still to come. In the case of the adult, unlike that of the child here, the experimenter interrupts the episode at or near the start - just when it might have been about to get interesting.

(c) Explaining the difference in performance on "P" in terms of the identified discrepancy in E's conduct

What I have established is (1) that the young child is told to expect a game and given a situation which could be a child's game; and (2) that the adult is told to expect a psychology experiment and - up to the point where \(Q_2\) is asked - given a situation which is highly unlikely to be a serious experiment. It should now be clear how the difference in performance is to be explained by this discrepancy.

1. **The child's wrong answer to \(Q_2\).** E tells the child to expect a game where something interesting will happen and then presents an episode where something interesting could have happened. But E arranges the episode to be one where nothing relevant does happen. Certainly we might go into the possible reasons why the subject here would suppose at \(Q_2\) that something relevant has happened - i.e. beyond the facts that (1)
he/she was, in effect, told that something would happen before the end of the episode, and (2) there is no more to come. And we might try to investigate what sort of thing the subject supposed has happened in relation to the array. But in fact we need go no further than acknowledge that the non-conserving response is a justifiable one. For the fact that there are always several potential explanations of the wrong answer - and of which particular wrong answer is elicited - is why it is, as always, more profitable to make the "successful" response (that of the adult in this case) the focus of interest. We need only refer to one reason here, and one that has nothing to do with specifically what happens to the array, what the subject believes happened to the array, or what the subject expected would happen in relation to the array.

2. The adults right answer to Q2. Q1 was a preliminary-phase question to which the approved answer was "same". Q2 is worded identically to Q1, and the subject has good reason to suppose, at Q2, that what he/she was told to expect is about to happen. This subject can therefore be seen to be likely to (incorrectly) suppose that the episode is still in the preliminary phase, and to take Q2 as no more than a repetition of Q1 - calling forth a repetition of the initial, approved answer.

What the experimenter does with one group of subjects (adults) is not what he/she does with the other group (children). And the difference in E's conduct here could be sufficient to account for the observed difference in performance.

Where the child "goes wrong" is to overlook the possibility that the point of the "game" was that nothing relevant was intended to actually happen or change. But the adult here fares even worse. As well as overlooking the possibility that the point of the "psychology experiment" (or whatever he/she was told or led to expect) was that nothing relevant was intended to happen, this subject overlooks the possibility that his/her answer to Q2 might mark the termination of the episode - as it does.

The crucial "non-developmental" point is that, with respect to the finding in question referred to by Question B, this happens to be the case, but could have all happened to be the other way round. While retaining the form of a bona fide C.T. - same array, same transformation of the array and same questions asked - it is possible, as I have shown,
to reverse the results merely by appropriately tampering with those "extraneous" features predicted on a non-developmental hypothesis, but not on a developmental one, to be critical.

G. Conclusion

(a) Question B answered on the same lines as Question A

What I am not saying is that my interpretation of the finding in question is right. The above analysis does not rule out the possibility that there is some sort of developmental component to how the subject performs - or even that a reasoning failure on the part of the child is involved somewhere along the line. What I am saying is that, given (1) the empirical evidence, and (2) the properties of the arranged episode and, in particular, given its nature as an impoverished and ambiguous interrogative context where - apart from the questions - nothing relevant actually happens, it could be right insofar as a developmental account is not required.

With the design of the task making it unsuited to resolving any significant developmental issue, a non-developmental account was able to be sought and supported. And the argument was that, where there is doubt about developmental significance, such an explanation is to be preferred - if only in the service of scientific parsimony. A further point in favour is that the resultant answer able to be offered to Question B is on the same lines as the simple answer already offered to Question A, and is derived from the same general explanatory framework.

Answering Question A involved viewing the C.T. solely as an interrogative context, where two identically-worded questions are asked in sequence. The fact that the subject concerned (the representative young child) gets the right answer on "O", but not on "P", can be sufficiently accounted for by noting that E does something extra in "O" that is likely to communicate to the subject that Q₂ is a mere repetition of the preliminary-phase question, Q₁. E says "where were we?" / "what were we saying?" just prior to Q₂. The "developmental question" (Question B) is answered in a similar fashion. The fact that the subject concerned (the representative adult) gets the right answer on "P", when the wrong answer would have been equally justifiable, and indeed is elicited on other occasions when "P" is presented, can be sufficiently accounted for by noting that again E does something that
will be liable to communicate to the subject that $Q_2$ is a mere repetition of the preliminary-phase question.

The point consistently made in the last two chapters is as follows. To explain the variety of results in these and all bona fide conservation experiments, we need only look at factors that might influence whether $Q_2$ is less likely or more likely to be heard by any subject as a pointless or memory-jogging repetition of $Q_1$. And it turns out that there are always features independent of and extraneous to the array and E's manipulation of it to be taken into account in this respect. Here I have attended to what E says just prior to and during the episode. On other occasions we might want to attend to the way E says and does what he/she says and does to explain some finding of "success" or "failure". Or we might want to investigate what happened or was going on for the subject some time before that might have had a relevant influence one way or the other.

(b) The C.T. as a task designed for children

A major underlying problem seems to be - as anyone who has tried to do C.T.s with adults knows - that these tasks were designed for children and seem peculiarly unsuitable for adults (see chapter 5 (D) ). For me this is significant evidence in itself and a good reason for being suspicious about what can be learnt from the results of C.T.s. But it might be argued that I have been led astray by choosing such an extreme age range for the comparison groups. However I would argue that a comparison of, say, 3 - 5 years and 8 year olds yields to the same sort of analysis. For example, the representative 8 year old gets the right answer even though he/she is quite likely to be told, and may believe, he/she is in a "game". But, as for the adult, we may be able to make no more of a developmental point here than, say, that what a 4 year old expects from a game is not what an 8 year old expects. Consequently, like the adult, the 8 year old is observed to be, incorrectly, still waiting for the episode to get beyond the preliminary phase, to the "point", when E terminates the episode.

(c) "U-shaped developmental curves"

Still on the topic of age-correlated performance in children, I mentioned at the beginning of chapter 3 the phenomenon of the "U-shaped developmental curve" in relation to C.T. performance. Frequently 3 - 4
year old children are found to conserve better than 5 year olds. This sort of finding posed a problem for those concerned with the developmental significance of C.T. results and often features in the false negative - false positive debate. Thus precocious success has frequently been put down to an attentional failure and hence dismissed as a "false positive".

Not only does such a result present no problem for a non-developmental explanatory framework, but it is the sort of supposedly incongruous finding that explicitly lends support to the particular non-developmental account advanced here. Like the adult, the 3 - 4 year old cannot be blamed for not attending to the interesting possibilities present - given that it is arranged by E that nothing relevant actually happens. And, like the adult - or the 8 year old - the very young child's success can be put down to the fact that in the circumstances he/she may happen to expect something other than that which he/she gets. Like the 5 year old, he/she is told to expect a game, but once more there is no reason to suppose that this means the same thing to the younger child. And again it can be presumed that this was not the sort of developmental issue that the C.T. was supposed to shed light on.

As always, the relevant evidence on my account would be anything that suggests that the conserving subject answers $Q_2$ as a mere repetition of the preliminary-phase $Q_1$, disregards the significance of the actually irrelevant intervening events and is left at the termination of the episode surprised, having expected more. Such evidence is readily forthcoming and, indeed, is perhaps already anticipated by the "attentional failure" account.

Other supposedly interesting C.T. performance findings should yield to the same sort of interpretation and be able to be accompanied by the required accompanying evidence - e.g. as to the manner of responding and reaction to the E's termination of the episode when $Q_2$ has been answered. Thus a finding such as Wishart's (personal communication) - that Down's syndrome children frequently perform "better" on C.Ts than healthy children of the same age - may be able to tell us little about the intellectual achievements of the former groups as it does about the intellectual lacunae of the latter.

Whatever the reason for one group - identified by age, health, sex, colour, or whatever - getting the wrong answer on a bona fide C.T. (e.g. "P"), while the comparison group gets the right answer, it cannot - on any rational standard, given the background of deception and lack of
information - be said that the "non-conserving" response is any less justifiable, or that the right answer necessarily shows that the subject concerned can reason about volumes and amounts, whatever precautions are taken and controls applied.

(d) The conclusions generalized to the findings of all C.Ts

A final point is that I would claim that the applicability of this sort of analysis is not restricted to volume/amount C.Ts. Taking the number C.T., for example, and overlooking for the moment the general reservations we might have about this type of C.T. (see post-script, chapter 3), it might seem even more baffling why the young child tends to get the wrong answer here. Why should the mere bunching up of a row of counters make it seem as if there are less counters? The transformation of the array does not appear to result in the same hiding of information as in, say, the volume C.T.

But we know by now that this is the wrong question to start with, there still being various possible reasons for a subject saying "not-same". The initial focus must be on the "successful" performance on this task. Even so we do not ask: why does the subject who gets the right answer know that the amounts being compared are the same before and after the transformation. Again we disregard the array and the E's manipulation of it, viewing this bona fide version of the C.T. solely as an interrogative context where all that happens is that two identically-worded questions are asked in sequence, everything else - including the traffic noise outside and the transformation - being equally irrelevant. The question to be asked is: why is it that the subject (child or adult) hears Qa as a pointless or memory-jogging repetition of the preliminary phase question Qi, answering correctly while incorrectly supposing that the episode is just starting and that the point lies ahead?

When put like this, our attention is again drawn to the background of deception and lack of information that I have shown operates in any C.T., for any subject and however he/she performs. If deception and lack of information constitute an inherent feature, the subject cannot be faulted for giving any answer, however unlikely, as this is a rational way to respond to an episode suspected (totally justifiably and correctly) to involve deception and information that the subject cannot be privy to.6

Further, we again are reminded - perhaps even more so than in the case of "P" or "O" - that the C.T. is designed to be a totally
impoverished situation and that, correspondingly, the subject can get it right in an emphatically "minimal" manner - i.e. by gross negligence in relation to everything that is arranged to occur except the two identically-worded questions. If getting the right answer need again make no more demand on the intellect than (a) recognizing that \( Q_2 \) is the same question that was initially asked \( (Q_1) \), and (b) remembering what the approved answer to \( Q_1 \) was, then this must once more have a bearing on the way we evaluate the "non-conserving" response. And it must again make us think twice about labelling it as a "failure".

*Unless* the subject counts, that subject (as in the volume/amount C.Ts considered) is justified in supposing that something relevant to the answering of the question has happened to the array. Yet if he/she *does* base his/her answer on counting, whether the subject counts correctly or incorrectly, the task may be testing for some ability, but is certainly not testing for the ability - conserving ability - it was supposed to be testing for. This, therefore, may or may not be present.

If it was legitimate in the first place to undertake a psychological investigation of conservation as a primary intellectual accomplishment (see Chapter 9), the conservation task was not the tool with which to investigate it. The unhappy alternative is that we are restricted to saying, as for I.Q., that "conservation" is what conservation tasks test for.

In any conservation task it is arranged that nothing relevant actually happens, apart from questions requiring answers being asked. We should not now be surprised that the results obtained from a version of such an impoverished "test of understanding" yield to the correspondingly impoverished and psychologically trivial level of explanation that has emerged in the attempt to offer answers to Questions A and B.

I conclude that not only do the findings obtained from the different versions of the "ingenious" C.T. no longer appear particularly interesting, but also that the C.T. cannot be made to do any of the developmental work that has been, or might be assigned to it.
'Given that Qi is assumed to be viewed as a preliminary-phase question.

2Starting from scratch with a "null hypothesis", there is at least the advantage of being able to begin with the psychological realm. In the first place the investigator can look to those potentially observable contingent events and circumstances - things that happened to the subjects - that might intuitively be expected to exert an influence on the conduct in question. From there on things may become increasingly complex with the spelling out of the psychologically relevant processes involved. However, an explanation in terms of some essential and, initially, non-psychological, difference between subjects (e.g. sex or colour or age), entails entering an altogether higher order of complexity to make the link to the predicted behaviour. The alternative is to merely explain away the difference to be accounted for, without properly addressing why having an X instead of a Y chromosome (or having a 4- instead of a 40-year old brain) should make a difference to the conduct in question in the way the correlated observations suggest.

3There is of course nothing unusual about this state of affairs in experimental psychology. The present case is, however, a particularly good illustration of the interpretative difficulties to which this can lead.

4It should be borne in mind that C.Ts are not normally performed with adults. While there is therefore a hypothetical element to this analysis, I start from my own work with adult subjects.

5If the child bothers about the tape-recorder at all, it seems that it merely adds to the fun and expectation of an interesting game. The adult is commonly disconcerted by it and even more convinced that he/she is participating in a serious experiment.

6Given that "I don't know" is not one of the available alternatives.
PART III - DISCUSSION: ERRORS AND IMPLICATIONS
Chapter 8

Identifying the Source of the Mistakes

A. Introduction

Certain pitfalls characterize uncritical applications of the constructivist approach in cognitive psychology. Making reference to the present controversy, general difficulties can be highlighted and potential sources of distortion identified. At the same time the more specific aim of deepening the insight into the particular muddle surrounding existing cognitive developmental interpretations of the C.T. findings is achieved.

Piaget's view was that getting the right answer on a classical C.T. like "P" depends on the possession of a certain reasoning ability. The young child goes wrong on the task and must do so because that ability is not yet present. His optimist critics reacted by saying that such a task involves reasoning to get it right, but also involves other things. The young child goes wrong, but the manifest error is compatible with the presence of reasoning ability and consequently some other developmentally significant reason for failure can be offered - e.g. one related to the development of language and communication.

The debate was able to blossom because the sort of modified versions (e.g. task "O") the latter camp employed to back up their "false negative" criticism in turn prompted the "false positive" criticism. The two sides here were substantially correct in their criticisms of each other's versions of the task as flawed tests of the presence/absence of a reasoning ability. But the above empirically-supported argument suggests that this is only because getting the right answer on any C.T. neither depends on nor need involve reasoning
ability. Given the essential specifications that any version must meet to be a bona fide C.T., it is always possible to put forward a more economical explanation of "conserving" (and, derivatively "non-conserving") responses - one not involving the exercising of any particular reasoning competence.

One consequence of this is that it is no longer legitimate to assume that the non-conserver goes wrong at all, given the available information. It is for this reason that the optimists are right to claim that the wrong answer on "P" is compatible with the presence of the reasoning ability. The other consequence is that it is no longer ever legitimate to assume that, in "conserving", a subject is manifesting an intellectual accomplishment superior to that of the "non-conserver". And it is for this reason that the critics of the optimists' project are correct in claiming that the right answer on a version like "O" is compatible with the absence of reasoning ability. As was suggested in chapter 4 and is now confirmed, on both "P" and "O" both right and wrong answers are compatible with both the presence and absence of reasoning ability. Hence, not only are both experiments bad tests of reasoning ability, but insofar as they share the essential features of any C.T., it is difficult to imagine worse ones.

The view, apparently held by all concerned, was that, when the false negative - false positive issue was resolved, the developmental significance of Piaget's original findings would emerge. My more general conclusion, however, suggests that there is something challengeable in the assumption that knowing why the representative young child gives the answer he/she does on a task like "P" could help resolve any developmental issue. In effect I challenged whether Piaget's supposedly interesting finding - to which Question B referred - posed a developmental question at all.

If all sides can be seen to have been misled about the very questions they sought answers to, the most powerful criticism will be that which (1) can be applied to all sides in the dispute, and (2) helps illuminate how and where they initially went wrong, rather than merely illustrates - as I have tried to do in the last four chapters - that they went wrong. Amongst other things, it will be seen that the false negative - false positive controversy can finally be laid to rest through such an excavation of the shared source of error.
B. How The Conservation Enterprise Went Wrong

(a) The two cognitive prejudices reconsidered

The whole enterprise may have gone wrong in the first place through assuming that the notion of "conservation" could be a useful one for a fundamental investigation of the psychology of thinking (see chapter 9). Nevertheless the test of conservation came into existence in its various forms. And my project here is limited to trying to identify the source(s) of error that led investigators astray in tackling the interpretative problem that the mixed bag of findings posed.

I have argued throughout that the key to sorting out the interpretative puzzle is to identify the basic problem as one of description, not interpretation. The question that concerns me here therefore is: given the sort of assumptions the disputants shared in their approaches, what were the predictable sources of distortion in the way C.T.s, and the conduct that results from them, were viewed and described?

Merleau-Ponty's critique of the rationalists and the intellectualist epistemology which underpins the constructivists approach of contemporary psychology provided a direct answer to this question (see chapter 2 and Appendix II). We can anticipate the consequences of the "prejudice in favour of a certain view of the world" allied with the "prejudice in favour of mediated experience".

To briefly recap on what was said at the end of chapter 2: in terms of an experimental context designed by the psychologist as a test of understanding, there is always the danger of an unwarranted intrusion of the investigator's point of view of (a) what he/she has arranged, and of (b) the meaning-giving processes involved in responding appropriately to it. Being a cognitive psychologist, the episode is arranged as, and viewed to present, a problem. The subject is consequently viewed as a problem-solver, problem-solving involving the mental operations of a mediating intellect. The above prejudices to be on the look out for therefore translate into (1) a prejudice in favour of the investigator's privileged perspective of the problem he/she has arranged, and (2) a prejudice in favour of the investigator's assumptions about the cognitive accomplishment required to resolve the problem and respond appropriately.
In short, the intended problem may not be the encountered problem, and the investigator will have been led into error if his/her view of the potentially significant features of the arranged context does not allow for this. Likewise, the reasoning intended to be involved - and perhaps reflecting the reasoning lying behind the design of the problem - may not be the reasoning actually required in responding in the approved manner. Again descriptive distortions are likely to arise, this time in relation to the investigator's view of the behavioural findings. Indeed the extent of the mismatch may be such that not only is the intended problem not the encountered problem, but the nature of the arranged episode may be such that the subject is able to respond appropriately without encountering a problem at all. This not only illustrates the close relationship between the two prejudices, but is pertinent to the discussion about the C.T. that follows.

Correcting for these linked prejudices can be seen to involve adjusting the balance in favour of the subject. Thus due attention should be paid to the subject's point of view of (a) the arranged context, and thus (b) what he/she is and is not required to do in "responding appropriately". But it is crucial here to stress that the conservation enterprise, and cognitive psychology in general, is not being charged with ignoring the subject's point of view. This is the prerogative of behaviouristic approaches, and cognitive psychology can be seen as a reaction against this sort of subjectless psychology. Rather than an essentially passive mechanism automatically supplying a given output for a given input from the environment, the subject was now to be recognized as an active meaning-giver; and the environment was to be seen as that which has to be represented in some sort of order before it can be effectively responded to.

However the view of input and output and what went on in between now had new restrictions placed upon it. The world is seen only as an already-there problem, conduct is viewed only in terms of more adequate or less adequate solutions and meaning-giving is equated with mediating problem-solving processes. Such a narrow view overlooks that part of the meaning-giving process whereby problems can arise in the first place, and needs the above biases to uphold it. And we are thus back to the inevitable distortion that comes with purely cognitive interpretations of observed conduct.

But the question here is: if the cognitively-oriented investigator can claim to be specifically concerned with the subject's point of view
in order to understand what he/she does and why, and yet is liable to be unduly influenced by his/her own point of view of (1) what (i.e. the problem) he/she has arranged, and (2) what is involved in responding appropriately, what is the best way of characterizing the sort of methodological error that will be the likely outcome of this conflict?

(b) The experience error

What we can expect is not total neglect of the subject's point of view, but an inadequate treatment of it. Confusions are likely to arise over assertions that can only refer to the investigator's own point of view of the episode, and those that can legitimately refer to the subject's point of view. What I label the "experience error" is to be attributed to a psychological investigation when an unwarranted oscillation between the privileged perspective of the investigator and the point of view of the subject is able to be identified.

The relationship between this error and what Giorgi (1981) calls the "psychologists' fallacy" is a close one. This author refers to it as a confusion of standpoints and quotes Warren's (1934) definition:

An error of method and interpretation which consists in attributing to a mental process all the characteristics which seem to be logically necessary from his knowledge of the process, the psychologist thus confusing his own knowledge about the process with the subject's direct experience during the process.

Giorgi goes on to refer to William James's (1950, p. 196-7) identification of two versions of the fallacy:

[Firstly] the confusion of the [psychologist's] own standpoint with that of the mental fact about which he is making his report; ... [and secondly] the assumption that the mental state studied must be conscious of itself as the psychologist is conscious of it.

I suggest, then, that it is such a confusion of standpoints, and - given the cognitive framework - an intellectualist version of it, that stands out as contributing to the muddle that envelops the conservation enterprise.

Returning, therefore, specifically to the case of the C.T., the assumptions that Piaget, his "false negative" (optimist) critics and
their "false positive" critics can be seen to hold in common are the following:

1. A suitably controlled C.T. (the disputants differed over this, i.e. over the precautions to be taken) poses a certain circumscribed problem concerning the array and its manipulation.

2. The significant features of a suitably controlled C.T. are those that concern the array and its manipulation.

3. In any C.T., the only truly appropriate response is the "conserving" answer, the wrong answer reflecting some sort of failure.

4. In a suitably controlled C.T., giving the right answer requires some sort of reasoning ability, while the wrong answer would suggest a reasoning deficit.

I have said enough to suggest that all these assumptions are mistaken. The specific claim here is that these mistakes are to be seen as a result of the confusion of perspectives I have called the experience error. The consequences were anticipated (see chapter 2) to be:

(a) identifiable errors of commission and omission in the description of significant aspects of the C.T., and
(b) a general overestimation of the mediating cognitive operations called for in "conserving".

In the remainder of this chapter I will therefore attempt to trace out the occurrence of the error as committed and compounded by these investigators. In this way the frequently subtle, and hence potentially pervasive, movement back and forth between perspectives will be directly linked to the eminently unproductive way in which the controversy has evolved. While the focus is on conservation research, the prescriptive implications are not confined to it.

C. Where the Conservation Enterprise Went Wrong: the Experience Error and Piaget

For Piaget the world as we encounter it presents an appearance that is not to be trusted. Rather than viewing our pre-reflective experience of the world as providing the occasion for reflection and that which thought must always refer back to (see Appendix II), it was seen as that which must be overcome and then be left behind by the workings of the
mediating intellect. It is therefore not just that thought is a "more trustworthy guide to action than perception" (Flavell, 1963, p.415), but the very goal of thought - which for Piaget means scientific thought - is to "always get further away from this lived world, contradicting it instead of utilizing it" (Piaget, 1972 b, p.87).

To test for the presence/absence of a concrete operational rational ability it was therefore a natural step for Piaget to employ an array that was designed to give a misleading appearance and that would therefore demand a mediating rational operation if the tested subject was to "see through" it and respond appropriately. Thus tasks like the classical C.T. came into existence to demonstrate that young children, unlike older subjects, are "seduced" by appearances, and to demonstrate a reasoning deficit in them.

In a task like "P" the final array, must have been intended to give an appearance suggesting that there is not the same in each container - that is, to anyone on initially being presented with it. If this was not so, he would not have been able to assume that there was a misleading impression to be overcome. With reasoning then not necessarily called for, the task could not be taken to be working in the way required. The implication, therefore, is that all subjects start off, at some level, being deceived that there is not the same in the containers. The difference between younger and older subjects is that, while the former remain fooled, the latter, bringing some logical ability into play, work out that there is the same in each in spite of the appearance of the final array giving the contrary impression.

The first question that occurs to us is therefore: what sort of reasoning is needed to overcome the alleged deception. Still only referring to the final array and setting aside for the moment the problem that, if it initially looks like there is not the same why the subject should go on to check whether he/she has been fooled, Piaget might here invoke "compensating operations". The rational subject may proceed to co-ordinate the facts of the greater height of the level in container A and the greater width of C, concluding that the amounts are equivalent. But in this case one might be justified in wondering why Piaget did not just present the final array plus Q2 and miss out the "establishing equivalence" and "transformation" phases.
INTERLUDE: a better test of conserving ability?

One obvious answer would be that this is not now a conservation task. The conservation question, Q2, is supposed to tap whether the subject understands that the amount initially in container B and finally in C remains the same. And Container B is not involved in the final array. But now one would be equally justified in wondering what is the purpose of the final array involving a comparison between A and C. Why bother bringing the contents of container A into it? It would seem to be irrelevant to this question, just as it is irrelevant how much is in either B or C. Whether there be half a pint, 200 ccs or "the same as in A", all that ostensibly matters is whether the subject understands that, however much is in B, it is not changed in amount by being poured into C and having its appearance thus transformed.

9th Empirical Note

Put like that it might seem that there was a more direct way of testing for conserving ability - that is, without bringing in A. The experimenter would simply present B and ask the subject what he/she sees in front of him/her. When an answer akin to "a glass containing some / a certain amount of water/powder" is elicited, the contents of B would then be poured into C. The subject would now be asked another open question: "what do you see now?" and/or "what happened?"

I did this with subjects of various ages (including adults and very young children). Obtaining the required response to the first open question presents no problem. Likewise in response to the second open question, I found that virtually everyone over the age of three says something to the effect that they "see the water [or powder] now in another glass"; and that what happened was that "you poured the water [or powder] from one glass into the other". Subjects consistently referred to the water [powder], not the the water plus or minus any water. There was never any indication that it was believed that some
water was lost or gained, or even that some water might have been lost or gained.

This is certainly not to say that (a) the design of this task and (b) interpreting the obtained results, need not present problems. For instance, just as in the C.T. proper, there might not be enough information for S to know that all I did was just pour the contents of B into C - see below. And of course there is no guarantee that, say, 3 year olds are using the definite article in the same way as adults here. What the child means by "the water" may not be the same as what the older subject means. But given the sort of cognitive ability conservation research has professed to be interested in, it would seem that this would have been a more obvious starting point than one involving an irrelevant additional container - not to mention the extra dimension afforded by the potentially misleading form of "closed" questioning employed in the bona fide C.T.

In relation to the latter point, it is safe to assume that the question, "what do you see in front of you?" is less loaded than the three-way multiple choice question asked, and then asked again, in the C.T. While some sort of reference to the amounts in the containers can be hoped for (and might be viewed to have been obtained, albeit indirectly), the open questioning manages to avoid the potential problem of asking the subject about relative amounts when the subject might not even have considered the question of relative amounts. In this respect it is significant that when an open question procedure is applied to what remains in other respects a bona fide C.T., with "what do you see in front of you?" and "what happened?" replacing Q₁ with Q₂ respectively, the following sorts of responses are obtained from subjects of all ages. To the first question: "two glasses with water [powder] in them"; to the second question: "you poured the water from that one [B] into that one [C]". Without further prompting, while the subject sometimes says that there is, or looks to be, the same amount in A and B, there is rarely any reference to the relative amounts after the transformation. To say the least, this would seem odd if we go along with the view that the significant consequence of the transformation phase is to make it look as if there is, or might be, a change in the relative amounts (see below).

While the 9th empirical note, above, might indicate the sort of experiment with which an investigator interested in conserving ability would start, it does not yield the apparently developmentally
significant result that the bona fide C.T. yields. Further, should it be insisted reasonably that to participate in a C.T. the subject involved should notice and understand that the water (powder) in container B ends up in C, in the light of the above this would tend to lead towards the unhelpful conclusion that anyone able to participate in a C.T. must already be assumed to be able to conserve.

Piaget of course was concerned with the experiment that produced the interesting result he needed. Likewise, I have not been concerned with the nature of conserving ability, nor directly interested in the question of its presence/absence in the young child. My goal has not been to introduce a new, improved C.T., but merely to provide the best, most economical, explanation of the C.T. results, it being these that the conservation debate hinges on. Let us therefore bear in mind the relevance of this interlude in relation to the involvement of a confusion of perspectives on the part of the investigator, but return to the C.T. and to the point above at which I left off.

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Going back to the question of why not just present the final array, what Piaget would presumably acknowledge is that, as it stands, there is not enough information for any subject, however rational, to work out that the answer to Q2 is "same". But what made him think that there was anything else in what is arranged that provides the missing information for the subject to come to know this as a necessary truth?

Piaget would now invoke reversing operations (see chapter 1). In his/her head, the subject has only to "play in reverse" what E is seen to do (i.e. the pouring) to see that what has ended up in C could only be the very same as what was originally in B. As it has already been agreed that what was in B was the same amount that is in A, then what is in C must be the same as amount as is in A.

There is an obvious flaw here. Given the relationship between thought and perception required by Piaget's framework and outlined above, "mere appearances" are misleading. If the appearance of the final array (whatever this is) is not to be trusted by the subject, then it also has to be allowed that other "mere appearances" may also be misleading and should not be taken on trust. And if reasons why the subject should not trust the evidence of his/her eyes are readily identifiable, this simply reinforces the point.
In the case of the C.T. doubt on the part of the subject about what E is up to is entirely justifiable. In any C.T. a subject has no grounds for trusting E, and taking anything E does or says at face value. After all, the subject is being deceived from the start into supposing that something interesting and relevant will happen at some point when it has been pre-arranged that nothing relevant will happen or change. More specifically, in relation to the transformation phase, how is the subject supposed to know that E just pours? As pointed out (see Chapter 4), it is not necessary to view the subject as being able to understand or think about the various individual interesting possibilities whereby the right answer to Q2 could be "not same". It is sufficient to suppose that the subject may be able to correctly acknowledge that such possibilities do exist.

This does not rule out the possibility of the subject doing reversing operations, but it makes it difficult to see with what necessity such a mental operation will lead to the prescribed right answer. Thus a rational subject may well "reverse" what occurs during the transformation phase. But this could involve, say, "replacing" liquid or powder presumed lost (false bottoms, sleight of hand, some interesting relevant property of the substance used, etc.) during the E's antics, just as easily as it could involve "just pouring" (in the subject's head) all that is in C back into B, and thereby seeing that there is the same in A and C. Certainly just pouring by E would not affect the relative amounts, and the subject may or may not know this. But there is no necessary reason why the most rational subject should suppose that E does "just pour" the water/powder.

But if the initial appearance suggests that there are not the same amounts in A and C, and subsequent inspection of the array plus consideration of the events and circumstances that led to it cannot yield the information that could disconfirm that belief, we have to ask why should it have necessarily occurred to the subject - however rational - to doubt that the amounts were unequal. That is, not only is there no rational way of confirming the deception once suspected, but there is no reason, as things stand at the moment, why a subject should suspect that he/she might have been deceived over the appearance.

Still omitting from consideration the possible effect of the questions on what the subject believes about the array and about what has happened, the rational subject reflecting about relative amounts would seem to be more likely to reflect on what happened to make one
quantity less than the other than on whether one quantity was less than the other.

The conclusion so far then must be that if the E succeeds in his/her aim of making the final array have a deceptive appearance with respect to the relative amounts in the containers, there is no convincing reason offered as to why even a rational adult should doubt the evidence of his/her eyes. And if doubts - for some reason - do occur, then, (1) there is no reason why doubts about other "mere appearances" should not also arise (e.g. that "just pouring" takes place), and (2) there is not enough information available to the subject to resolve these doubts and conclude that there is the same amount in C as in A, and that E just poured. Thus it has not yet emerged why the C.T. could have been supposed to function as the sort of reasoning task it was intended to be.

The support for the fact that Piaget’s C.T. is - somehow - functioning as some sort of reasoning ability lies in the evidence that younger subjects tend to get the wrong answer, while older subjects tend to get the right answer. However we cannot now say that it is the logical ability of, say, the adult that allows him/her to overcome the deceptive component we have assumed is inherent in the appearance of the final array. This is because, as I have shown, no amount of rationality can establish the necessity of the assertion: in spite of the appearance to the contrary, there is the same amount in C as there is in A. Only if we assume that, after all, the appearance of the final array need not be misleading in relation to the relative amounts in the containers, can we understand a fully rational subject arriving at the correct belief (incorrectly) believing he/she has enough information for that belief. If the amounts do not strike the subject as unequal, there is no reason why he/she should then go on to consider the possibility of inequality.

It does not require this or any other reasoned argument to prove that the array is not necessarily deceptive with respect to a comparison of the amounts in each container. For example, it is self evident that there is no reason why it should necessarily look as if there is more in A just because of the differences in levels.² And it would seem intuitively obvious that, in principle at least, it could appear from the start that there is the same in each container here, with or without the information to back
this initial belief up. In certain circumstances - and notably those afforded by the C.T. - it need not occur to the subject, at any level, that there is not the same or that there might not be the same. The experimenter's transforming act being actually of no more relevance than the noise of the traffic passing by outside, the subject not only can ignore it and disregard its consequences, but might be expected in the circumstances to be quite liable to do so.

Involvement of the Experience Error

That there have been confusions on the part of Piaget, between his own privileged perspective of what a task like "P" involved and what the episode need involve for the subject (conserving or non-conserving), is clear from this discussion. It is evident that he was mistaken in assuming that the array and the way it was manipulated necessarily posed the problem he intended. And I have indicated that he was specifically led astray through: (1) supposing that he had arranged that the final array necessarily presented a misleading appearance, and (2) supposing that, if it did, it would lead to a conflict or contradiction, and thus to a cognitive operation in the rational subject, (3) supposing that, if it did prompt such a mental operation, that operation would allow the contradiction to be resolved by purely rational means.

But the experience error is involved at a further level, and a more important one - if the aim of the investigation is to interpret C.T. results rather than, say, to decide whether young children can reason logically. Even if the circumstances relating to the array could - somehow - be argued to constitute a reasoning test, what chapters 5 and 6 show is that there is more to a C.T. than those events and circumstances that concern the array and its manipulation. Thus the possible relevance of such features as (a) the fact that the subject does not know when the episode will end, and (b) the fact that two identically-worded questions are asked in sequence, for interpreting performance should at least have been considered. This is especially so since, whatever else we might claim about the array and its manipulation, the significant feature that we have to come back to is that it is arranged that nothing relevant happens. This is the rationale of the C.T., and was a point emphasized in chapters 4 and 6. Yet the full implications of this fact, the fact that all subjects are
deceived into supposing at the start that something relevant will happen, and the possible consequences of the form of questioning, have been generally neglected. And again the source of the difficulties is to be traced to a confusion of, and oscillation between, perspectives.

One aspect that the subject and the investigator might have been guaranteed to agree about is that two identically-worded questions are asked. But Piaget disregards the potential significance of this in accounting for "failure" (see Rose and Blank, 1974), and in explaining "success" (see chapter 6).

The foundations of the conservation enterprise therefore can be seen to be inordinately unsound for supporting a useful debate about how, what and whether subjects understand. On the one hand the intended problem concerning the array is rationally unresolvable but can be ignored. On the other, there are other neglected aspects of the episode that the subject can attend to in order to answer the E's "conservation question".

The flaws here are thus able to be linked to what might be identified as an "egocentric" carelessness over the need to consider valid alternative perspectives, while not conflating them. It might be suggested that Piaget was sufficiently in tune with the young child's point of view to be able to design a context which yielded the sort of behavioural result he saw himself as needing. But it was his own privileged perspective of what he had arranged that interfered to prevent him seeing that such a finding from such a context could lend no support to his view of cognitive development. And the same is true of Piaget's optimist critics. They sought support for their view of the developmental significance of Piaget's finding, and used a similar context to achieve the "opposite" result.

D. The Experience Error and the Optimists

Insofar as they held the view that, with respect to a classical C.T. like "P", there was a reasoning requirement in there somewhere and that it was akin to the one designated by Piaget, a strong "optimist" position is subject to most of the above criticisms. However, while
such optimists would probably see the array as posing the same problem and involving the same sort of logical competence to get it right, they recognized that it was a bad test of reasoning ability. This was not because they spotted that the most rational answer for anyone is "I don't know", but because they believed it to be unfair on young children. There were seen to be various other things for the child, apart from absence of logical ability, that could interfere and result in a non-conserving response.

Thus, while the subject is still viewed as facing the problem of "seeing through" the misleading appearance of the array, he/she also faces the problem of seeing through the other potentially deceptive components of Piaget's task. An important advance, in relation to the expansion of the investigator's point of view beyond the real world of objects and its logic (see Rotman, 1977, p. 181-2), lies in the recognition that there is more to a C.T. than an array that changes its appearance. The C.T. is therefore now acknowledged to be a social interactional setting requiring, for example, inferences by the subject about E's intentions in relation to what the latter has arranged.

In spite of this, the view of the significant features of the C.T. remains a distortingly narrow one. "Other potentially deceptive components" such as, (1) nothing relevant actually happens while all subjects are led to expect that something will happen, (2) the subject does not know how long E intends the episode to last, and (3) the characteristic form of questioning, tend to be neglected. Instead the focus remains on the array and its manipulation, the sought for deception being one that might influence how the young child views the intended problem concerning the array.

With a more radical orientation it might have become apparent that C.Ts in general are unable to do the sort of developmental work that has been assigned to them. It is not just that Piaget's classical version is artificial; it is inherent in the nature of any bona fide C.T. to be an artificial social setting, potentially influential deceptive elements never being able to be excluded.

With mention of this last point, it will be seen that the involvement of the experience error becomes particularly conspicuous when the rationale behind the optimists' empirical project to turn failure into success is examined once more.
The fact that Piaget’s negative findings from the classical C.T. might be able to be interpreted as "false negatives" prompted the introduction of modified C.T.s such as "O". With this sort of experiment the optimists set out to:— (1) use as a reasoning task a context that is still a bona fide C.T. of the same form as "P". But they aimed to (2) employ content that is more meaningful to the young child, and to (3) make it a fairer test by ensuring that the fact that a transformation of the array comes about is emphasized as being incidental" to the intended point of the episode. By so doing, the intention was to (4) produce the opposite result, eliciting a successful conserving response where before there had been failure. In turn such a result would (5) demonstrate that Piaget's negative finding was likely to be a false negative, and (6) suggest that some other sort of developmentally significant failure is responsible for the non-conserving performance of the young child on a task like "P".

At the start the potential danger of what I referred to in chapter 4 as the "optimists' cleft stick" can be discerned. To reiterate: from their point of view they are presenting a reasoning task of the same essential "form" as "P". But if they are mistaken, then the results of "O" cannot be assumed to be able to shed any light on why the child fails on "P". On the other hand, if they are not mistaken, we need to be convinced that what is wrong with "P" as a test of reasoning ability is not also wrong with "O". My argument was guided by the latter point. Hence I emphasized the similarity rather than difference, indicating that the same background of deception and lack of information exists in both tasks.

The second point, relating to the aim of making the task content "more meaningful", is equally suspect. It can be seen to beg the question of what is the meaning of the C.T. episode for the subject. And a major part of this investigation has revolved around clarifying what we are entitled/not entitled to assume about the subject's point of view of what is arranged for him/her. Indeed, the central point of this chapter is that there has been an inadequate treatment of meaning through a confusion of, and oscillation between, points of view by all concerned. Somewhat paradoxically, as I have demonstrated, a little more care in this respect would have led to a simplification of the final job of interpreting performance. Instead the trend has been one of spiralling complexity. Thus a recent paper (Pratt, 1988) reports a suspected "important link between [C.T.] performance and
metarepresentational ability" (see also Hundeide, paper in preparation, mentioned in chapter 9 (C)).

It is in relation to the third point that the confusion becomes most conspicuous. In the first place, on what basis are we to judge that "O" is a fairer and better task than "P"? As Light et al recognized, the mere fact that a right answer might now be elicited where before there would have been a wrong answer does not show that it is a more sensitive index. And examination of the manner by means of which "fairness" is intended to be achieved only enhances such doubts. For a start, in intending to "emphasize that the transforming event is incidental", the experimenter here is explicitly aiming to communicate an untruth. The transformation, he/she knows, is not an incidently occurring event. Not only is it intended from the start, but it is intended to be the crucial event that (a) brings about the change in appearance, (b) thereby poses the "conservation problem", and (c) characterizes the episode as a C.T.

However, not only does this aim reflect a further intention to deceive, but should the fact that the transformation is emphasized as incidental appear in the explanation of why young children succeed on a modified C.T. like "O", the optimists run the risk of psychological incoherence. It may be a fact for the investigator. But from a psychological point of view it is a contradiction in terms. What occurs as "incidental" to the project in hand for the subject is exactly that which is not emphasized. And to emphasize that something is incidental is likely to de-emphasize its significance for the subject even more.

The implications of this for providing a parsimonious and psychologically intelligible account of why the child should be more likely to give a conserving response on "O" were seen to be profound. The E intends that the subject should notice and believe that the manner by means of which the transformation of the array comes about is irrelevant. But a possible result of this is that the subject will now notice nothing of significance in relation to the transformation. That is, the subject is liable to disregard not just (a) the manner of the transforming act, but also (b) the intended "fact" that a significant transformation has occurred, and thus (c) the intended perceptual consequences of the transforming event.

Nevertheless to attempt to offer an explanation of the subjects' observed conduct (i.e. performance) in terms of that which is ignored by the subject is psychologically illegitimate, given the criteria I have
adopted. Just as in the case of allowing reference to the fact that a deception has occurred to form the basis of a psychological interpretation of some aspect of conduct, this would involve a conflation of perspectives and thus be to perpetuate the very error that I am attempting to eliminate. But it was useful in my analysis as it allowed me to recognize that on any C.T. the subject can get away with ignoring not just the transformation, but all aspects relating to the array and its manipulation. It therefore prompted a switch of emphasis away from the events and circumstances concerning the array, to those events not able to be ignored by the subject - notably E's questions.

In relation to the fourth point, the switch of emphasis in relation to the view to be taken of the significant events of the C.T. alters the way the results of C.Ts are to be viewed and described. What, in cognitive terms, is potentially impressive failure (on "P") is now to be seen as being replaced by apparently totally unimpressive success (on "O"). This can only make us more sceptical about the psychological usefulness of talking about these as "opposite" responses. It also raises questions about the appropriateness of applying the "failure" and "success" labels in the first place.

When it comes to explaining why "O" achieves a different result than "P", due importance can now given to other events and circumstances apart from those concerning the array and its manipulation, all of which can be disregarded by the subject. In relation to the questions and the relationship between them, it might have been acknowledged - but for the bias in favour of the investigator's perspective of what he/she arranges - that there was a much more straightforward explanation of the "success" findings on "O". Thus, instead of trying to argue that this was the result of making the C.T. a "fairer" test through "emphasizing that the transformation was incidental", the fact that in "O", unlike in "P", there is an explicit request for repetition of the answer already given (or assumed given) to Q1 might have been noted. It is this sort of observation that would have made the task of interpretation simpler rather than more complex (see above). Turning to the fifth stage in the optimists' rationale, their aim is to show that the wrong answer on "P" is compatible with the presence of reasoning ability. In the light of the above, all they succeed in doing is showing that both right and wrong answers on both "P" and "O" are compatible with both the presence and absence of reasoning ability. The conclusion from their project thus far is therefore not that the young child has the reasoning
competence in question, but that "P" is a hopeless test of it and that "O", for the same reasons, is no better. In both tasks nothing happens, something might have happened but the subject is not given enough information to decide either way.

However, the optimists accepted that Piaget's finding posed an interesting developmental question, going on to offer their own accounts of why young children fail while older subjects succeed on the same task (e.g. "P"). But the sixth point is that the fact that such similar contexts as "P" and "O" could be used for opposite ends might - with a less restricted perspective - have enabled them to challenge the assumption that the C.T. could be used to make any significant developmental point.

Instead, what we note is the perpetuation and compounding of Piaget's experience error throughout the optimists' project. The aspects that were significant to them tended to concern only the array and its manipulation, and the way they might influence the subject's understanding of the arranged "problem". They explicitly demanded that more attention be given to the subject's point of view. But they continued to neglect features that might be important in this respect - e.g. the temporal aspect and the characteristic form of questioning. Their confusion prevented them from seeing that their modifications do not help - as intended - the subject to understand that the significant aspect of the episode is that nothing relevant happens (conservers are left still waiting for the expected something to happen). But a by-product of that modification ("where were we?", etc.) is likely to suggest to the subject that the answer to Qz is "same".

E. The Experience Error and the Optimists' Critics: Laying the False Negative-False Positive Controversy to Rest

In terms of combatting a confusion of perspectives on the part of the investigator, the positive aspect in the work of those such as Neilson et al (1983 (a) and (b)) was the recognition that what the optimists intended with their modifications might not be what they got - apart from the eliciting of the desired right answers. Another good feature of their challenge was that it took them to a broader view of
the subject's conduct. Thus they saw the need to consider elicited behaviour other than that constituted by the conserving/non-conserving answers themselves. For example they took into account the fact that in the "naughty teddy" version the conserving child frequently attempts to restore the "accidentally" transformed array to its former state.3

But again they were mistaken about what, in principle, a C.T. can and cannot be used to show. Thus they too were unable, or unwilling, to take their insights to the point at which it becomes possible to question whether the mixed bag of C.T. findings have any developmental significance. Instead they were led in the direction of trying to shore up the conservation enterprise. In particular, as was seen in chapter 3, this took the form of applying precautionary measures to ensure that, when correct "judgements" were obtained, true positives could be discriminated from false ones. The specific aim was to ensure that, (1) the subject attends appropriately to what he/she is intended to attend to, and (2) right answers are accompanied by appropriate and adequate justification. In both cases my main criticisms revolve around problems that must be associated with the use of the term "appropriate" here. And the intrusion of the experience error is to be detected in the fact that the investigator is misled as to what is actually appropriate by what he/she intends to be noticed and understood "appropriately".

In chapter 3 I have already discussed (e.g. in relation to Bryant, 1982 a, p. 4-5) the difficulties that the attentional requirement runs into. With particular reference to the modified C.Ts., we are told that the subject, while he/she may disregard how and why the transformation comes about, must be made to attend properly to the consequences of the transformation. We saw, however, that problems arise, (1) through the need to arrive at a suitable measure of the subject's attention to the final array, and (2) through the legitimacy of requiring the subject to attend properly to the consequences of the transformation. In relation to the first point, simply making sure that the subject notices that the transformation has a certain result is not enough. We also need to know how he/she attends to it, and what importance he/she attaches to it (e.g. see Bovet et al, 1981). But consideration of the second point allows the first difficulty to be put into perspective. Now it is acknowledged that the consequences of the transformation not only are irrelevant in relation to the relative amounts involved, but are intended to be understood as irrelevant to the answering of E's question
about relative amounts. The most appropriate way to "attend" to irrelevance is not to attend to it all.

The only way to argue that it is appropriate to attend to the result of the transformation is to switch to the subject's perspective, pointing out that he/she does not know that it will be irrelevant. This is exactly correct. And it is also correct that the closest inspection will not provide the missing information. With this reminder that the wrong answer is always in some way justifiable, this takes me to the problem of demanding "appropriate" justification of the right answer. This is another case of the "false positive" critics taking into account conduct beyond mere performance. But there are fundamental reasons for supposing that the purpose that retrospective justifications were intended to serve was an illegitimate one.

I discussed in general terms the problems arising through the asking for justification to gain access to what the subject did and did not understand (see chapter 3). At a less general level I pointed out that the "judgement" in the C.T. which is to be justified itself does not occur spontaneously, but because E asks for it (see also this chapter, section C). So it may already be a rationalisation of what the subject experienced and understood, rather than a true reflection of what he/she understood at the time of the events in question. To ask for a justification on top of this is liable to generate an account of the subject's processes of understanding even further removed from what actually took place.

Thus, without actually considering what is to be rationalised, there were independent reasons for not bothering with justifications. But now, given the essential nature of the C.T, there is a much more specific reason for excluding them from consideration.

The false negative - false positive dispute can be directly linked to the issue of justification. This is seen in the respective claims of the optimists and their critics: that (a) classical tasks like "P" may produce justifiable errors from the child, and that (b) modified tasks like "O" tend to produce right answers without the possibility of accompanying adequate justification. The optimists might argue that the right and wrong answer by the child on Piaget's version are equally justifiable, given the procedure. But, having introduced what is claimed to be a fairer version in "O", they would presumably allow that negatives - if and when they occur here - are more likely to be true negatives. Failure on a fair test is to be taken as real failure. The
point is that negatives here are seen as less justifiable than positives — irrespective of whether or not justifications are requested. (For sound general reasons McGarrigle and Donaldson refrained from using such evidence). Thus all sides in the dispute assume:

(a) the possibility of obtaining adequate justification of a right answer on a C.T. from someone (e.g. an adult), and

(b) the possibility that a wrong answer on some version of the C.T. will be manifestly less justifiable than the right answer on the same task.

To have any potential use as a test of reasoning ability, both these assertions about the C.T. must be true. In relation to the first point, however, we can now see (1) that there is never, on any C.T., enough information to fully justify the conserving response, (2) that the required justification undermines itself. Thus the sort of justification stated to be required is that which shows that the subject has attended properly to the consequences of the irrelevant transformation, deciding that the appearance (whatever this may be) is irrelevant. But, given its arranged irrelevance, a much more sensible and justifiable response is to ignore it, along with the multitude of other irrelevant things going on — like the E’s mannerisms and the coincidental noise of traffic passing outside, etc. As was seen, the evidence suggests that subjects (however old), when they succeed on a C.T. can be taken to get the right answer in a way that involves such negligence.

As I have just pointed out, the requirement that the subject should attend properly to the array and its transformation is only itself justified if it is acknowledged that something relevant to judging the relative amounts could have happened. Given the lack of information, this is the case and answers the second point. That is, the wrong answer is always as justifiable as the right answer because something relevant — while it never does — always could have happened.

Still disregarding the actual evidence of justifications (because of its assumed unreliability), and ignoring for the moment the possible effects of asking for a justification of a right or wrong answer, the following conclusions are arrived at:

(a) If the measure of the “falseness” of a positive is to be taken as the degree to which the justification offered is inadequate, then all positives that occur must be judged to be false. This is simply because, on any C.T., accompanying, adequate justification is never
possible, the only potentially completely justifiable answer being "I don't know".

(b) If the potential for a negative being in fact a false negative is to be taken as the degree to which the validity of the best sort of justification that could be offered for the wrong answer approaches the validity of the best justification that could be offered for the right answer, then all negatives must be judged to be potentially false. This is simply because, on any C.T., wrong answers are always as justifiable as right answers.

If the nature of the C.T. is such that an elicited correct answer can never be judged to be a true positive (being able to have been arrived at through something other than the exercising of rational ability) and a wrong answer is always, potentially, a false negative (hiding a rational accomplishment), then the attempt to discriminate true negatives and positives from false ones must founder. Yet the conservation controversy, outlined in chapter 3, revolves around the dispute as to which of the negatives and positives obtained from the different versions of the C.T. are to be taken as true ones. If the distinction was not appropriate in the first place, we are now able to understand the difficulty encountered in resolving such questions. The obvious alternative is to seek a valid perspective on the C.T. that avoids viewing (and describing) the results in terms of failure (negatives) and success (positives). This was the strategy that resulted from the challenging of the various identified assumptions. And, as well as allowing an understanding of the conservation muddle to emerge, it permitted me to sidestep it as the first move towards offering a parsimonious level of explanation embracing all C.T. results.

While the justifiability issue, and the implications for interpreting elicited "non-conserving" and "conserving" responses, is of central importance to my thesis, it arose here as a by-product of my criticisms of those who sought to use justifications of right answers to shore up the conservation enterprise. Their intention was to weed out those conserving responses that were inadequately justified, dismissing them as "false positives". What we now see is that, apart from (1) the general unreliability of such evidence, and (2) the problem that a right answer here is never completely justifiable, we have to accept (3) that it is inappropriate, and perhaps unjustifiable, to ask for justifications from "successful conservers".
With the transformation and its consequences disregarded, we saw that all the subject needs to do to "succeed" is recognize \( Q_2 \) as the same as \( Q_1 \), and remember the approved answer to the preliminary-phase question, \( Q_i \). Such a subject would be justified in supposing that, as yet, there is nothing that requires justification. To then be asked for a justification of the taken-for-granted answer to \( Q_2 \) ("same") might be anticipated to be confusing - perhaps as confusing as being asked to justify why he/she ignored the traffic noise outside. In fact we can see - with adult or children conservers - that the request for justification frequently tends to induce (justifiable) doubts that the answer was "same".

In general, therefore, we can conclude that it is unfair to expect a subject to put together a decent justification from events and circumstances the significance of which he/she can justifiably disregard. As regards the resulting evidence: certainly this is liable to lead to the patently inadequate and inappropriate justifications that the pessimistically - inclined "false positive" critic needs. But the significance of such evidence remains obscure. It can be added that if it is inappropriate to ask for and expect an adequate justification of a right answer, we can presume that it is also misguided to expect to learn anything from the justification of a wrong answer.

There was a step in the right direction here in that, with the shift in emphasis away from a consideration of performance on \( Q_2 \) alone, the evidence sought was intended to reveal more about the subject's point of view - particularly with respect to the modified tasks of the optimists. But the significance given to that evidence betrays the illegitimate intrusion of the investigator's point of view. And we need look no further than the fact that from their point of view an adequately controlled C.T. (a) constitutes a reasoning problem that must be attended to, (b) has a right answer that can be fully justified, and (c) has a wrong answer that cannot be justified so well. From the subject's point of view of any C.T., there might be no problem and thus nothing to be expected to be called on to justify. They were right to point out that the subject was likely to get a task like "C" right without doing the reasoning operation intended. But an overly narrow cognitive perspective prevented them from concluding that this might indicate the way that anyone could be viewed to get any C.T. right.
given that all C.Ts are designed around the fact that nothing relevant actually happens.

The confusion of perspectives on the part of those "false positive" critics thus led them to overlook that their "negligence" explanation, in relation to conserving children on a task like "0", (1) told us more about flaws in any C.T. than flaws in the mind of the child; (2) suggested a way of viewing "success" by anyone on any C.T.; (3) did not constitute, on its own, an adequate psychological explanation of the conduct in question (see chapter 6); but (4) pointed the way to a parsimonious explanation i.e. in terms of what the S and the investigator cannot ignore (the questions asked).

F. Implications: Overcoming the Experience Error Alters the View Taken of what the Subject Responds to

(a) Introduction

The business of psychology is to arrive at an adequate understanding of what people do and why they do it. The business of psychologists has been seen to be to interpret observed conduct, evaluating existing interpretations and attempting to improve on them. The first step here might be seen to be to identify and agree about the subject's response that needs to be psychologically interpreted. However, before that it has to be assumed that a level of agreement has been reached about what is responded to.

Taking a contrived experimental situation, this descriptive base must be derived from the investigator's point of view about the significant aspects of what he/she has arranged. This point of view cannot be excluded from the investigation, and most researchers would neither try nor want to exclude it. But - looking beyond behaviourism - what a subject does in an arranged situation is to be related to, in the first place, the investigator's view of the meaning of the situation for that subject. That is, a properly psychological approach aims to gain access to the subject's point of view of what is arranged, and to give an account of how this point of view - and the conduct linked to it - comes about. The investigator must make assumptions about what are the
significant aspects of what he/she has arranged. But, by being able to distinguish his/her own perspective from perspectives available to the subject, he/she should be able to discriminate those facts and assumptions that can be legitimately and usefully referred to in a psychological account from those that cannot be.

In any experimental situation we can presume that there will be a discrepancy between the investigator's and the subject's point of view of what takes place. The investigator knows things that the subject does not know and the subject may notice things that the investigator does not notice, or disregards as irrelevant. In terms of the item of conduct of agreed interest that is to be psychologically interpreted, privileged information may give the investigator an insight into the reasons for the subject seeing the situation as he/she does and thus into his/her observed response. Likewise the investigator may be justified in disregarding something when it comes to offering an account of the conduct in question, even though the subject does not disregard it. However, as we have seen, the investigator must be on guard against allowing an illegitimate intrusion of his/her own point of view into the investigation, such that (1) facts that are unavailable to the subject are referred to as if they were available, and (2) facts that are available to the subject and which could influence the conduct in question are neglected.

What I have argued in the present chapter is that there has been such an illegitimate intrusion of the intellectualist point of view of these conservation researchers working within their explicitly cognitive framework. A preoccupation with the problem they intended the subject to encounter led them into confusion with respect to how that which is to be responded to is viewed and described. This narrowness of perspective in relation to the significant events and circumstances in turn led to a correspondingly over-narrow view of the conduct to be taken into consideration. And, in line with the intellectualist bias, this led to an overestimation of the cognitive accomplishment that (1) is required in responding in the approved manner, and (2) can be attributed, given the evidence, to "conservers".

On the one hand there has been an arbitrary - and thus challengeable - emphasis on viewing the "appropriately controlled" C.T. as a problem concerning the array and its manipulation. There has been a corresponding neglect of other aspects that could (a) influence how
the subject performs, and, when taken into account in the investigation, could (b) influence how that performance is to be in the first place viewed and described. On the other hand, within the confines of that narrow perspective, facts about the array and its manipulation have been referred to as if they were available to, or deducible by, the subject, when this was not the case.

Ideally, the attempt to eliminate the experience error would not only highlight where all these investigators went wrong in their various efforts to establish what particular version do show. It would also allow that which they sought to explain - i.e. the mixed bag of C.T. findings - to be demonstrably more adequately explained. The latter achievement does not necessarily follow; for eliminating the experience error may just lead to the conclusion that previous types of explanation were not justified - without allowing a satisfactory account to emerge.

Indeed, in normal circumstances this would be the more likely outcome. Since a disciplined attempt to avoid a confusion of perspectives involves sticking only to those facts that everyone - including the subject and experimenter - can be assumed to be able to agree about, this tends to lead to an impoverished, "lowest-common-denominator" view of what takes place. Not only is the mention of potentially useful facts that only the investigator can be privy to eschewed, but there will inevitably be a patently incomplete view of the meaning for the subject, this being restricted to a level of description we can be sure the subject can agree with the investigator on. Thus it would generally be overoptimistic to suppose that such an initially sparse descriptive base could support the interpretative burden placed upon it when the item of interesting conduct is considered.

But we are lucky (?) here insofar as the peculiarly impoverished nature of the C.T. lends itself to this strategy. The one and only time we need to look beyond the subject's point of view is at the start, before the subject appears on the scene. Thus we take cognizance of the fact that, whatever else the C.T. is intended to be and do, as a C.T. its basic design feature lies in the fact that nothing relevant to what is to be asked about must happen. It is this that makes it an ideal context for demonstrating (1) the dangers of conflating perspectives, and (2) the value, from the point of view of psychological interpretation, of rigour in identifying likely investigatory prejudices and in avoiding the consequent confusion. Needless to say, however, there is a severe price to be paid, given the work the C.T. was intended
to do and the sort of interpretative burden placed upon the mixed bag of C.T. findings obtained from young children.

I have already indicated in the previous chapter what the final price will be. My aim now is simply to reconsider the descriptive starting point, showing how my conclusions may be seen as directly deriving from a disciplined attempt to avoid the experience error. The claim being considered is that the mistakes made all round here were based on essentially the same confusion of perspectives. It should therefore be possible to show that adhering to a perspective based on such an attempt to eliminate the experience error at least highlights the mistakes, and at best allows a more adequate interpretation of the performance findings to emerge. The best way to show why the accepted "cognitive" emphasis was liable to lead only to confusion and controversy while the "alternative" emphasis results in clarification, is to explicitly pit them against each other.

(b) Two contrasting ways of looking at the facts about the C.T. ("P" and "O")

Perspective (a): The cognitive point of view

(1) At the start there is an array consisting of the glass beakers A and B with the same amount of water/powder in each.
(2) The experiment can only proceed on the assumption that the subject (S) understands this.
(3) The contents of B are then simply transferred into the wider container, C.
(4) This results in a significant alteration in the appearance of the array.
(5) The experiment has reached its conclusion (ignoring subsequent requests for justification) when a judgement about the relative amounts in A and C has been given.
(6) The E's interest is in whether or not the S understands that in spite of the altered appearance of the array, there must be the same amount in A and C.
Perspective (b): An alternative point of view

(1) A multiple-choice question requiring an answer is asked (Q₁).
(2) The episode proceeds when the approved answer is given.
(3) An identically-worded question requiring an answer is asked (Q₂).
(4) The E is interested in what sort of answers the subject gives to those questions that occur during the episode.

On the one hand we have the episode described solely in terms of the events and circumstances concerning the array and its manipulation. On the other hand we see it described solely in terms of a question-and-answer situation (an "interrogative context"). Both descriptions involve only incontrovertibly true facts. The crucial difference, however, is that while any S can be presumed to agree with all those assertions listed in (b), he/she cannot be presumed to agree with any of the facts in (a). In pitting the two perspectives against each other we need to know how far they get on their own in supporting psychological interpretations of the conduct that results from the C.T.

(c) The cognitive perspective: the array and its manipulation alone

On the cognitive perspective - that is, through consideration only of the events and circumstances concerning the array and its manipulation - it should be clear (1) why the question of relative amounts should be an issue - something to be thought about - at the point where Q₂ would have been asked, (2) why it should pose a problem, (3) why the presence of a rational, problem-solving ability would be more likely to lead to a correct belief about the relative amounts, than to an incorrect belief, and (4) why the absence of this ability would be more likely to result in an incorrect belief than a correct belief.

The discussion of Piaget's rationale in section C above, which is equally relevant to his critics and their critics (notwithstanding attempts to improve the procedure), shows that each of these points can be challenged. In other words, in terms of what is required, the cognitive perspective is deficient. The investigator needs the issue of relative amounts to pose a problem. We can pass over the difficulty of turning it into a resolvable problem while preserving the characteristics of a bona fide C.T. From those statements that
constitute perspective (a) as it stands, there is no necessary reason why relative amounts should even be an issue for the subject, let alone a problem. Nevertheless we have to presume that it was an issue with respect to the initial array, as the episode can only proceed on the assumption that S understands the equivalence of the amounts in A and B. We can let this go, pointing out now that there is no necessary reason why it should remain an issue. As everything that subsequently happens to the array is actually arranged to be irrelevant to the issue of relative amounts, it becomes impossible to point to a necessary reason why it should remain an issue for S - let alone one subject to doubt or speculation.

(d) Combining the two perspectives

To ensure that relative amounts can at least be seen as an issue for the subject on encountering the final array, it would seem that we need to borrow from the "alternative" perspective. The significance of the questions must be taken into account. The cognitive perspective did of course recognize the need for asking questions in running a conservation experiment. But they were seen (pace Rose and Blank's - albeit restricted - treatment of the questions) as having only a referential function. They were needed to direct the subject to the arranged problem, and because answers were required if the subject's understanding of the problematic situation was to be tapped.

Indeed, what is referred to in the wording of the questions would seem to ensure that the judging of the respective amounts of water/powder in the containers is at least made an issue, both at the "establishing equivalence" phase (Qi) and at the final, post-transformational phase (Qz). However, Qz may do more than this.

The asking of the questions can be presumed to communicate to the S something about E's intentions. And now we see that, depending on how the question is taken, Qz may actually affect any existing belief about the relative amounts. For instance, if the S assumes - albeit with insufficient information to do so - that it is obvious that there is the same in each container before Qz is asked, the asking of that question explicitly referring to the amounts may give rise to (justifiable) doubt. The S may take the view that the question about amounts would not have been asked if it was that obvious. The asking of Qz, in this case, may
lead to a change in belief and to a "non-conserving" response. Similarly for the case where S assumes, prior to Q₂, that it is obvious that there is not the same in each. Here the asking of Q₂ might change that belief and result in the "conserving" response. That is, because of what it refers to, Q₂ has the potential to change the situation that S is participating in, change an existing belief about relative amounts and thus exert an influence on performance.

But we have seen that the asking of the questions can influence performance by another route. When considering responses to questions, we not only have to remember their function and possible effect (see above) in referring to certain events and circumstances. We may also have to bear in mind their independent properties as non-transparent events themselves.

(e) The alternative perspective: the C.T. reconsidered solely as an interrogative context

In chapter 6 I argued for considering the C.T. solely as an interrogative context in the interests of providing parsimonious answers to Question A and B. Here the aim is simply to show how considering it thus avoids the experience error and the problems arising from it. Inevitably, however, points already made recur, but with a different emphasis.

I have shown in chapter 6 that not only can we emphasize the events that are constituted by the questions at the expense of those relating to the array, but we are able to dispense with the latter altogether. In other words a consideration of the referential function of the questions, beyond the fact that they refer to something, is not needed. This is because what is referred to by the questions can be disregarded by the subject - and thus also by the investigator in describing what the S responds to, and in interpreting the subject's conduct.

By making the questions the focus of interest it might seem that, as our concern will be with making sense of the subject's answers, the first thing to be looked at is the meaning or possible meaning of the words occurring in each question. Normally this might be so. But here there is another potentially significant feature that may be taken into account. This is the relationship of the questions to each other - independent of what the words are intended to refer to and might mean
for the subject. A fact available to all is that the two questions are identically-worded. Even so, the recognition by someone that an encountered question is identically-worded to a question previously encountered (and answered) might in normal circumstances be of only passing interest. A reason is needed why, in these circumstances, this fact might be viewed by the subject as being a significant one, and therefore be posited to exert an independent influence on S's response to Q₂.

My specific concern is with S's point of view of the relationship between the first and the second question. However, at this point, just because this is my concern, I have to consider aspects only available to the privileged perspective of the investigator. This is necessary simply because when exploring potentially available meanings it is important to establish what is un-available. What we know is that the questions refer to the array and its manipulation and that the net effect here is that nothing relevant happens. And we also know that not only is the subject never in a position to know or deduce this, but also he/she has additionally been deceived in to supposing at the start that something relevant will happen before the end of the episode. Further, we know that if the questions had been omitted the S would have been unlikely, especially after the pouring (transformation), to see the relative amounts as an issue at all (see 9th empirical note).

But the questions are asked. For convenience we can assume initially that we are dealing only with subjects capable of reasoning logically. We can also remember that "I don't know" is not one of the offered alternatives. Such a rational subject, having given the approved answer ("same") to Q₁, now hears Q₂ and tries to make sense of that to which the question might refer. But we can now see that the subject is liable to encounter only confusion.

Fortunately for this subject there is the above-mentioned feature of Q₂ that, if attended to, may allow him/her to answer it reasonably without getting involved in the unresolvable confusion over that to which the question might refer. That is, the question of the relative amounts in the final array and what E might or might not have done to affect them can be put aside if S decides to act rather and only on the fact that Q₂ is identically-worded to Q₁. I have considered here the rational subject. But there is no reason why this should not equally well apply to the non-rational subject. For he/she too may simplify matters by disregarding that to which the question might refer. But
he/she must (if he/she is able to participate at all) be aware that questions requiring answers are asked. He/she also can be presumed to be able to recognize that \( Q_2 \) is identically-worded to \( Q_1 \), and remember what the given/approved answer to \( Q_1 \) was.

What the subject, having acknowledged this fact, makes of it is another question (see Chapters 6 and 7). But the important point is that the fixed events and circumstances applying to any C.T. cannot actually help him/her to know what to make of it, whereas other, "extraneous" events and circumstances may provide - and may be seen to provide - concrete clues in this respect.

(f) Implications of the comparison of the two perspectives in relation to the view to be taken of what is responded to

Perspectives (a) and (b) were pitted against each other. It was seen that the former point of view of the significant events and circumstances of the C.T. could not provide the sort of descriptive base able to support the interpretative burden placed upon it. Whatever "appropriate controls" are applied, given the nature of the C.T., right answers can never be taken to indicate that the subject has the envisaged reasoning ability. And wrong answers can never be taken to indicate that the subject does not have that ability. On the other hand, the latter point of view - based on eliminating the experience error - has the potential to provide a totally adequate descriptive base whereby right and wrong answers can, in principle, be accounted for: i.e. as alternative and equally justifiable responses to an ambiguous interrogative context. In any particular case we know what sort of additional aspects of (1) the context, and (2) the subject's conduct (see next section) to take account of with the latter perspective, but not with the former.

As mentioned, the price to be exacted for implementing the "alternative" perspective is a heavy one. In the first place, difference in performance between subjects cannot now be interpreted in terms of a difference in any reasoning ability. If a rational ability is to be invoked, we would first have to pitch the description of the sort of logical operation involved at a very low level - along the lines of: (1) questions require answers (for which there are alternatives); (2) this \( (Q_1, Q_2) \) sounds like a question (with offered alternatives);
(3) therefore I will answer (from the available alternatives). This might explain the fact that subjects answer, but it cannot explain the answers chosen in particular instances. That is, it does not distinguish between conservers and non-conservers who both offer answers from the alternatives provided by the multiple-choice questions, Q₁ and Q₂.

Beyond this, we might invoke the following operation; (1) a multiple-choice question, for which the approved choice of answer is "same", is asked; (2) later an identically-worded question is asked; (3) therefore, remembering the approved answer for the first question, I will answer the second question in the appropriate manner. But again this does not distinguish between conservers and non-conservers. This is because repeating the original answer and changing it are both "appropriate". A developmental point, other than one to do with the presence/absence of reasoning ability, might still be made by pointing to the tendency of younger subjects to be non-conservers and older subjects to be conservers. But, on the "alternative" perspective, this would involve having to say that older subjects tend to take the second of two similar-sounding questions as a repetition, while younger subjects do not. Apart from a tendency to expect/not expect repetition being an obscure trait from the point of view of its psychological significance, it would be hard to support as a general assertion with much relevance beyond the narrow confines of classical CTs.

This is the negative side of things. The positive side is that an alternative view of what is to be responded to - one based on the attempt to eliminate the experience error - leads in at least one respect to an expanded view of the response to the C.T., i.e. that which includes performance and is to be interpreted in a psychologically relevant way.

G. More Implications: Overcoming the Experience Error Alters the View Taken of the Subject's Response

In terms of the subject's observable conduct, both of the above perspectives would probably grant that the evidence from C.T. performance on Q₂ alone is not sufficient for their respective
interpretative projects. But, from what has been said, the problem with the "cognitive" perspective is that in the end we are left not knowing what sort of additional behavioural evidence to go looking for. Tacking on an attentional requirement, as we have seen, does not help. Nothing relevant actually happens, and the subject does not need to attend to anything related to the transformation – including its "problematic" consequences. Again, asking for justifications seems to be a doomed exercise in terms of what could be learnt from them.

Because a cognitive perspective, almost by definition, is concerned with problems and solutions (or attempted solutions), those adopting it tend to lose interest in a subject's conduct after the answer is elicited in response to the specified problem (apart from post-episode justifications). Hence it becomes a little more comprehensible why the significance of potentially crucial evidence as to what happens after the subject's performance on Q₂ – notably S's reaction to E's termination of the episode – was generally overlooked or disregarded.

Such evidence, as well as that relating to the manner in which S answers, cannot be neglected on the alternative perspective. Being concerned with the question of whether or not, in a particular instance, the subject takes Q₂ as a mere repetition of the preliminary-phase question, Q₁, it becomes a matter of central importance how the subject answers and whether or not he/she expects to be questioned further.

Those adopting perspective (a) might not be too perturbed by the evidence of a correlation between correct performance and expecting there to be more to come at Q₂. Presumably they would view things the other way round: i.e. the latter as a by-product of S having arrived at the explicit beliefs that there is the same in containers A and C and that the transformation could not have made a difference. But such a view still faces the difficulty of showing how such conclusions could ever be arrived at as necessary rational truths. This is the case whatever "suitably controlled" version of the bona fide C.T. is employed.
H. Conclusion

I have suggested that in a C.T. what the subject responds to may be viewed solely as an interrogative context. The subject's conduct may be viewed solely in terms of his/her response to the form of questioning, and independently of that to which it refers and might be taken to refer. As anticipated, this view offers us an unduly impoverished picture if we are concerned with what it means for a child (or adult) to participate in a C.T., and with what is going on psychologically in any particular instance of "conserving" or "non-conserving".

However, the point is, (1) that it works in supplying a parsimonious, psychologically-relevant account of performance: the account is intelligible both in terms of the chosen, narrow view of the arranged episode and in terms of the additional behavioural evidence able to be obtained; (2) it draws attention to where the dominant cognitive perspective on the C.T. went wrong; and, related to this; (3) such a strange, impoverished episode deserves such an impoverished treatment.

The prescriptive message here is that, before investigating "interesting" responses, we should be clear that there may be alternative ways of viewing what is actually there to be responded to. More specifically, in designed question-and-answer situations, before setting about interpreting and evaluating offered answers we should look hard at the nature of the questions asked. I have indicated that this is relevant to making psychological sense of what subjects ("conservers" and "non-conservers") do in C.T. situations. But it is also relevant for making sense of what investigators in this area of psychological research have done. When we look closely at the sort of questions that those caught up in the conservation controversy sought answers to (e.g. Questions A and B) we find that they are wrongly, or at least misleadingly, posed.

That this was not recognized, I have argued, was due to the confusion of standpoints that is a consequence of the cognitive approach's intellectualism. As a result: (1) challengeable assertions were made, and unchallengeable facts mistakenly neglected; (2) there was an overestimation of what, cognitively, is involved in "conserving"; and (3) an unduly narrow view was taken of what might constitute the
relevant evidence. That is, all along the line mistakes attributable to what I have labelled the "experience error" have been made.

Instead of paying proper attention to the implications of the inevitable mismatch between the investigator's point of view of what he/she arranges and that of the subject, there was an illegitimate oscillation between these perspectives which could only lead to psychological incoherence. Specifically the confusion:- (a) led in the first place to the C.T. being seen as potentially useful to cognitive developmental psychology; (b) led to challengeable descriptions of the significant features of the C.T., the key feature that nothing relevant happens, apart from the questions, not being adequately taken into account; (c) led to flawed descriptions of the results: the terms "success" and "failure" are inappropriate, as is the assumption that "conserving" and "non-conserving" responses here are usefully viewed as "opposite"; (d) led to an unwarranted interest in the results and a flawed approach to interpreting them - i.e. as the products of reasoning tasks. Finally it (e) led to a huge fuss and a colossal amount of literature.

A better way of explaining the results was available and would have involved viewing any C.T. as, in the first place, a peculiarly ambiguous interrogative context. The significance of the form of questioning was largely overlooked. And the question that arises is why such a crucial neglect of the "non-transparent" aspect of the interrogative events (questions) is a predictable one on a cognitive perspective.

On a cognitive view, the world to be given meaning to is seen as posing an intellectual problem. As such it is no great distance to a view of the world as one big, ready-made interrogative context - i.e. an arena where questions (problems) requiring answers (attempted solutions) are asked (posed). On such a view it is perhaps natural that questions actually asked, and viewed to be relevant, should be seen as merely articulating questions already there for the subject, or his/her cognitive apparatus. This would allow those questions to be seen as "transparent". But in the case in hand this view comes patently unstuck, and the resultant confusion over perspectives is able to be highlighted.

To encapsulate in one sentence the involvement of an experience error throughout the conservation debate, and at the same time draw attention to its implications, I would offer the following. When an
experimenter arranges a bona fide C.T. for a subject — be he/she a child or adult, genius or simpleton — it can be assumed that, whatever the subject participates in, it is not a conservation task.

NOTES

1See Merleau-Ponty (1962, p.5) for a wider, though not unrelated usage of the term.

2If the levels alone were the critical factor in relation to the young child's characteristic "more in A" non-conserving response on a task like "P", then it should be a generalizable one. But it would probably be hard to find anyone, of any age, who thought that there was more water in a full, 40cm deep bucket than in a nearby swimming pool filled to a depth of 20cms — even when the difference in levels is pointed out.

3For them this suggested that the subject had disregarded the consequences of the transformation — see chapter 3. While agreeing, the significant aspect from the point of view of the general framework of my account is seen differently. It suggests that the subject is under the impression that the episode is still at the preliminary phase and is waiting for something to happen.

4If the S has not noticed those events and circumstances that must figure in an adequate justification, then such justification is impossible — whether or not it is requested.

5For convenience, we can exclude number C.Ts where counting strategies may muddy the picture.

6It is justifiable to disregard these events and circumstances, being and intended to be — as irrelevant as the traffic noise which, presumably, the subject is also completely justified in ignoring. This is not the same as saying that the subject is completely justified in explicitly believing that there is the same amount in A and C, rather than different amounts. To believe the latter assumes that those events and circumstances have not been disregarded, in which case the lack of sufficient information as to the relative amounts should have been acknowledged.
CHAPTER 9

IMPLICATIONS FOR CHILD PSYCHOLOGY

A. Introduction

The aim of child psychology is to understand children better. Consideration of the outcome of its empirical procedures should shed light on what the child does, or fails to do, and why. The empirical procedure examined here was the conservation task, and the main conclusion from Part I (chapters 1 - 3) was that there is an escalating interpretative muddle which may reflect an underlying descriptive problem. That from Part II (Chapters 4 - 7) was that, taking into account the nature of the C.T., developmental interpretations of the findings are inappropriate. Chapter 8 was concerned with identifying a common source of error and relating this to the identified interpretative problems.

Thus, rather than increasing our understanding of what children do, and fail to do, this enquiry seems at best only to be able to claim an improved understanding of what Piaget and the other concerned parties did and failed to do, in relation to the misguided conservation enterprise.

In this final chapter I will stress the positive implications for child psychology that derive from the negative-sounding conclusions, and from the route taken in coming to them. Before that, however, in the next two sections, I will round off my analysis of the conservation controversy. In the section that immediately follows I will set aside the debate about the conservation task, instead returning to the prior issue (see chapter 1) of the perceived significance of the notion of conservation - and the doubts that now arise.
B. Why a Conservation Controversy — and Why only Controversy?

Starting from the fact that conservation tasks exist, I have indicated that descriptive difficulties arise as a result of the application of an intellectualist approach. Specifically I pointed out:— (1) that the view of what, significantly, the C.T. consists of is challengeable — support here coming from the manifest interpretative difficulties that seem inevitably to arise, (2) that a better way of viewing what occurs is available. The evidence in this respect lay in the fact that adopting a deliberately non-intellectualist perspective allows the C.T. findings to be explained in a parsimonious, empirically-supportable way. This allowed the controversy to be resolved or bypassed — albeit at a severe price.

The questions that concern me now are the following:— (1) why should that same intellectualism have led, in the first place, to such significance being given to the notion of conservation, or conserving ability? (2) why might difficulties have been predicted to arise through permitting such a notion to occupy a central place in a fundamental approach to thought and understanding?

I have already indicated that, insofar as cognitive and cognitive developmental orientations tend to view the function of thought in terms of rational problem-solving, they are to be seen as intellectualist. The various authors considered have (1) adopted basically the same sort of cognitive framework as Piaget, and (2) like him, given prominence to the concept of conservation. To make my points I will therefore be content to focus on aspects of Piaget’s project, and the doubts that arise as a direct consequence of his intellectualism.

(a) Piaget’s intellectualist approach to achievement and progress

Piaget offers us what he would claim is, in the first place, a biological account of knowledge and mind. As Rotman (1977, pp. 18 — 19) says:

[For Piaget] the mind grows from a perpetual interaction with its environment, modifying itself, and consequently what it takes to be the world, until it achieves a balance, a state of temporary equilibrium with reality. Temporary, because unforeseen events, new patterns of cause and effect,
the infrangible complexities of the world, upset the balance forcing new and more complex equilibria to be achieved. The necessity for these equilibria, their occurrence and their form, is for Piaget part of the order of things to be found throughout nature. Indeed equilibrium, by being an intrinsic and constitutive property of organic and mental life provides the unifying principle of Piaget's biological vision. Its pursuit is a universal characteristic of life that lies as much behind the progress of evolution from amoeba to the higher primates as it does the stages of intellectual growth in children, and the evolution of the objective structures of thought found by mathematicians.

Piaget's [biological] theory, then, will try to explain the occurrence of thought in the universe; not only why it had of necessity to evolve but why its patterns have the biological forms he describes. It is an aim that credits the concept of evolution with vast explanatory powers.

But we need not be unduly diverted by Piaget's biological framework here, nor by the biological-sounding concepts such as equilibrium, assimilation and accommodation (see chapter 2). For, at the same time as presenting us with a biology of cognition, he gives us what is a very cognitive view of biology. The environment is viewed as that which poses problems; and the organism (or species') as that which is faced with, and equipped with an inherent tendency for solving them - i.e. making them go away. The interaction between environment and organism is thus seen in terms of the necessarily progressive consequences of a directed problem-solving process.

I will pass over the difficulties that arise from such a view - except to mention that, as for the other areas that his theory addresses, Piaget is unable to give an adequate account of (1) why one particular "solution" should have been preferred to another, and (2) the emergence of novelty (e.g. see Gruber and Vonèche, 1977, p. xxxv). The underlying problem here is the lack of clarity in making the distinction between, on the one hand, a criterion used for evaluating an achievement and, on the other, the posited process by means of which that achievement came about. As Aristotle, Erasmus Darwin and Goethe all pointed out (see Darwin, 1968, pp. 53-55), understanding the use of some morphological character is not to be conflated with an explanation of why and how it came into existence. Piaget tries to bridge the gap between function and origin with his theory. This involves an act of faith in a universal tendency of "living developments" for progressive functional adaptation, in company with other dubious notions (see
footnote 1). In the biological realm at least, the discredited Lamarckian component in his theory allows us to conclude that he fails to bridge it.

My concern here is with Piaget's view of cognitive achievement and the growth of knowledge. Rather than emphasizing its intended biological base, it is possible to characterize it in terms that owe more to Piaget's intellectualism. Piaget's concern is with useful knowledge. By this he means that which allows us to gain control over our environment. The focus is therefore on the mathematically-based knowledge of the natural sciences. An intellectualist approach will be one that tends to view the project of science predominantly in terms of a rational problem-solving exercise. And indeed Piaget does view the growth of knowledge, like biological adaptation and evolution, as the necessarily progressive process whereby ever-more adequate solutions to given problems come into existence.

As for any view of a problem-solving process, the criterion of a good solution is whether it makes the given problem disappear. In the biological realm a problem may be seen as that which poses some threat to the organism's (or species') continued survival. And the threat may be seen to be removed by, say, some anatomical or physiological change. In the realm of scientific thought and knowledge a problem is also that which requires to be removed. But the stuff we are dealing with comes in the form of articulated theories or explanations. The criterion of survival is not going to be of direct use to us here, and the question that therefore arises is: on what grounds do we decide that one explanation is better than another in relation to its problem-removing function? While there is nothing particularly startling about the answer that follows from this question, it can be put in a way that highlights the significance of the notion of conservation. In turn this will be helpful in, firstly, understanding a certain view of the process of scientific achievement - one shared by Piaget - and, secondly, in spelling out its limitations.
(b) Scientific knowledge and the significance of the notion of conservation

In chapter 1 I pointed out that the concept of conserving ability owes its significance in developmental psychology to the view that for someone to operate effectively in the world, the latter's stable and predictable features must be recognized. Things constantly happen and change, but they do not happen capriciously. There is always going to be a reason why something changes, just as there will always turn out to be a reason why something - if it is judged to do so - stays the same. In other words, everything is in principle able to be accounted for. We still need a criterion for judging the adequacy of an account; but the important point here is that science might be seen to operate on the same principle. The starting assumption in the natural sciences is that there is a reason for everything; and its business is explicitly to provide these reasons or answers and, in doing so, account for observed phenomena. To work as the shared project of an epistemic community, it needs and provides us with an apparently sound criterion for evaluating a given answer or explanation. It is the consideration of this criterion that might seem to put the notion of conservation, or something like it, at centre stage for investigations of knowledge.

The ideal goal of an explanation in the natural sciences is to fully account for the phenomenon in question: that is to account for it without loss or residue. As such, as mentioned in Chapter 1 (Section C), a "conservation principle" might be discerned to run through science. For an explanation to be judged as adequate, there should be nothing "lost" - that is, nothing associated with the original phenomenon left unexplained. Also, there should be no gain or "residue" - that is, nothing, itself unexplained, should be introduced with the offered account of the phenomenon in question. Within this framework, scientific knowledge of the phenomenon in question should therefore involve an account of it whereby the whole phenomenon - nothing more and nothing less - is able to be "re-constituted" from the explanation by working backwards through its conceptual steps, adhering to empirically-checkable facts and rationally-linked, non-contradictory, assertions about them. In the case of the account being deemed adequate, such a "reversing" operation, in effect, preserves intact - or "conserves" - the original phenomenon and allots it its place in an orderly and predictable reality.
I have related a view (Piaget's) of the intellectual ability individuals need to develop if they are to make sense of the world, to a view (shared by Piaget) of the goal and achievements of science in its project of explaining natural phenomena. The point here is that a case can be made that a similar notion of conservation is central to both views.

A criterion such as the above might be useful in gauging the level of understanding reached by an individual, or in evaluating a knowledge claim made by a branch of science with its latest theory. But whether it can tell us anything useful about the actual process by means of which that level of understanding or scientific theory came about is a different matter. On a "necessitarian progressivist" (Boden, 1979) evolutionary view of knowledge, such as Piaget's, it would seem to be the case that it should be able to.

There is no sleight of hand here. I have basically articulated the same viewpoint in two different ways. Piaget would have agreed that the goal of scientific knowledge is to solve problems, and its progress is marked by the emergence of better and better solutions. He would presumably also have agreed that its goal is to fully and rationally account for phenomena of scientific interest - that is, without loss or residue (see above). The question that arises now is: what could be wrong with, or missing from, a view that tries to characterize the achievements and growth of the natural sciences primarily in terms of a process based on the rationally-bound effort to make problems disappear and leave nothing unaccounted for?

(c) Critique

The criticism of Piaget here is that his intellectualist framework leads him to what must be a patently incomplete view of science and its achievements. As in other areas (see biological evolution, above) we find him unable to adequately account for the emergence of novelty. And the reason for making the above case that a "conservation principle" is to be seen as closely linked to his intellectualist (problem-solving) framework, is to suggest that such a deficiency would be one likely outcome if an unduly restrictive conservative component is built into his approach. In other words, while Piaget's view of science - as I have characterized it - might have some use as a retrospectively-applied
evaluative criterion, it does little to illuminate the actual process he sought to explain.

Once a scientific problem has been formulated and agreed about there should, in principle, be no difficulty in also agreeing about what could and what could not constitute an appropriate explanation. That which is seen as the most adequate explanation then depends on what the observable facts can be agreed to indicate. For Popper (1972) the scientific enterprise starts with a problem. If progress is to be made towards solving it in a scientific way, a rationally bound procedure should be followed. For him, to follow the scientific method involves the erecting and testing of falsifiable hypotheses (potentially "refutable conjectures") that have the capacity to account for (or help account for) the problematic phenomenon, removing the problematic aspect. Through rejecting those that are falsified, progress is guaranteed. And the culmination of the process - insofar as it is ever culminated - is the emergence of an hypothesis that is falsifiable but, despite efforts to do so, not falsified. This is to be taken, at least for the time being, as the explanation and the solution to the problem. This view of science - outlined in the sketchiest terms here - is intended to be not merely prescriptive, but also descriptive. That is, the history of science is seen as supporting such a view of the process underlying its progressive achievements.

There are many aspects of convergence, and some of divergence, in Popper's and Piaget's evolutionary approaches to the growth of scientific knowledge (see Rotman, 1977, p.56-60). The important point here is simply that Piaget's intellectualist framework commits him also to a view of the process as starting with a given problem and then following some rationally-bound procedure that guarantees progress towards an objective truth which solves/dissolves the given problem.

There is now a formidable body of opinion (see Kuhn, 1970, Lakatos, 1970, Feyerabend, 1978) to suggest that this sort of view of the scientific process both (a) fails to address what actually happened and happens - i.e. is inadequate as a description, and (b) does not work - i.e. is inadequate as a prescription. Kuhn is prepared to accept that Popper's account, or something like it, might characterize what he calls "normal science", and thus can account for small-scale progress. But if our concern is with the growth of scientific knowledge, then we have to attend to and try to provide a framework for understanding the dramatic
"revolutions" that punctuate the history of scientific thought. At this level, neither Popper's nor any other rationally bounded framework is up to the job assigned to it. To discern what directs its course, the view of science as a disciplined, rule-guided affair is largely replaced by one that emphasizes more "wordly" traits, such as vested interests and fashion. In other words, the sociology of science and social psychology of the scientist become relevant domains. Only with such input can we begin to approach an understanding of the paradigm shifts that change the course of science and mark the moments in history when significant progress is made.

When we examine what actually happens - what scientists do and why - the view that emerges is that of a much more messy business than the neat, logical picture painted by, say, Popper would have us suppose. As in the case of Piaget, the social context of thought and the acquisition of knowledge tends to be neglected by Popper. Even when the problem is identified and agreed about by those concerned (see below) things are not straightforward. For overt (as well as covert) biases play a part in what sort of hypotheses are advanced. More significantly, at least from the point of view of criticizing Popper, falsifying evidence has frequently not been enough to kill off hypotheses. A favoured hypothesis tends to be tenaciously clung to, in spite of the existence of evidence against it, the point being that such "tenacity" frequently turns out to have been justified. Bias also shows up in relation to the rejection of hypotheses. One that would appear to "wrap up" a domain of enquiry frequently arouses more antipathy than one that would keep it open and provide "jobs for the boys". Not unrelated to this, it is as often the case that theories have died through neglect rather than through refutation. This brings me to the occurrence of significant changes in perspective and to the implications of the notion of "paradigm shift" in the present context.

The main point I want to make here does not refer to what occurs after the problem of scientific interest has been identified, but to that which is involved in its identification. Piaget's starting point, like Popper's, tends to be a circumscribed problem. But, insofar as the notable advances in science have been marked by paradigm shifts, another way of saying this is that significant progress has been marked by shifts in the way the problem (that which is to be accounted for) is viewed.
Genetic approaches are not ruled out, but a switch of emphasis to a more basic genetic question is suggested. We can consider science's explanations as answers. And we can address the supposed necessarily progressive processes by means of which more adequate answers are to be seen as coming from less adequate answers (Piaget and Popper). But, if we are agreed that we are dealing with answers, then science's achievements must be seen in the first place as having their origin in questions. So, just as the key to understanding the C.T. results lay in attending to the questions, seeing how they could be and are observed to be taken, so the same sort of approach has an application for investigations of the processes underlying the scientific enterprise. The difference between the two approaches to genesis here reflects the difference between the achievement of providing a satisfactory answer and that of asking a good question, between the achievement of making a problem disappear and that of making a problem appear, between the achievement of accounting for something and that of seeing that there is something to be accounted for.

In relation to this last point, my approach to explaining the C.T. results might be seen as prompting, or reminding us, of a further insight into the process by means of which major scientific discoveries come about. This is again to be linked to changes in perspective and thus to potential paradigm shifts. But the point now is that it is not merely the case that there are always alternative ways of viewing a problem. It is always possible, if not always wise (see Chapter 6), to refrain from acknowledging that there is a problem at all.

The history of science is littered with notable examples of powerful theories being enabled to be put forward through the ignoring or overlooking of what might have seemed substantial problems. Thus Galileo produced a gravitational law that applied only to freely-falling bodies in spite of the fact that no one had or could have ever observed a freely-falling body, for which a vacuum is needed. The problem he was able to ignore was that, in effect, such motion did not exist. There are also copious instances, and not just in psychology, of scientists producing elegant solutions to problems that, in retrospect, were not problems at all and should have been ignored. Such examples draw attention to the fact that, as a solution is that which makes a problem go away, the best strategy for science is to disregard the problem if it can get away with it. More generally, however, we are reminded that problems do not come ready-made, but at
some point have to be created, noticed (or passed over) as suitable objects of reflection and scientific enquiry. And, once noticed, there are always alternative viewpoints as to what the object of such an enquiry should be.

If a low-flying F-111 jet is seen to pass across the sky, this may not present me with anything to account for, being such a frequent occurrence. But if I see it crash into the side of Ben Cruachan, this might be assumed to present the scientifically-minded observer with an obvious, ready-made problem: that of accounting for the fact that it crashed (pilot error, engine failure, turbulence, etc). However even here the problem might be legitimately framed in a very different way. Thus it is conceivable - e.g. if I was from the Kalahari Desert - that what I would see as needing to be accounted for was not the fact that a jet crashed, but the fact that a lump of metal weighing 10 tons could have stayed up in the air so long before hitting the ground. (It is likely that I would be able to take more scientific insights back to my fellow tribesmen through framing the problem in the latter way than through framing it in the former way.)

When we look back on the process of scientific discovery to try to understand it and evaluate its achievements, in any particular case we need to know what the problem was - what was to be explained away, accounted for. But having got this far, the fact that there was something to be accounted for points in two directions. On the one hand it points forward to the assumption of a stable, rule-governed and predictable world where there is a reason for everything. Correspondingly we are directed to the outcome, and to the process by means of which evermore complete, reason-bound accounts were able to be offered. On the other hand, the fact that there can be something to be accounted for points backward to a very different world: one that is ever-changing, where things arise un-accounted for. If not, nothing would ever surprise or puzzle us. Nothing would stand out as of interest, to be reflected on, or made a topic of scientific concern, thereby requiring to be accounted for.

When we address the means by which problems appeared in the form they did, we have to take account of contingencies and prejudices that influenced what was attended to, and the way it was attended to, prior to there being a formulated problem to set about solving. While this might not suggest the stuff of progress, it allows for the introduction
of novelty and leaves room for creative input. An initial emphasis on different ways of viewing the world is more appropriate here, and less potentially distorting, than a focus on better or worse ways of viewing things. But if our concern is with progress, it turns out not to be difficult to relate accepted major advances in scientific explanation to the emergence of novel perspectives and new ways of seeing problems.

If, in our concern to understand progress, we retrospectively identify or extract a set problem and examine the scientific enterprise for the steps that led to its disappearance as a problem, then severe limits are placed on the extent to which the emergence of novel perspectives can be taken into consideration. A presumed fixed problem refers to a frozen view of the world and tends to leave this creative side of the process in penumbra. More seriously, from its own point of view of shedding light on science and its achievements, such an approach also leaves out the question of why there was something, rather than something else, or nothing, to be accounted for also in deep shadow.

The best a rationally-bound, intellectualist approach like Piaget's can do, insofar as it recognizes problem-posing achievements, is turn them also into problem-solving achievements. But this will not do as, at some point, Piaget still has to acknowledge what any account in problem-solving terms presupposes: that a problem was somehow able to arise. Even if we insist on viewing thinking in problem-solving terms, it can be seen that the creative aspect implicated deserves more than the "post-script" treatment it often receives.

(d) Where does this leave the psychology of conservation?

The assumption that events in the natural world do not happen capriciously and can, in principle, be fully accounted for (that is, in accord with the principle that overall nothing can be lost or gained) has proved a useful one for the natural sciences. And we can assume that an analogous view, based on the assumption of a stable and predictable reality where nothing happens without there being a good reason for it, is also a useful one in every-day life. But the reservations I outlined above, in relation to Piaget's view of science, recur here.

A view of scientific achievement and progress in terms of a rationally-bound, directed problem-solving process, based on the drive
to leave less and less unaccounted for, is a distorting one. Correspondingly, the ability in a subject to know that when things occur - however puzzling - they do so for a reason and will be able to be accounted for, is no use at all on its own. And it is no use even if the subject also has the ability to set about rationally accounting for things. What is also needed is the presupposed knowledge that, in any particular concrete case, there is something to be accounted for; that there is a problem or puzzle. And to get as far as acknowledging that there is a puzzle is to have formed some view - however misguided - as to what is puzzling about it. If the subject (or his/her cognitive apparatus) does not at some level acknowledge that there is a problem, or that there is confusion, then the occasion does not arise for moving on to attempting to account for anything (irrespective of whether or not he/she has the ability to do so). And if the subject is not to be taken to already have some opinion of the sort of puzzle or confusion he/she is confronted with, then we could not understand how he/she would be able to set about resolving it.

We can see that Piaget's intellectualist framework - equating thoughts' accomplishments with problem-solving achievements - leads him to pass over a level of meaning-giving that is presupposed by it, but which it cannot adequately address. For, as we have seen, prior to cognitive processes taking over, there are, on Piaget's account, only appearances that are not to be trusted. But in this case the appearance that there is a conflict or problem to be addressed is also not to be trusted. If nothing is allowed to be certain at this level, then the subject is denied even the knowledge that he/she is totally confused. Piaget needs to be able to grant the subject at least this achievement, yet would seem to be prevented by his intellectualism from doing so.

We are certainly now able to see why the notion of conservation was so important to Piaget in his investigations of children's thinking. But we now also have an idea why difficulties should have arisen. Flavell (1963) and others (see chapter 1, section C) pointed out that conservation-formation is for Piaget the central achievement of the stage of concrete operations. With its appearance "the world is beginning to stand still and stay put". Something here might have struck a false note right at the start. For the world, fortunately, does not "stand still and stay put".
Meaning is forever moving on, and it is the nature of thought to constantly outstrip itself and its products. As Merleau-Ponty (1962, p.viii) pointed out, the sort of frozen view of the world that nourishes the natural sciences is a secondarily-derived one. We will never properly understand the process and achievements of science until we address what that view presupposes — that is, "re-awaken the basic experience of the world of which science is the second-order expression" (see Note²).

It would now seem that to suppose that the notion of conservation or invariance could be productive for any fundamental psychological approach to thinking is misguided. Understanding must be viewed, in the first place, in its dynamic aspect, and the means by which the ever-changing significance of the world we live and go about in is grasped. The vegetable I notice on the kitchen table is "a cabbage". On going out and coming back ten minutes later, the thing on the table, unchanged and conserved, inevitably has new meaning for me. If I do not specifically attend to it, it has become part of the background. If I notice it, it has at the very least the meaning of "the same-as-before cabbage", as opposed to that merely of "a cabbage". And if this change in meaning was not able to be registered, I could never even start to grasp the notion of conservation.

Seen in this light, the "psychology of conservation" starts to sound almost like a contradiction in terms. Whatever, we might already anticipate that the attempt to investigate conserving ability in isolation, and as a fundamental component of thinking, would lead to confusion. That it did so is clearly the case.

C. The Conservation Enterprise: Potential Source of Insights, or Runaway Pantomime Donkey?

Let us try to put such doubts aside. We can then suppose that something that could be called conserving ability plays an important role in the understanding of our environment as a stable and predictable arena where all that occurs can, in principle, be accounted for. What more natural then than to test for its presence or absence with a conservation task? What more natural than to present subjects, for whom
this ability is in doubt, with what might appear to be the simplest of
tasks in terms of what is to be accounted for?

Natural maybe. But it was a mistake all the same. And it is one
that led nowhere but into an apparently bottomless morass of confusion
and controversy. The aim of employing the simplest of concrete
reasoning tasks is a reasonable one. The materials (glass, water,
counters, plasticene, etc) can be assumed to be familiar to the young
child. And to understand what is involved would appear not to depend on
extensive experience of the world that it would be unfair to expect the
child to have. But in aiming at simplicity Piaget overshot the mark and
instead provided research psychology with a minimal situation: one whose
key feature is that nothing relevant actually happens.

As I have shown, and as the literature would suggest, it is a far
from simple task for a subject to know what to make of such a strange
and empty situation. And the controversy demonstrates that it was a far
from simple task to know what to make of what children make of it. As a
minimal situation it is comparable to a Rorschach ink-blot test. Or
rather it would be if, instead of being encouraged to look for
interesting possibilities in relation to what the shape suggests, the
subject is required to conclude and report that what he/she sees is
actually an ink-blot.

The conclusion drawn from my empirically-supported argument was
that the C.T. cannot be used to resolve, or help resolve, any
developmental issue. Examination of various approaches in the vast
literature only lent support to this conclusion. And since it was
developmental psychology that had the vested interest in the C.T. it is
now confirmed that it is this rather than "the absence of a simple
control [that] discredits the whole conservation enterprise" (Bryant,
1982a, p.5).

This "enterprise" was founded on the dispute over what the variety
of results obtained from the different versions showed. With hindsight
it can be seen that it was always going to be debatable what particular
versions do show that could be of developmental significance. Looking
back, it can now be seen that more attention should have been paid to
the question (see chapter 4): Given the nature of the task, what is it
that any version can show? And asking this question means interrogating
various assumptions about what the C.T essentially consists of and might
involve on the part of the subject to perform in the approved manner.
We do not normally invent a test and then look around for what it is a valid test of; and we are, presumably, more interested in children's abilities than in tests of those abilities. It is therefore perhaps understandable that the above, more fundamental question was neglected. Nevertheless, with only controversy being generated, the danger signals were there from the start. Perhaps the most salient warning, indeed, was that the C.T. could be so easily put to use to suit the aims of both "pessimists" and "optimists".

The Piagetians and their critics now appear to have employed a task that could be of no use to either camp, while trying to go in opposite directions on the central issue - the presence/absence of logical ability in the young child. In this respect the conservation enterprise might be likened to an out-of-control pantomime donkey.

Rather than, as they tend to claim, "merely standing on Piaget's shoulders", the optimists may be seen as the hind legs, with Piaget as the front legs. They were harnessed together by essentially the same intellectualist epistemological presuppositions and saddled with still too many of the same challengeable assumptions about the nature of the task they were all using. Consequently, the optimists seem to have been unable to get the clear view of the stage and scenery that was needed to pick their way through it and push their insights to a suitably radical conclusion.

The relevant point here is that they saw the need to view an experiment like the C.T. as consisting of more than just the intended problem about the manipulated array. Thus it was recognized to be also a social interaction. And it was assumed that this must make the task of interpreting the child's answers more complex. This point was mentioned in the previous chapter. To affirm it we need only refer to Chapter 3; or to Hundeide's reference (paper in preparation) - with a conservation task as his example - to the need to consider "metaproblems" and the "negotiation of congruence" in relation to "metacontracts for situational definitions". By contrast, Piaget's terms of reference start to appear almost straightforward, and we can be forgiven for wanting to get back to a simple world of solutions to posed problems, or, better still, to an episode where an answer is elicited by a question. And, to be fair to Hundeide, he does also refer to the "power of the questions". But he does so only to make a further supposedly developmental point, that is
to demonstrate how most children can easily be
detracted [sic] from their logical conviction to
adopt rather bizarre solutions completely out of
tune with their "common sense" ... provided these
solutions are in congruence with the social logic of
... the question.

I say "supposedly" because it is patently obvious that this applies also
to adults. They too can be induced to say/do the most bizarre, stupid,
dangerous and even self-destructive things, given appropriate
manipulation of the "social logic" - and sometimes, apparently, without it.

What I have shown is that, at least in the case of the C.T. (see
below for other examples), the task of interpretation could have been
simplified by their "social" insight. At least this would be so if they
had been able to set aside their preoccupation with developmental issues
and see past their intellectualist concern with the problem supposedly
posed by the "logical requirements" (Neilson et al, 1983 a) concerning
the manipulated array. They would then have been in a position to see
that to present the C.T. to anyone involves presenting a totally
impoverished and ambiguous interrogative social context - where the
only relevant thing that happens is that questions requiring answers are
asked. We do not need to worry about what the subject supposes the
questions refer to, or even what he/she supposes the words mean.

Picking up the reins of the metaphor again, those at the rear of
this pantomime donkey suffered the confusion that comes of restricted
vision. They are caught in the dilemma of wanting to bring out the
young child's abilities at every opportunity, but at the same time
needing the notion of failure if they were to be seen to be trying to
pursue a developmentally-relevant path. It would have helped if they
could have seen that viewing an observed response as some sort of
success (e.g. in terms of social cognition) is not the only alternative
if we are unhappy with a view that dismisses it out of hand as a
failure. Thus it can be, as here, that both terms are inappropriately
applied, whatever the elicited performance. And it may be in general
that, given that description should precede interpretation, a more
concerted attempt should be made to exclude such evaluative terms where,
if, and as long as possible. Whatever, instead of stumbling about in
the dark, their cause gradually suffocating in the heat of controversy,
they would have been better off shedding their role and seeking a more
useful one - one that could shed light on real developmental issues,
where terms like "success" and "failure" might turn out to be more appropriately applied.

By contrast, at least Piaget - at the front - had the benefit of vision. His vision was of his clear goal in the green, far-distant pastures where the explanation of knowledge itself was to be found. But closer to the hand (or hoof?), all that kept this rampaging animal together and drove it on was the illusory carrot held out temptingly just in front of it. This was the erroneous conviction that just round the corner (see Bryant, 1982 a, p.5), must lie the resolution of some important developmental issue.

D. Some Specific Implications for Cognitive Developmental Psychology: Corresponding Analyses of the Class-Inclusion and Tunnels Tasks

I would hope that aspects of the argument laid out in chapters 4 to 7 would apply beyond the conservation task. Thus we can look at other Piagetian tasks that involve the asking of questions requiring answers, to see to what extent the results obtained might yield to the same sort of impoverished interpretation. Insofar as they do, this will undermine developmental orientations, of whatever shade, towards them too.

As links are constantly made between the rationale employed here and the rationale of the argument relating to the C.T., the following also contains a summary of the reasoning underlying the main points of the chapters in Part II.

(1) The Class-Inclusion task

(1) Background

The literature on the various approaches to, and versions of, this task is reviewed by Winer (1980). To make my points, however, it is only necessary to mention the two versions already alluded to in chapter 4.

Piaget (1952) was concerned with the young child's understanding of the relation of a class of objects to its sub-classes. To test for its
presence/absence he used a range of tasks of which the following, designated "P1", is probably the best-known example.

A row of 6 toy cows, 4 of which are black and two of which are white, are placed on a table in front of the subject. The subject is first questioned to establish that he/she understands what the array consists of (Q1). The question now asked is: "Are there more black cows or more cows?" (Q1') When the subject has answered this question, the episode is terminated. Most children in the 4-6 years range are found to answer the "class-inclusion question" with the former of the two alternatives ("More black cows"). This was taken to indicate that they do not understand class-inclusion.

As in the case of the classical C.T., the objection we find is that this might represent an unduly "pessimistic" interpretation of the findings of a "loaded" experiment. Thus we have on our hands another competence vs performance issue. Donaldson (1978, p.43-44) spells this out:-

There is not much doubt about what a child does when he makes the standard type of error: ... he compares one sub-class with the other sub-class. His spontaneous remarks often make this quite clear. ... The question is why does he compare sub-class with sub-class? Is it because he cannot compare sub-class with class, as Piaget maintains. Or is it because he thinks this is what he is meant to do? Is there once more a failure of communication? (her emphasis)

McGarrigle, Grieve and Hughes (1978) suspected the latter and believed the source of error to be related to the perceptual salience of the classes under investigation. They therefore set out to present essentially the same task to children, but to do so in such a way that the reference to the total class was emphasised. To this end the cows were all laid on their sides, the episode only continuing when the child showed that he/she accepted and understood that all the cows referred to were to be taken as being asleep. The class-inclusion question now became: "Are there more black cows or more sleeping cows?" Many more children responded correctly on this modified version than on the standard version - about 50% as opposed to about 25%, when 6-year olds were tested. This was taken to support an "optimistic" conclusion with respect to this particular rational ability.
(ii) Critique

As for the conservation task, the goal of analysis here is:

(1) to attempt to view the class-inclusion (C.I) task solely as a question-and-answer situation, i.e. as an interrogative context;
(2) to try to interpret success/failure findings obtained from any version of the task as the equally legitimate and justifiable answers to a question whose form – independently of the content to which it refers – contains a crucial ambiguity; and thereby,
(3) to find a way of interrogating the assumption that the C.T. findings can shed light on any developmental issue.

Thus an interpretation is sought that embraces the above findings, as well others, and which excludes the need to consider (a) what actually happens / is present, and is referred to by the question – i.e. the actual events and circumstances concerning the array, and (b) anything that could be linked to an essential child-adult intellectual/psychological difference.

The aim from the start this time is to see if the developmental assumption can be challenged. We can thus stay with the original "developmental" question – why does the child fail on P1 while the adult succeeds on the same task (Question B') – as long as possible. In relation to the C.T. the need soon arose to take account of task "O" and address Question A – why does the child fail on "P" but succeed on "O" – before addressing Question B. As will be seen it turns out in the present case to be unnecessary to address the corresponding question (Question A'), referring to the standard and modified versions, until the end.

As for the strategy, the steps of the argument are precisely the same as those laid out in Chapters 4 to 7, i.e.:

(1) Instead of bothering about what particular versions do show, ask what any version, given its nature, can show. In relation to that "given nature", I point out that, like the C.T. and in a corresponding manner, the C.I. always takes place against a background of deception and lack of information.
(2) Get away from the preoccupation with explaining failure; instead initially address the question of what is involved in success.
(3) View the answers of subjects, before anything else, as coming from questions. As for the C.T., I therefore attempt to isolate the
questions from the rest of the context in order to consider their possible significance with respect to the answers elicited.

(4) Having identified the task as an interrogative context, ask what sort of interrogative context it is. What is sought here is a source of oddness or ambiguity in the form of the questioning that would make right and wrong answers equally justifiable (or equally unjustifiable), and equally impressive (or unimpressive) in terms of the intellectual accomplishment reflected by the elicited answer.

(5) Use the resulting interpretative framework to try to explain the results, and answer the "developmental" question, in non-developmental terms.

(1) Background of deception and lack of information?

As for the C.T., the C.I. task is an episode where it is arranged that nothing relevant to the array will actually happen. And, as in that case, all subjects suppose that something relevant will happen, and not only do not know what, but also do not know when. They do not know how long the episode will last. Basically, S cannot know why E is doing what he/she is doing and saying what he/she is saying, and is not in a position to know that E is not going to be doing and asking very much.

(2) What is involved in success?

Again the key strategy would be (and was, though in very cursory fashion) to turn to adults with a view to see what is involved in answering correctly and how they actually seem to do it. As Donaldson pointed out, what successful subjects do is answer the question they are asked (comparing sub-class with class), while failures answer a different question (comparing sub-class with sub-class). But, following the strategy used for the C.T., to get an idea of what may be involved we have to look beyond performance. Here, as there, we can show particular interest in the subject's response to the termination of the episode by the experimenter (E) after the successful ("including") response has been elicited in "P". The successful adult is not at all surprised when E says "Thank you, that's all". Indeed the subject and E frequently laugh together at the shared, completed joke: apparently it
is quite an event for them to be asked a question such as "Are there more black cows or more cows?" On the other hand the children who fail on "P" are surprised at the termination, apparently expecting there to be more to it than that.

What is interesting here is that such empirical evidence is the mirror image of the equivalent evidence from C.T. successes and failures. For the adult subject there is a point to being asked this question (Q2); whereas he/she tended to believe there was no point in being asked the "conservation question" (Q5) on the C.T., and was consequently surprised/disappointed/irritated (see Chapter 5) when it turned out to have marked the end of the episode. Again the suggestion is that something critical may be happening with the asking of the question. This leads to the next step.

(3) Can the question be isolated from the context?

As for the C.T., indeed it can. Referring to Chapter 6, with a C.T. the answer ("same") has to be assumed to have been given to Q1 - that is, if a C.T. is to take place. All the subject has to do to answer Q2 correctly is recognize this question as the same as the one that was asked at Q1 and remember what he/she answered. The subject does not need to have attended to the array, its transformation, etc. And not only does he/she not need to know what the noun "water" and the terms "more", "less" and "same" refer to here, but he/she does not even need to know what they mean.

Similarly here, no mention of the array, or of what the subject made of it, is actually necessary to explain his/her correct answer. Here also it neither matters what the supposedly crucial words of the question refer to, nor what they mean. Whatever the adjective "black" means, and whatever the noun "cow" means (assuming that it does not mean something that is necessarily black), there must be more cows than black cows.

On its own this does not get us any nearer to challenging the developmental assumption. For one thing, an understanding of necessity is implicated in the above view of how an "includer" could arrive at the right answer without attending to the array. And it could be that the failure on "P" (i.e. the young child) has not got this yet. We might,
however, allow the child to be "innocent until proved guilty"; and, as Donaldson tells us above, the question that we are directly concerned with, and must approach with an open mind, is why the child makes the wrong comparison and answers the wrong question.

What it does show is that the question is not dependent on the context to support it. As for the questions in the C.T., it does not just transparently refer to the context, but is part of the context. As an event in its own right, the properties of the question can be considered in relation to their potential influence on performance, and can be considered independently of the properties of the array referred to. So, having shown that the question E asks can be isolated from the context, what do we do with it?

(4) What sort of interrogative context are we dealing with: is there anything odd and ambiguous about the question?

Everyone, perhaps with the exception of Piaget, agrees that the question is an extremely odd one. As was pointed out (again in chapter 6) the "conservation question" (Q2) is also odd. There can be no doubt that it is odd to ask a multiple-choice question (Q1), get the approved answer and then ask it again when nothing relevant has happened - or only irrelevant things have happened - in between. But at least it is a form of questioning that one can imagine occurring outside the confines of the task - e.g. if E had not heard the subject's answer, had forgotten it, wanted to remind the subject of it - or if something relevant had actually changed. However, beyond the C.I. task it is hard to imagine circumstances in which one would conceive of asking, or could conceive of being asked, "Are there more black cows or more cows?"

But it is worse than odd. What appears to have been passed over is that it is grammatically incorrect because of an illegitimate use of the conjunction "or". The shorter Oxford English Dictionary tells us it is a "reduced form of the obsolete 'other'" and that it is a "particle co-ordinating two (or more) words, phrases, clauses between which there is an alternative". In other words it is a term used to denote a degree of mutual exclusion in whatever is being compared. At the very least, it must imply the possibility of some mutual exclusion whereas here there is no possibility of this (any black cow must also be a cow). Thus, in using the term "or" in this context, E would normally be taken to be
demonstrating either his ignorance in relation to the use of language, or that it is he/she who is the one who has not got a proper understanding of class-inclusion and is conducting him/herself irrationally. Whatever, as a question implying a degree of mutual exclusion of the compared classes, it would seem to have no place in a test of class-inclusion - unless the subject was intended to spot the mistake and correct for it. And this takes me to the next point.

Having identified the source of the strangeness, what concerns us here are the possible legitimate responses to an oddly out-of-place and grammatically incorrect question. If there are two or more justifiable responses, then there is ambiguity. Thus the findings might be able to be interpreted in terms of the ambiguity resulting from the form of the question.

The ambiguity resulting from the form of questioning used in the C.T. came about because the second of two identically-worded questions asked in sequence can either be taken as (a) a pointless repetition of the first, or (b) not so (and therefore having a point). The best answer is "I don't know", but failing this, taking it as a repetition will lead to the right answer, while equally justifiably not taking it so will make the wrong answer more likely. Likewise in the case of the C.I. task, looking to the form of the questioning rather than to its referential content, we now find we are dealing with an odd and ungrammatical question. Ostensibly, the most justifiable response would probably be something like: "Could you please repeat the question?"

Failing this, there are again two obvious, equally justifiable responses: either (a) take the oddness/incorrectness as intended, seek out a reason for E allowing such oddness to intrude, and do one's best to answer the question as it stands, or (b) take it as unintended and set about answering a "corrected version" of the question. The first sort of response will tend to lead to the right answer ("more cows"). On the other hand, the second sort of response will be liable to lead to the wrong answer ("more black cows"). This is so because the most obvious way of correcting the question is to insert "non-black" before the second mention of "cows".

The aim in chapters 4 to 6 was to put forward the most parsimonious, psychologically-relevant and empirically-supportable general explanation of how anyone succeeds or fails on a bona fide C.T. (involving a consideration of both "P" and "O"). This was given as the answer to Question C (see chapters 4 and 6) and was: Whoever the subject
(S) is, however old, etc., if that S justifiably hears Q2 as a pointless or memory-jogging repetition of Q1, he/she will give the "conserving" response. If S, equally justifiably, does not, he/she will be likely to give the non-conserving response. In the present case (where we are restricting ourselves to the consideration of standard versions until we need to look further afield), the equivalent general statement is: whoever the S is, however old, etc, if that S hears the oddness/incorrectness of Q2' as intended, he/she will be likely to give the right answer. If S, equally justifiably, takes the oddness/incorrectness as unintended, he/she will be likely to give the wrong answer.

Comparing any successful subject with any unsuccessful subject, it was noted that both initially have the expectation that something relevant/interesting will happen. They expect there to be a point. On being asked an odd/incorrect question, the one who takes its wording as intended is likely to take this peculiar event as the point - and is therefore likely not to expect a further point. The one who sees it as unintended, and therefore answers the "corrected version", would be likely to presume that this was not the point. After all it had already been established at Q1' that there were more black cows than non-black cows. He/she would therefore be still waiting for the point. As indicated above, the required correlations - between (a) success and no surprise at "termination", and (b) failure and surprise - are obtained.

However, of course there is also the correlation between age and success to be considered. Yet, considering the C.I. task solely as an interrogative context characterized merely by the oddness/incorrectness of the question, there is ostensibly no reason why it should serve as a divisive ploy, separating children from adults. To take an odd/incorrect question as it stands, or not to do so, are both legitimate responses that, we might suppose, lie within the general capacities of both children and adults. This would seem to rule out separating adult and child in terms of some ability here, on the present account. And if we are just seeking to identify a generalizable psychologically significant difference between child and adult in the different responses, we have to face the fact that on other occasions it may all be the opposite way round - that it is the adult who corrects (or mishears) some odd question, and the child who takes it as it stands and does his/her best to answer it.
As we might expect from considering the C.T. and other contexts, so much seems to depend on the circumstances that caution needs to be exercised in making generalisations. We may be faced with what seems to be the developmental question of why, in these circumstances (of task "P"), do adults respond one way, while children, equally justifiably, respond in the other way. But if I am right so far - i.e. that it is legitimate to investigate the performance results solely in terms of justifiable alternative responses to the oddness/incorrectness of the question and without reference to the array or the subject's understanding of it - then, if only in the interest of parsimony (see Chapter 7), it is right to try to offer a non-developmental answer to the "developmental" question (Question B').

(5) Can a non-developmental answer to the developmental question be put forward?

As in the case of the C.T., but even more clearly, the answer is yes. For the C.T., whether or not the young child has a rational conserving ability is beside the point because (a) such an ability is not required for success, and having it would not necessarily result in the right answer; (b) the findings of all bona fide C.Ts are most parsimoniously accounted for simply in terms of whether or not S hears Q₂ as a mere repetition of Q₁. Why adults should hear the conservation question (Q₂) as a mere repetition of the preliminary-phase question (Q₁) on "P", while on the same task the child does not, is answered by saying that it is inappropriate to talk of the "same task" here, and by suggesting why (chapter 7). The optimists' modified tasks (e.g. "O") are crucial in the argument enabling the link to be made between the cognitively unimpressive way the child appears to "succeed" to the similarly unimpressive manner of the adults' success on the classical task. From the resulting framework for understanding what is required for success, predictions as to what is needed to (1) turn child failure into success, and (2) turn adult success into failure were made and supported by evidence. Without altering (a) the array and its manipulation, and (b) the questions asked, all that was found to be needed was, respectively, something "extraneous" that makes it more likely that Q₂ would be heard as a repetition, and something to make this less likely.
In relation to the C.I. task, the question of whether or not it is an incomplete understanding of the relationship between sub-class and class on the part of the child that leads to his/her wrong answer likewise does not come into it. This is because what E asks, and S answers, has no place in a test of such an ability — indeed no place in the English (or French) language. But we still want to know why the adult does not do what the child does: that is, answer a more sensible, "corrected version" of the question.

While more sensible, the "corrected version" is rather a pointless question to ask and we might be tempted to suggest that adults, more than children, are less disposed to entertain the possibility of pointless questions — though the evidence from the classical C.T. might be taken to suggest the opposite. However, the more crucial observation here is that even as the "developmental" question is posed its legitimacy comes under suspicion. And the challenge takes a much more direct route than was the case for the C.T. We do not need to consider modified tasks, how easy it is to turn failure into success and elaborate manipulations of the context that induce adults to fail.

The fact is that adults frequently do give the wrong answer ("more black cows"), and they do so because they take the question intended to have been "Are there more black cows than non-black cows?" It is already relatively easy to get adults to "fail". Thus we can presume that any small manipulation (e.g. in the way the question is asked, in the sort of overall context that the C.I. task is embedded in, etc.) that would make it more likely that the subject will expect a question that makes sense, would markedly increase the "failure" rate.

To explain why more children fail than adults on the same task as it stands, we would then follow the strategy employed for the C.T. That is, it would be suggested that where there is a difference in performance in such an episode we are not justified in referring to the "same" task. We would therefore look for features that could contribute to making a difference to the sort of question-and-answer situation that the subject expects and encounters. In the end, we would want to be able to say: if it happens that adults respond one way and that children respond in another, then there is good reason to suppose that it happened that the relevant circumstances were not the same.

The conclusion as for the C.T. is that, through showing how the task episode can be viewed solely as an interrogative context and
pointing out the ambiguity that results from the form of questioning employed, the previously accepted view that Piaget's original findings have some developmental significance is able to be challenged. The work involved in challenging the developmental assumption in the case of the C.T. was much greater. This was because we can see why the view was taken that the findings from classical C.Ts must give rise to some developmental question. The purpose of the optimists' project to challenge Piaget's particular developmental interpretation, and the rationale behind introducing their modified tasks, followed from the fact that, through the terms of reference adopted, he had set himself up for contradiction. In the case of the C.I. task, what we notice is that Piaget did not really even set himself up for an alternative developmental interpretation. This is because, with hindsight, we are entitled to wonder why it was thought that there was a developmental issue at stake in the first place.

The "sleeping cows" modified task ("O'")

It was not necessary to consider modified versions to undermine the posited developmental significance of Piaget's findings. Further, the modified task in question did not lend itself to being incorporated in the argument to such an extent this time. In spite of a corresponding background of deception and lack of information, not only is the array noticeably different (unlike in the case of the C.T. versions, "P" and "O"), but the crucial question asked is also different from that asked on the standard version ("P'"'). We would not have been dealing here with a necessarily comparable interrogative context. However, the general strategy commits me to try to address the above framework to the question of why the young child happens to be less likely to answer "more black cows" to "Are there more black cows or more sleeping cows?" than on the standard question.

As an isolated question, the form of it this time is more acceptable on grammatical grounds. "Or" again implies a degree of mutual exclusion, and, with the extra qualifying adjective, this becomes a possibility. But given that E intends a comparison to be made between a sub-class and a class that totally includes it, and that it has been established at Qi' that all the cows considered shall be designated "sleeping cows", it is still an odd question to ask. In the light of
this, two sorts of explanation might be offered for an increased success-rate in children:-

(1) Something about the question makes the child react to it in the way the adult does to the question on the standard task - i.e. S is prompted to consider the question as it stands and remember (from the preliminary questioning) that all cows were supposed to be sleeping and that only some of them were black. This reaction would tend to lead to the right answer. As a question that requires some thought - on the part of anyone - the subject is entitled to presume that this was the point. (Therefore the investigator might expect to find evidence that the S is not surprised at the termination of the episode when the answer is elicited).

(2) The question is still only marginally legitimate, and the subject again may attempt to answer a "corrected version". To make it more legitimate the S would be justified in correcting it by substituting "non-sleeping" for "black". The answer to "Are there more non-sleeping cows, or more sleeping cows?" will be "more sleeping cows". While this question makes sense, it is a bit boring as this is obvious and had already been established. S is therefore entitled to presume that the point of the episode lies ahead. (Hence E might seek evidence that the S is surprised at the "termination"). However, there is another way to correct the question: i.e. by substituting "non-black" for "sleeping". The answer to "Are there more black cows or more non-black cows?" is "more black cows" - i.e. once more the wrong answer. (Again surprise at "termination" is to be expected.)

A cursory investigation of "reaction to termination" proved inconclusive. But, in passing, it is interesting to remember that McGarrigle et al reported that about 50% of children succeeded on the modified task. Such a result would be predicted by the second sort of account - that is if each of the ways of "correcting" the question was equally likely. Either way, while such an issue might prompt further research, from what I have said above, it would be unduly optimistic to hold out any great hopes that such research could shed light on any significant developmental question.

Post-Script
It might be feasible to make the analysis of the C.I. task even more convergent with that offered for the C.T. Thus the E may be seen as asking (or at least implying) the same sort of question at Q1' (when the array is set up and initially presented) and at Q2' (the test phase). Nothing relevant intervenes. Now a case could conceivably be made that the issue again boils down simply to whether the subject justifiably takes the second question as a repetition of a preliminary phase question, or not. If such an analysis could be sustained, the point of interest here is that the young child would need to be seen to be "at fault" on the standard C.I. task through taking the test question as a repetition; whereas on the classical C.T. he/she is - on my account - seen to be at fault for not doing so. Such an inverse interpretation would merely underline the pitfalls of continuing to try to make a developmental issue out of the child's "failure" on a C.T. such as "P".

(2) The Tunnels Task (See Chapter 4)

Having already discussed this Piagetian task in some detail in a previous chapter, my comments here will be brief. I likened the procedure employed by Piaget (1970, pp. 122-132) to that of a C.T. in that (1) an array is presented and questions asked about it, (2) an irrelevant transformation of the array occurs (the tunnels are placed in position), (3) the initial procedure is repeated and an identically-worded question (Did A go faster than B; or did B go faster than A; or did they go at the same speed?) is asked at the end of it, and (4) the episode is terminated when the subject's answer to the second question has been elicited. Again there is a comparable background of deception and lack of information. The subject is led to believe that something interesting and relevant will happen but does not know what or when. In this task the events and circumstances in both phases are less static and more inherently interesting. But the intervening transformation results in the crucial post-transformation phase - supposedly serving as a test of the child's understanding of movement and relative speed - consisting of an array where the movement that occurs is obscured from view.

Adopting the same framework, it should be possible to view the subject's answer to the test question as depending on whether or not he/she takes the second identically-worded question as a mere repetition
of the preliminary-phase first one. Again the subject's reaction to E's "termination" becomes relevant. And the fact that the same sort of correspondence tends to be observed that was seen in the case of the C.T. - linking success to surprise and failure to lack of surprise - prompted the following manipulation.

This was in line with the inadvertent consequence of the optimists' "incidental" strategy for turning failure into success (see chapter 3). Thus I set out to adopt a procedure that would be likely to result in S hearing the second question as an explicit request for repetition of the first, but that would keep the essential form of the task (as regards the array and what is asked) intact. I therefore:- (1) completed the initial phase (dolls race out in the open, one overtaking the other), asking the question and establishing that the overtaking doll went faster, (2) provided the occasion for the transformation phase by saying "Oh wait a minute! I forgot something. I was going to make it so they ran through tunnels (tunnels found under the table and put in place), like this (re-run the race). Now where were we? What were we saying? Oh yes: Did the boy-doll go faster, did the girl-doll go faster, or did they go at the same speed?"

Predictably there was an increased rate of successful responding. And predictably the successful 3-5 year olds seemed to answer in a way that suggested they were repeating the answer that they had already given. There was also evidence that they were surprised and/or dissatisfied at the termination of the episode, indicating that they had expected something more than "thank you, that's all."

E. Some General Implications for Cognitive Developmental Psychology

What comes out of some area of cognitive developmental research should be seen to be both "cognitive" and "developmental". My attempt to understand and resolve the controversy surrounding the conservation task findings culminated in the conclusion that the best (i.e. most parsimonious) empirically-supportable account of the mixed bag of C.T. results is not only a non-developmental but also a non-cognitive one. At each stage of this investigation I have tried to indicate that there
are general lessons to be drawn by cognitive developmental approaches. Here I will merely summarise some of the main points.

(1) The identification and interrogation of assumptions

When a domain of enquiry has run into profound difficulties assumptions need to be interrogated (see chapter 1). My investigation demonstrates the value of a strategy based on putting selected, taken-for-granted assumptions out of play. And there is good reason (see above) to suppose that such a strategy can be a fruitful one in other related areas.

(2) Problems at the level of interpretation may reflect difficulties or ambiguity at the level of description

There is a direct link to the first point here. Because of shared assumptions, there was in previous C.T. research little disagreement about what was to be interpreted, and complete agreement that there was an interesting developmental issue at stake. However, the fact that the outcome of that research effort was only a colossal interpretative muddle might have suggested that renewed attention to the descriptive level was called for. In this case, as I showed, a very different view of the sort of task that is arranged could have emerged, as well as a different way of viewing the subject’s conduct. A central concern was with the question: what can be agreed to be the facts to be interpreted? Thus, even if an alternative view emerges as to the relevant facts, there must be - and was - a concerted effort to ground the investigation on indisputable facts.

Other disputes over psychological interpretation may likewise be best addressed and resolved through prior attention to potential descriptive problems.

(3) The consequences of the intellectualist bias associated with cognitive approaches

Again there is an obvious link with the above points. For the assumptions that go with adopting the cognitive approach tend to lead to a certain way of viewing (a) what the experimental situation consists
of, and (b) what the subject does/does not do. That which is arranged to be made sense of and responded to (the manipulated array) is seen as posing a problem, and the response is seen as the product of some rational, problem-solving process directed at the array - or else seen in terms of an absence of, or breakdown in, such processes. In this case the minimum requirement of an experimental situation for a cognitive psychological approach would seem to be that: (1) we can agree about what is to be made sense of and responded to, (2) there is a problem, (3) it is a certain sort of problem, (4) applying the rational processing ability in question will be more likely to result in the approved answer than in some other answer, and (5) the terms "success" and "failure" are appropriate in describing different sorts of performance.

In the first place we may be able to dispute whether the arranged situation does need to constitute the intended problem. In the case considered it was found that the subject could get away with ignoring the problem; but also that, if he/she did not, it was a rationally unresolvable one due to a lack of information. The conclusion was that an intellectualist bias led researchers astray in relation to their view of what a C.T. consisted of and involved on the part of the subject - or rather what a "suitably controlled" C.T. could and should be ensured to consist of and involve. A different view of the C.T. was called for. But, due to the cognitive bias, there was an inadequate consideration given to alternative perspectives, and in particular the subject's "unprivileged" perspective. In line with this, there was an overestimation of the cognitive accomplishment needed to respond in the approved way.

The problem of the perspective disparity between the investigator and the investigated is a general and pervasive one in experimental psychology. And it is one that becomes particularly acute when processes or levels of the subject's understanding are being investigated. In relation to the prescriptive lesson that can be drawn here, an obvious way of overcoming it - though it is not always going to be so productive as in the present case - is to follow the rigorous descriptive guideline of sticking, as far as and as long as possible, with facts that everyone, including the subject, are able to agree about. This lowest common denominator approach predictably left us here with a very impoverished description, with mention of the array and the subject's possible understanding of it being eliminated. But it drew
attention (a) to the fact that we are dealing with an impoverished episode which may deserve such treatment, (b) to the fact that the questions asked are events that do not just refer to the context, but are a part of the context, and (c) to the fact that, if they are legitimately viewed as the crucial part of the context, such a description is rich enough. This is because it suggests a parsimonious way of explaining conserving and non-conserving responses without (1) invoking the presence/absence of a particular rational ability; hence the emergence of a non-cognitive account where description of the performance in terms of "success" and "failure" is inappropriate; and, ultimately, without (2) having to view the difference in performance between children and adults on the classical C.T. as posing a developmental question. The possibility of a non-developmental account of the findings suggests a further lesson.

(4) Developmental issues may turn out not to be developmental issues

An instance of supposedly interesting and supposedly developmentally-significant childish or child-like conduct is identified, or induced by an experimental manipulation. However, a developmental psychologist who is fully aware of what he/she is doing and why, should be able to say what prompted him/her to pick on this piece of conduct, and exactly why it was taken to be childish - therefore requiring a developmental interpretation. In other words, the developmental assumption in any particular case should allow itself to be interrogated. I suggested in Chapter 7 that, other things being equal, a non-developmental account (involving one sort of subject matter) is always to be preferred to a developmental one (involving the sub-division into two or more sorts).

In challenging the developmental assumption in relation to Piaget's classical C.T., the procedure that might be generalised to other contexts was as follows:- (1) to argue that, given the nature of the C.T. and the fact that it could be considered solely as a certain sort of question-and-answer situation, there was no obvious reason why it should serve as the divisive ploy - separating younger from older subjects - that Piaget intended it to be; (2) to show that both children and adults could be induced, merely by manipulating extraneous factors, to perform either way on intact bona fide versions of the task - those
factors always being able to be viewed to exert their influence via the ambiguity in the form of questioning; (3) to show that the fact that young children and adults performed differently on the same classical C.T. could be explained in terms that made it dubious whether reference to the "same" task here was useful or legitimate. Differences in what the experimenter arranges, in relation to those extraneous factors that can influence the way the form of questioning is taken, is again seen to be the crucial component - not the age of the subject.

(5) Cognitive-developmental tasks should make rational sense

When the purpose of an experiment is to test for the presence/absence of a rational ability in the subject, the least we might expect is that the conclusion we go away with is not that the experimenter's rationality is in doubt. Yet, in the case of the C.T., not only do we see the odd circumstance that E arranges an episode where the intended essence is that nothing actually happens, but he/she asks a question without providing the relevant information to answer it. Then, having received the approved answer, E asks the question again with only irrelevant things having occurred in between. As we have seen in the previous section, corresponding doubts arise in the case of the class-inclusion task. Here the E is supposedly testing for the presence/absence of the rational ability to understand the relationship between a class and one of its included sub-classes. But, in doing this, E is seen to use an odd and grammatically unsound question whose form conveys the mutual exclusion of whatever is to be compared. This therefore might suggest that it is E who has not got the ability in question. Lastly, in the case of the "tunnels task" we have the peculiar situation where E, purportedly investigating the subject's rational understanding of movement and speed, employs in the crucial phase of the experiment an array in which there is no visible movement.

These are just the sorts of task that have been praised - by Piaget's supporters and critics alike - for their "ingenuity". And they have been repeated ad nauseum with and without minor modifications. Presumably the term "ingenious" refers to effect of the original versions in producing the "failure" results Piaget's theory required. For we can suppose now that it does not refer to the extent to which they make rational sense and mirror real-world occasions for exercising a problem-solving intellect.
F. When is a Posited Developmental Issue to be ACCEPTED as a Developmental Issue?

I have argued that, in the case of the conservation controversy, what was supposed to be a developmental issue cannot in fact be taken to be one. And I have also suggested above that various other Piagetian tasks may also yield to the same sort of analysis and non-developmental interpretative framework. The suspicion might therefore arise that I am setting my criteria too high and that, as a consequence, we are liable to be left without any issues to investigate from a developmental perspective.

Where there is doubt I have claimed that it is up to the "developmentalist" to establish that a developmental account is required rather than up to me to prove that such an account is not required. The question is, therefore: under what circumstances is it to be accepted that there is no longer doubt and that the developmental claim is established? In concrete terms: when subjecting a set of findings (from, say, young children and adults) which shows a pattern of correlation with age to the "developmental challenge", at what point would I be forced to give up that challenge and accept that the child responds differently because it is a child, rather than for some other reason?

To argue that some issue, resulting from an experimental situation, is a developmental one, the first step is to establish that, say, children and adults do respond significantly differently on what there are at least some grounds for calling "the same" arranged situation. The next step calls for a focus on what is the most economical psychological account of what is involved in responding in the way the older subjects do (in the case of the C.T., as "successful conservers"). Having arrived at such a parsimonious account, a reason would then need to be offered as to why the younger subjects might not be expected to respond in that way (in the case of the C.T., why young children would be expected not to take Q₂ as a pointless or memory-jogging repetition of Q₁, or - if they did - why, for example, they might be expected not to remember how they answered Q₁).

Having offered a plausible reason, the final step in establishing that indeed there is a developmental question at stake would involve producing evidence that young children are not easily able to be induced to respond like adults - and vice versa - merely through manipulating
what can be agreed to be "extraneous", contingent circumstances. As long as what are agreed to be the essential features of the experimental situation (here, the array, its manipulation, the questions asked about it) are kept intact, the trend of the findings (separating the younger from the older subjects) should be clearly maintained.

In principle, there is no reason why such a procedure should not leave the developmentally-oriented researcher with a significant developmental issue to investigate if indeed one is implicated. What may happen, however, is that it is not the significant developmental issue he/she thought was at stake - or that it is not able to be seen as significant as he/she assumed. In the extreme - as in the case of the C.T. - we may have to decide that the criteria have not been adequately met to usefully continue to seek a developmental account, especially if an alternative non-developmental one has suggested itself in the process outlined above.

G. Concluding Remarks

There are no offered insights into the psychology of the child or processes of cognitive development in this work. This is the price to be paid for making the thrust of it a challenge to the assumption that to resolve the conservation controversy will be to resolve some developmental issue. If there are insights, they stem from the conclusions as to what is wrong with the conservation task in relation to the work it was intended to do, and the manner of arriving at those conclusions.

Criticism of Piaget's tasks is not new of course. However, such criticisms tend to arise from dissatisfaction with the implications of his theory of cognitive development and are diluted by the inevitable praise for his experimental ingenuity in designing tasks that achieve the supposedly interesting results his theory needs. Thus, without challenging Piaget's theoretical coherence, we come across the understandable objection to the unduly pessimistic view of the young child's rational abilities required by the theory. Or we come across the not-unrelated objection that the framework and terms of reference of his theory, while again coherent, depended on an unduly narrow view of thought and understanding which, in particular, tends to bypass the social component and hence the development of aspects of communication
and social cognition. The corresponding criticisms of an experiment like the conservation task tended, therefore, to emphasize the facts that it was "loaded" in favour of failure and that the results obtained could not be assumed to be generalizable to other, more natural social contexts.

In chapter 2 I suggested that, as there is now sufficient reason to doubt the coherence of Piaget's theory, there is also reason to suppose that the problems with the keystone conservation task go deeper than the quibbles over the valency and generalisability of interpretations. If so, the most powerful criticism of his empirical project would be that which does not require a shift in theoretical orientation, but demonstrates that, even within Piaget's own framework and terms of reference, his tasks cannot show what he needs them to show. My objections are thus directed at the very way the task and elicited responses are viewed and described and is based on facts about task and performance that are either obvious or, if not, easily checked. As the culmination of this criticism is the conclusion that the latter do not lend themselves to any sort of developmental interpretation, it is as much a criticism of the bulk of Piaget's critics in this area of research, as of Piaget.

For insights into thought and intelligence to emerge, Piaget needed a task that reflected his view of the world. Thus he needed a procedure that posed a problem, and that provided enough information for the subject to solve it and arrive at a single necessarily true answer. And he sought to design an episode whose essential feature was the potentially misleading appearance of the manipulated array.

Instead he gave us, in the conservation task, an episode which (1) need pose no problem, (2) if it does, it contains too many interesting possibilities and insufficient information to allow it to be resolvable by rational means alone, and (3) has as its essential feature the troublesome fact that nothing relevant actually happens - apart from two identically-worded questions being asked in sequence.

What such conclusions about the nature of the task seem to highlight is therefore not that the world is a rationally-ordered place where there is always a reason for everything, and where the events and circumstances that pose problems can be accounted for without loss or residue. Rather it is that (1) problems have to be seen to be posed (made to appear) before the possibility of reasons and solutions (that make them disappear) can arise; (2) there are always alternative ways of
looking at things, and interesting possibilities able to be discovered; and (3) there is rarely enough information to arrive at necessary truths about our concrete surroundings.

In making such points, a flaw inherent in all tests of concrete reasoning ability may be indicated. The domain of necessary truths tends to be an hypothetical one: we can usually find, or think up, counter-examples to assertions seeking to generalise from observations of actual events and circumstances. *

But the main point here is that, if we can look beyond a problem-solving characterization of the function of thought - and beyond the question of presence/absence of rational abilities - to thought’s creative aspect, there may after all be insights to be gleaned for child psychology from the consideration of the conservation task and the controversy that surrounds it. Accordingly, my negative-sounding conclusions should not be taken merely as an object lesson in how not to do child psychology. Alongside a concern over the question of the development of rational abilities, various other important questions suggest themselves. These would not only have consequences in relation to what we look for in, and how we go about, our psychological investigations of the child. For instance, we can also expect there to be implications with respect to what and how we teach the child.

In terms of these educational implications, the promise that was widely agreed to have been held out by the Piagetian approach is now widely agreed (e.g. Boyle, 1983) to have remained largely unfulfilled. Kestenbaum (1974), following Dewey, argues as I do (following Merleau-Ponty) that intellectualist approaches, viewing understanding and knowledge as the product of mediating problem-solving operations, leave an unacceptable lacuna. By viewing the world essentially as problem and the subject essentially as problem-solver, I have pointed out that problem-posing achievements tend to be overlooked. More basically, reference to mediating operations presupposes that something is mediated, and should direct the investigator’s attention to the level of experience where what has to be taken as unmediated meaning arises. Criticizing Piaget along these lines, Kestenbaum goes on:—

To argue as Piaget does, that lived meaning is incidental to intellectual activity, that it can be precipitated out, leaving only the subjects ‘operations’ as the basis of intelligence, is to promote the most emaciated conception of intelligence and will certainly give encouragement to those who see education exclusively as the
transmission and mastery of organized bodies of knowledge ... Piaget's rejection of lived experience is not only detrimental to the soundness of his cognitive theory, it undermines the very conceptual basis upon which any argument for the continuity of cognitive and aesthetic experience can be built. Because it lacks a comprehensive theory of experience, Piaget's genetic epistemology is appealing to those who think in terms of [this sort of] mastery, ... behavioural objectives, performance contracting, etc. Individual differences in developmental growth are used by these people simply to decide what can reasonably be expected of a student in mastering some set of behavioural objectives. Rather, then, than being a great liberating force in education, it is not inconceivable that Piaget, due to his truncated vision of experience, could provide considerable support for the more prosaic mentalities at work in education.

This is certainly not the intention of Piaget's work - linked as it is to the view that the child learns through his own actions and by a process of self-directed discovery. But his intellectualism leaves him with a correspondingly "truncated vision" of the goals of action (solving problems, resolving conflicts), and unable to adequately address the required creative component of such a process of discovery.

Many of the points made by Kestenbaum above coincide with those I made (1984) in a contribution to a debate on the philosophy of education in "The Guardian" (extract quoted in The Open University publication "Teaching for Diversity" [1987, pp.83-84] ). In it I referred to the dangers of promoting an attitude of detachment in the learner in relation to the world to be learnt about. I thus urged caution in embracing a view of education merely as the mastery and "accumulation of specialized knowledge and hard facts", saying:

Clearly we should decide that it is more than a resource, something to be accumulated - one tends here to think of a vacuum-cleaner model of education - to gain control over nature, and advantage over others.

Such a warning is especially topical in 1988, the year of education reform, and closely resonates with Kestenbaum's concluding remarks:

When education is increasingly expected to deliver products with measurable attainments, it is not surprising that the stages, schemes, criteria, and definitions of Piaget's theory should strike a responsive note.
The conservation task, from the interest it aroused, evidently also "struck a responsive note". What seems clear now is that it did so for the wrong reasons. But, viewed as the ailing product of Piaget's "non-comprehensive" intellectualist theory, it inadvertently highlights things that children (and adults) need to know and/or be taught. Thus the fact that the questions asked can be viewed as the crucial feature of the context reminds us of the importance of teaching children that asking the right questions is as important as knowing how to answer them. The fact that the intended problem can be disregarded reminds us that children should be taught that problems need to be formulated (made to appear) before they become problems to be solved (made to disappear); but that problems are sometimes best ignored. The fact that, while oddly - nothing relevant actually happens, something interesting could have happened reminds us that children might, though probably do not, need to be taught that there are always unexpected interesting possibilities, and alternative ways of viewing "the facts".

Finally, the fact that the subject is given insufficient information to work out the correct answer and - more generally - what the experimenter is up to, reminds us that the child should be taught that there is nothing to be ashamed of in saying "I don't know". Given the uncertain world we live in, to know that one does not know can represent the most potent wisdom.

Notes

1 Holding to a belief in the inheritance of acquired characters, Piaget would see continuity between processes occurring at the levels of the individual and species. Ostensibly this simplifies matters.

2 Explain: literally "to make flat". Merleau-Ponty frequently points out that if we make the goal of scientific understanding to explain away a phenomenon, there is a price to be paid. That is, we risk losing contact with the phenomenon that prompted the investigation in the first place, and provided the rationale for undertaking it. Taking an extreme example, we might seek to explain the experience of "falling in love" in terms of brain transmitter levels. Problems arise when the explanation of the phenomenon is substituted for the phenomenon: in this case, when falling in love comes to be viewed as, say, merely a certain dopamine: endogenous opiate balance. There is more here than simply a criticism of reductionist psychology (see Merleau-Ponty, 1962, 1965).
Piaget's original tasks of course involved French-speaking children and the French language. I am assuming that the use of the conjunction "ou" is no less odd and grammatically dubious in the original version of the question.

Smith (1982, p.112), discussing the child's understanding of necessity, inadvertently provides us with an example. She says that "a young child who sees that his square jigsaw has four corner pieces might fail to see that his jigsaw [necessarily] has to have four such pieces if it is to be a square one". This is simply not so. If one corner piece is divided into two by being cut diagonally from the junction of its right-angle, the square jigsaw will have only three corner pieces.
CHAPTER SUMMARIES

In CHAPTER 1 I provide some background as to the perceived importance of the notion of conservation and of the conservation task. I justify doing yet more research into what is already the most repeated experiment in the history of psychology, and anticipate the overall strategy of the investigation: to identify taken-for-granted assumptions and subject them to interrogation. Three which spring to mind are:—

(a) that the C.T. can be viewed as a test of reasoning ability; (b) that the non-conserving response is psychologically interesting; and (c) that the mixed bag of findings have some developmental significance.

I select representative versions of the C.T — "P" (Piaget's classical C.T.) and "O" (a modified version, after Light et al, 1979). And in CHAPTER 2 I pose the two central questions concerning the findings obtained:—Question A: why does the young child fail on "P" but succeed on "O"? Question B: why does the young child fail on "P" while the adult succeeds on the same task. While the overriding aim is to supply parsimonious answers to them, I raise the possibility that they are illegitimately posed. In other words I suggest that, prior to any problem of interpretation emerging, there may be a problem of description. Some reasons for doubts, even before any evidence is examined, are mentioned. Referring to work of Merleau-Ponty (see also Appendix II), I anticipate likely descriptive errors concerning the view taken of (a) the task and (b) the subject's conduct. I return to this theme in chapter 8.

With lack of clarity at the descriptive level, the outcome predicted would be confusion at the level of interpretation. In CHAPTER 3 I review some of the literature concerning the variety of findings obtained from children on classical and modified C.Ts. That there is an interpretative muddle is confirmed. In the attempt to make the best of the confusion, I suggest that the main thing of value to come from the "optimists" false negative charge against Piaget was the design of tasks (e.g. "O") on which the young child now "conserves" and
the accompanying false positive criticism. With this switch of focus to success on modified C.T.s., the important evidence was:– (1) subjects seem to disregard both the transforming event and its consequences (Neilson et al, 1983 a); (2) subjects are left at the end waiting for the game to start (Bovet et al 1981); (3) subjects appear at Q₂ (the conservation question) “merely” to repeat the answer already given at the establishing equivalence phase (Dockrell et al, 1980); and (4) subjects commonly fail to accompany their right answers with adequate justifications (Neilson et al, 1983 a). My conclusions as to the significance of each of these four points, in the light of the identified source of errors, is given below (see chapter 8 summary).

The optimists rightly pointed out that Piaget's classical version (e.g. "P") is a bad reasoning task. The dilemma they face stems from the fact that, spotting that Piaget sets himself up for contradiction over the question of the young child's competence to conserve, they intend the modified task to be seen as having essentially the same form as "P". Either they are wrong – in which case the positive results on "O" are unlikely to shed any light on why the child fails on "P" (Piaget's view). On the other hand, if they are right, they have to convince us that "O" is not also a bad reasoning task, and for the same reasons – irrespective of the different result obtained. This is the line I adopt in CHAPTER 4, switching attention from claims about what particular versions of the C.T. do show to the question of what, given its essential nature, any C.T. can show.

This brings the issue of description back into focus, features shared by all C.Ts being identified as; (a) nothing relevant happens; (b) subjects are led to believe at the start that something will happen; and (c) there is not enough information for subjects to know (at Q₂) whether something relevant has happened or not. Since both right and wrong answers on both "P" and "O" are compatible with both presence and absence of reasoning ability, this suggests that the C.T. is not just a bad test of reasoning ability, but the worst possible. However, we are left with the question: what does determine how the subject performs?

All C.Ts take place against a background of deception and lack of information. The assertion that is put to the test in CHAPTER 5 is that the "opposite" (conserving/non-conserving) responses may be considered the equally legitimate products of the same deception and same lack of information.
While it is relatively easy to see how failure (e.g. on "P") could be viewed as the product of such a combination of deception and lack of information, more puzzling - and therefore more interesting - is how success (e.g. on "O") might be accounted for on the same lines. This chapter therefore marks the second shift of emphasis: away from the manifest preoccupation with explaining supposedly interesting failure, towards gaining an understanding of what is involved in so-called success. Empirical evidence, most importantly from conservers (children on "O"; adults on "P"), is considered from the point of view of identifying an aspect that could be tripping all these subjects into the right answer and would therefore, as for non-conservers, involve a mistaken belief.

To this end a further item of lacking information needs to be posited, and is identified in the fact that the subject in a C.T. does not know how long the episode will last. The hypothesis now is that the fact that all subjects are led to incorrectly believe that something will happen before the end of the episode is crucial. It deceives those who correctly take the second question (Qz) to mark the end of the episode into wrongly supposing that something relevant has happened - and thereby into the wrong answer. And it deceives those who incorrectly take Qz not to mark the end of the episode into wrongly supposing that something significant is about to happen - and thereby into the right answer.

The attraction of such an explanatory framework was that, firstly, it would support the view that emerged in the previous chapter that describing C.T. performance in terms of "success" and "failure" may be inappropriate. Secondly, by switching our attention away from the problem supposedly posed by the array, it would allow that which the conservation controversy has been seen to revolve around to be bypassed.

From the evidence, the required correlations were identified, i.e. between (a) "success" and being left at the end waiting for something to happen in both children and adults, and (b) "failure" and not being left waiting for something to happen (just children at the moment). However, in the end this sort of account was found to be, on its own, both implausible and psychologically illegitimate.

Having failed in the attempt to view the right answer merely as a by-product of the subject's incorrect belief on the "temporal problem", and thus by-pass the need to invoke a Piagetian-based framework to
interpret C.T. performance, the strategy in CHAPTER 6 moves to the other extreme. Hence the aim now is to exploit that cognitive-genetic-structuralist framework, still with the focus on successful conservers. The subject can and - in the case of success - seems to disregard the intended problem concerning the transformation of the array, and the investigator may follow the subject's example. On the other hand, the right answer - like any answer - has its origin in a question; and neither subject nor investigator can ignore the questions asked. Thus I arrive at the conclusion that the C.T. may be considered, for the purposes of the investigation, solely as a question-and-answer situation. It is an ambiguous interrogative context where all the subject can be sure of is that two identically-worded questions are asked in sequence. The sought-for general explanatory framework that follows from this is that: a subject will "succeed" if Q2 is justifiably taken as a mere repetition of Q1, and will be likely to "fail" if Q2 is - equally justifiably - not taken in this way. Returning to the specific question of the child's different performance on different versions, I point out that, given this parsimonious (indeed impoverished) framework, all that needs to be identified are "extraneous" factors. By this I mean factors, nothing to do with the array, that would affect the likelihood of Q2 being taken as a mere repetition of Q1.

Question A is now able to be answered by pointing out that in "O" there is an additional question which is also an explicit request for repetition ("where were we?" etc. followed by Q2), whereas in "P" there is not.

It is clear that, if my analysis is well-founded, Question B - the "developmental question" - should be able to be answered on the same lines as Question A, above. That is, the adult may be viewed to succeed on "P" solely by virtue of taking Q2 as a pointless repetition of the preliminary phase question, Q1; and the child may be viewed to fail on "P" through, equally justifiably, not taking Q2 in this way. There is still a difference to be addressed, even if we are no longer able to view it as a difference in ability. But the desirability of applying such a parsimonious, empirically-supported explanatory framework evidently brings pressure to bear on the assumption that the C.T. findings have some developmental significance. For some reason would need to be advanced why young children would be less likely than adults to take the second of two identically-worded questions as a mere
repetition. Apart from pointing out the psychological obscurity of such a trait, two complementary strategies present themselves in CHAPTER 7 for challenging the "developmental assumption".

Firstly, allowing that we are dealing with the same bona fide C.T., the robustness of the adult findings may be questioned. To this end I show that adults can easily be induced to answer wrongly on an intact version of "P" - and can be viewed to fail in the same way that the child fails. This evidence complements that concerning the child on "0" - he/she being seen to succeed in the same way that the adult succeeds on "P" (see chapters 5 and 6). Secondly, remembering the descriptive problem, given that there does normally happen to be a difference in the performance of the child and that of the adult, how justified are we in referring to the same task? To this end I note that what E does in presenting "P" to the child is not what he/she does (or would do) in presenting it to the adult; and the discrepancy is now able to be viewed as crucial.

It can be seen that, somewhat paradoxically, (a) answering Question A might have seemed to depend on pinpointing a difference between versions of the C.T., yet crucially involved highlighting key similarities, while (b) answering Question B might have seemed to depend on pinpointing a difference between subjects, yet involved highlighting the fact that the task is not, as presumably would be intended, kept the same across subjects.

In this way I argue that the representative young child is able to be seen as giving the wrong answer not, in the first place, because he/she is a child, but for some other reason. Suggesting on the grounds of parsimony that - other things being equal - a non-developmental account is alway to be preferred to a developmental one, I conclude that the C.T. is eminently unsuited to help resolve any developmental issue for psychology.

It emerges from the above analysis (chapters 4 to 7) that some basic mistakes have been made by all parties to the controversy. The aim of CHAPTER 8 is to locate the shared source of error at the descriptive level and identify aspects of the interpretative muddle as a product of it. The biases mentioned in chapter 2, and to which Merleau-Ponty's critique of intellectualist approaches alert us, are anticipated to lead to a confusion of points of view and hence to an illegitimate oscillation on the part of the investigator between his/her own privileged perspective of what (i.e. the problem) he/she has arranged,
and the point of view of the subject. I call this the "experience error" and demonstrate the value in making a disciplined effort to avoid it here.

Piaget’s biases and resultant confusion are confirmed to have led to errors of both commission and omission. Thus he assumed that the transformed array must pose, through its appearance, an intellectual problem, when it need not. If it does, it is a rationally insoluble one - given the lack of information. One thing he omitted to take sufficient account of was that the C.T. episode consists of more than the manipulation of an array. It is a social transaction and, as such, an odd and deceptive one.

The optimists acknowledged the latter point. But they failed to see that (1) their modified tasks were no better, and that (2) given the sort of social transaction it was, this could simplify the task of interpreting performance. Because of their cognitive orientation, the social aspect was seen to provide an extra, complicating dimension. The classical C.T. is still viewed incorrectly as a test of reasoning ability - albeit an unfair one. To make a task like "P" fairer for them in general involved making the episode "more meaningful" for the child. This merely begs the central question of what is the meaning of the C.T. for the subject. Specifically, it involved "emphasizing that the transformation was incidental" by pretending that it had not been originally intended (viz. task "O"). Apart from overlooking the additional element of deception, their confusion of perspectives is manifest in the fact that such an aim is, psychologically, a contradiction in terms. What, from E's point of view, is "emphasised as incidental" is exactly that which, from the subject's point of view, is de-emphasized to the point where it and its consequences are liable to be ignored or even not noticed. The "false positive" critics recognized the above point and thereby explicitly acknowledged the possibility of a non-cognitive explanation of elicited "conserving" responses. Also, to their credit, it was acknowledged that a broader view of the subject's conduct - i.e. beyond mere performance - was required if the answers of subjects were to be explained in a psychologically adequate way. But they failed to see that the evidence they quoted, on tasks where the transformation is intended to appear incidental or accidental, may tell us more about the flaws in the design of any C.T. than about the flaws in the modified C.T. and in the child's intellect.
Referring back to the four key facts they mentioned in relation to modified C.Ts. like "C" (see chapter 3 summary): instead of disputing them, I augmented them with corresponding evidence from adults on "P", and took them to indicate or confirm, respectively that for any C.T.:-

(1) the essence of the task being, strangely, that nothing relevant happens, any subject is best off ignoring the events concerning the array and their consequences; (2) subjects lack information as to what will happen and when, but all are deceived into wrongly supposing that something will happen; (3) the episode is able to be viewed solely as an ambiguous interrogative context where two identically-worded questions are asked in sequence, the second being able to be taken as a mere repetition of the first, or - equally justifiably - not so; and (4) right answers are no more justifiable than wrong answers, neither are completely justifiable, and it is inappropriate in such a situation to ask for justifications.

Amongst other things, by making such points the basis of the false negative - false positive developmental controversy is seen to finally collapse. The very basis of the distinction between "negatives" and "positives" being now unclear, the distinction between true and false negatives and positives itself is seen to be a false one. The wrong answer is, potentially, always too impressive to be dismissed as "failure"; and the right answer is, potentially, always totally unimpressive.

The message that we are left with might be taken as: if we want to test for the presence/absence of a rational conserving ability in subjects then we need something other than a conservation task. But at the start of the CHAPTER 9 I suggest that the notion of conservation was never likely to be a useful conceptual tool for a fundamental investigation of thinking and understanding.

I consider the implications of my analysis for approaches in child psychology. Thus I make some general points relating to the need to be aware of and, where necessary, to challenge taken-for-granted assumptions. More specifically, I show that my empirically-supported argument can be usefully applied to other Piagetian cognitive tasks. Examination of the case of the class-inclusion task provides the opportunity to summarise my strategy, as well as demonstrating once more that sometimes what we take to be a developmental issue may not be a developmental issue at all.
APPEIX DIA

Classical and Modified Conservation Tasks with Young Children

The main tasks used were (a) the classical version, "P" (after Piaget, 1952), and (b) the modified version, "O" (after Light et al, 1979) of the volume (or amount, when powder was used) C.T. The procedure I used for carrying out these tasks with 3 - 5 year old children taken from the Edinburgh University Psychology Department kindergarten, is described and illustrated in Chapter 1, pp. 18 - 21.

Between 1982 and 1986, performance (conserving vs non-conserving) results were obtained for a total of 59 children tested on either "P" or "O". These were as follows:-

<table>
<thead>
<tr>
<th>Task</th>
<th>Conservers</th>
<th>Non-Conservers</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>&quot;P&quot;</td>
<td>11</td>
<td>19</td>
<td>30</td>
</tr>
<tr>
<td>&quot;O&quot;</td>
<td>17</td>
<td>12</td>
<td>29</td>
</tr>
<tr>
<td>Total</td>
<td>28</td>
<td>31</td>
<td>59</td>
</tr>
</tbody>
</table>

Initially, 40 subjects had been tested on either "P" or "O", the results (a subset of the above) being:-

<table>
<thead>
<tr>
<th>Task</th>
<th>Conservers</th>
<th>Non-Conservers</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>&quot;P&quot;</td>
<td>7</td>
<td>14</td>
<td>21</td>
</tr>
<tr>
<td>&quot;C&quot;</td>
<td>11</td>
<td>8</td>
<td>19</td>
</tr>
<tr>
<td>Total</td>
<td>18</td>
<td>22</td>
<td>40</td>
</tr>
</tbody>
</table>

In addition, 18 of these subjects were tested on classical (after Piaget, 1952) or modified number C.Ts. The modified condition corresponded to the "Naughty Teddy" version of McGarrigle and Donaldson (1975), the difference being that a puppet puppy was used instead of a teddy bear. (For descriptions of these tasks and the differences between them, see Chapter 3, pp. 50, 53.) The subjects given the classical task had previously been given the modified procedure, "O", and those given the "naughty puppy" task had previously been given the classical version, "P". The results were in line with the others:
Conservers | Non-Conservers | Total
--- | --- | ---
Classical number C.T. | 3 | 5 | 8
Naughty puppy modification | 6 | 4 | 10
Total | 9 | 9 | 18

The 1st Empirical Note (see p. 98 et seq.) drew attention not so much to differences between versions of the C.T. and potentially significant factors that might affect the relative probabilities of success/failure, but rather to the points:

(a) that the non-conserving response - whenever it occurs - may be potentially too impressive to be dismissed as "failure".

(b) that the conserving response - whenever it occurs - is potentially totally unimpressive.

(c) the task - whichever version is employed - is an odd one in that its central feature is that nothing relevant to what is asked happens, while information having a bearing on this fact is either missing or liable to be misleading.

With such points, the focus of the investigation shifted from the preoccupation with explaining supposedly interesting failure, to an attempt to characterize what is (and is not) involved in "success". Corresponding to this shift, I sought to implicate the feature of temporal ambiguity in the determination of performance on Q₂.

Accordingly, the aim behind the 2nd Empirical Note was to show that, at the point of answering Q₂, conservers are more likely to - incorrectly - suppose that there is more to come in relation to the array than non-conservers.

Examination of teaching videos - involving children and varieties of C.T. - from Aberdeen University played an important role in prompting this part of the investigation. The reactions which were of interest to me now were:

(a) tone and style of responding to Q₂ (waiting vs non waiting response), and

(b) that to the termination of the episode by E, after S has answered Q₂ (surprise vs non-surprise).

The first step was to re-examine, where possible, the tape recorded interactions involving the above 40 subjects. However this proved unsatisfactory owing to (a) the absence of accompanying visual information relating to conduct, and (b) the frequent sparseness of verbalisation towards the end of the episode on the part of the subject.
The second step involved an analysis of 19 previously untested subjects (the other subset of the total of 59), taking into account both heard and seen aspects of accompanying conduct. Thus (see also p. 132):-

(a) in relation to conduct accompanying performance on Q2, what I took account of included (i) tone of answering, (ii) whether or not the array continued to hold S's attention (posture/changes in posture relevant here), and (iii) facial expression (and changes occurring)

(b) in relation to S's reaction to E's subsequent termination of the episode, I was concerned whether or not S registered surprise in some way. Surprise was taken to support a "waiting" interpretation.

The results were as follows:-

<table>
<thead>
<tr>
<th></th>
<th>Waiting</th>
<th>Non-Waiting</th>
<th>Indeterminate</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>P</td>
<td>non-conservers</td>
<td>1</td>
<td>3</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td>conservers</td>
<td>2</td>
<td>0</td>
<td>2</td>
</tr>
<tr>
<td>O</td>
<td>non-conservers</td>
<td>1</td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td></td>
<td>conservers</td>
<td>5</td>
<td>1</td>
<td>0</td>
</tr>
<tr>
<td>Total</td>
<td>9</td>
<td>5</td>
<td>5</td>
<td>19</td>
</tr>
</tbody>
</table>

While the posited link between "success" and waiting was supported there were obvious shortcomings in such a procedure - allowing as it does the intrusion of an experimenter bias in the course of making his/her judgements. However, this can be passed over, given (a) that the results are - at least in part - anticipated by the work of Bovet et al (1981) and Neilson et al (1983a), (b) that their importance lay largely in prompting my turn to adults where evidence of unimpressive success was also sought (and found); and correspondingly, (3) the form of my subsequent analysis: the significance of what the subject does/does not do became subordinated to the project of elucidating the potential significance of what the experimenter does/does not do.

Should this not have been the case, it would have been advisable to ensure that judgements of waiting vs non-waiting (incorporating surprise at termination vs non-surprise) are, as far as possible, done on a blind basis. Video recording techniques and naive judges might be employed. The judgements would still be made in the knowledge of how the S performed, but potentially biasing information as to whether a classical or modified C.T. was involved could be hidden by appropriate editing.
In the extreme, the naive observer might be asked to make a judgement on the basis, say, of two stills from the recording: one at the moment when S has just finished answering, and the other at the moment when E has just finished "terminating".

The final step, involving 13 children, was indeed to film them as they participated in classical and modified C.Ts (and in other Piagetian tasks). While the aim was a general, illustrative one, it affords the opportunity for detailed analyses of subjects' responses - should a good (development-related?) reason emerge for doing this.
APPENDIX II

Adults and the Conservation Task ("P"): Transcripts and Protocols
(see Chapters 5 to 7)

Subject 1 (female, 26 years, German)

Transcript

E. What do you see in front of you?
S. On the table I see some glasses, water, matches, candles and ... a mess of things.
E. Is there more in this one [A], less in this one [A], or the same in each [of the glasses A and B; this question is Q.1]?
S. More in that one [B] ... Ah no [she moved to the side] you have to view them from here [to see them as the same][she was correct in this]
E. What information are you using to say "more", or the "same"?
S. What information am I using? [I confirm] Well ... my eyes. I just looked to see if there was the same level.
E. From here [her position] then ... Q.1?
S. The level is the same, but perhaps there is more in this one [B].
E. Why?
S. Because they are not really the same ... the glasses.
E. O.K. ... let's pretend that they are. But I'll certainly take some out if you want.
S. [Apparently taking pity] No I think its the same.
E. Watch carefully [Pour contents of B into large glass, C]. Is there more in this one [A], less in this one [A] or the same in each [of A and C; this is Q.2].
S. It's the same. The same quantity of water ... in a different glass.
E. O.K. That's it [she looks quite surprised and quite disorientated] ... What do you think is the point of doing all this?
S. You asked me about whether it was the same amount of water because of the level ... and now I couldn't say that ... I just knew that it was, before, the same quantity of water.
E. When I poured the water, did it not cross your mind I might have done some magic or conjuring?
S. No.
E. What do you think the point of it all was?
S. Well, I wonder [laughs] ... It seemed so simple and your question made it so complicated.
E. Great [encouragingly] ... I could see you were surprised when I said "that's it." I could see you were waiting [questioningly]?
S. Yes [emphatic, relieved that this was not an inappropriate reaction] ... for the point.

E. O.K. Imagine it was a pub ... relaxed atmosphere, no tape recorder. Would you have any other ideas about what might be going on?
S. Yes. Sometimes people do really silly things in pubs ... because they are bored. I remember that word association thing ... [etc]
E. What relevance do you think this has got to my work as, supposedly, a psychologist?
[Total blank. General bewilderment]

E. Let's go back to the pub. How would you react?
S. I think I would have reacted in the same way, because ... [pause]
E. Because I was bored?
S. No. But it perhaps depends on you as well. If I saw that you were really interested, and that this really meant something to you, that you really wanted to know the answer ... never mind if it was in a pub, or here.

[Throughout, the delivery was thoughtful and careful]

Protocol

I started describing the mess on the table, thinking that I was probably being analysed and that the matches, candles, etc were supposed to have psychoanalytic meaning. But eventually I decided not to bother about being analysed. At this point Robert asked which glass contained more water. At first I thought there was more water in the right one, but soon I realised that this was an optical delusion [sic]. The glasses had the same size but a different volume (?) of glass. They obviously contained the same quantity of water. I could tell that, because the level of water seemed to be the same in both glasses.

Robert then poured the water from one of the glasses into a different glass and again asked me which glass contained more water. This question appeared to be the point of the test. I wondered what it was all about. If someone else asked me these question in a different context I would probably find this person rather childish. But Robert really seemed serious and interested in my reaction.

Subject 2 (male, about 26 years, Scottish)

Transcript

E. What is there in front of us?
S. Pint glass and two glasses of ... water [? questioningly] ... almost full.
E. Q?
S. [Pause] It appears that that one [B] is down farther at the base. That's not to say its got more water in it though.
E. Does it? [sic] What would you have to do to even them up?
S. I'm not saying its got more water in though.
E. [long pause] I think its got the same [nervous laugh]. So I'll ask again: [Q,]?
S. Its not going to make a lot of difference. I still think slightly more in that.

E. Watch carefully [pour B into C]. What happened?
S. You poured one of the glasses into the pint glass.
E. Qs?
S. Obviously it would be just the same only slightly more.
E. Slightly more? [surprised] Why?
S. Well its the same water that came out of that glass where ... 
E. Oh I see ... because there was slightly more the first time. well that's it ... the end of it [S frowns, looks disbelieving and examines the array] You seem surprised.
S. [After oppressive pause] Well [threateningly], what conclusion do you want me to draw from that then?
E. [a little irritated] I don't want you to draw any conclusions. But what do you think was going on? What do you think I was up to? Why do you think I bothered to come round to do all this?
S. To see whether observation changes ... consistently.
E. Could you jot down your impressions of what happened ... that might be relevant to the situation?
S. Do you want me to write anything I found relevant?
E. Yes.
S. I didn't find anything relevant [dismissively]. I thought that the two schooner glasses weren't exactly the same. Anyway, how am I supposed to know that it was water in both glasses at the start? I only guessed it was water because I think I heard you getting it from the tap in the kitchen.
E. [exasperated groan]
S. It's a fact. I can't look at liquids like someone else. Remember I did three years of chemistry. It changes the way you look at liquids ... volumes and that.

[At this point we are joined by S2's cousin (subject 3, below)]
E. What would your impression be if this took place in a pub?
S3. He'd just look as if he was bored with the company, wouldn't he?
S. I'd think you were trying to win some money off somebody. That's the sort of thing I do.
E. You mean some sort of trick?
S. Aye.
S3. Magic.
D. The pub's a trick environment. I used to do it with cigarettes. At the end they're always trying to get some money out of you.

He refused to write a protocol, saying that all he would write would be that everything that happened he found to be irrelevant. He was cautious and critically sceptical throughout: apparently a combination of (a) knowing something about Asch's group conformity experiments, (b) knowing, as a chemist, a lot about the interesting properties of liquids, and (c) being irritated that I had caught him just before he went out.

Subject 3 (female, 25 years, Scottish)

Transcript
E. What's going on [here on the table]?
S. Well you've got two identical glasses with two equal amounts.
E. [Pours B into C] What did I just do?
S. Well, you poured a glass into a pint glass which gives you half a pint of water [? questioningly]
E. You're looking at me very quizzically.
S. [dubious about this word] Half a pint of water ... But still the same amount of water in the first.
E. Yes [noncommittal]
S. Well?
E. Nothing. I'm just interested in what you're saying and how the whole situation strikes you.
S. Did I miss something?
E. No. There was no right and wrong answer.
[Later, having set up the array again while S did something else]

E. Q₁?  
S. There should be a fraction less in this one [B] 'cos you've still got some dregs of water in the other one [C]. Get it all out [as I shake the remains in C into B]. Its ... no its not. Its slightly higher than this one [A], Robert. You do something wrong in the first bit?  
E. Well I'll pour a drop in. Why am I doing it though?  
S. To make the glasses have the same amount of water.  
E. And why should I want to do that?  
S. I don't know. You tell me. The way you are playing about gives the impression that you want equal amounts in them.  
E. O.K. Q₁? [repeated]  
S. I would say this one is slightly more. Maybe its me eyes.  
[I pour a few drops from A into B and she agrees] O.K. [same]  
E. O.K. Watch [I pour B into C]  
S. Oh! [mock surprise]  
E. Q₂?  
S. There's the same ... [pause]. There's the same [resignedly, now getting bored]  
E. That's the end.  
S. It's just that the glasses are different, which gives you the impression that there's less water in this one [C]. The volume of the glasses is different.  
E. What are your comments?  
S. Hmph! [disappointed]  
E. You sound disappointed  
S. Yes. I was expecting something more.  
E. What sort of thing?  
S. I don't know ... well, when you pour one glass of water into another ... well ... well what!? [laughs scoffingly]  
E. Suppose I'd done this in a pub and you didn't know anything about me ... like being a psychologist.  
S. I'd think you were playing with your glass with boredom. Lack of company. [she is definitely fed up]  
E. Anyway can you write down just what happened, and ...  
S. What, that you made a mess of water on my kitchen table. That's all that happened.

Protocol  

I was expecting more to this experiment. But no, it was very simple and not at all misleading ...  

[This subject knew before the start that the experiment was something to do with judging the amounts of liquid in the containers]

Subject 4 (male, about 23 years, Scottish)

Transcript

E. What do you see in front of you  
S. Two glasses, identical, same volume of water.  
E. Q₁?  
S. Same
E. Watch closely [pour B into C]. Q2?
S. There's the same [waiting, get-to-the-point tone of voice]
E. That's the end [IS surprised]. Any comments? What's going on?
S. [Thinking hard] Well it's something to do with ... ummm ... capacity. Obviously just to look at it, if I was to walk in the room, the first impression would be that there's more than that [A more than C]. But it's not.

Protocol

On entering the room, a squash raquet fell - nothing to do with what was to happen!? Phone went! - Again nothing to do with the experiment? There were two tall slim glasses filled with equal amounts of water. Described what I saw. Water from one tall glass was poured into one of the wide glasses. There was a difference in water level, but amount of water was equal.

As it was an "experiment" I realised there is more to this than meets my eye. But what, I'm not too sure about as my knowledge of psychology is limited. If it had been demonstrated in a pub, or somewhere else, as a "blind" experiment, observations may have been more genuine as I wouldn't view it as an experiment.

[He was expectant but quite cautious and even nervous at participating in a psychology experiment]

Subject 5 (female, about 21 years, Scottish)

Transcript

E. What do you see
S. Three glasses, two full of water, one larger than the other two.
   Larger one empty.
E. have you any expectations about what I might be about to do?
S. No ... I might have to drink some of the water.
E. Q1?
S. Equal amounts
E. Watch [pours]. Right what happened?
S. You transferred some of the water from the smaller into the larger glass.
E. Some of the water?
S. All of the water.
E. Q2?
S. Equal.
E. If I was to say that was the end of the experiment, what would you say?
S. I wouldn't think you've achieved anything.
E. On the contrary [laugh]. So what do you think it was about - because that is actually the end of anything I do [she needed convincing]?
S. You maybe just want to know different people's explanations for actions ... [floundering] descriptions?
E. Say I did this in a pub ... ?
S. You'd expect a trick
E. Some people say they'd think I was just bored ... (?)
S. No. I think the person doing the trick wouldn't be bored. But everyone else would be.
E. If I did that with children and asked them the same questions ... ?
S. What? About equal amounts? ... och, they might be slightly confused by the water looking less in the pint glass.
E. [pouring C back into B] How do you know they were the same amounts?
S. I can see the levels.
E. How do you know the glasses are ... ?
S. Have got false bottoms or something? [inspects the glasses] I can't see anything like that.
E. So you just assumed?
S. I just assumed ... I took what I thought to be ... what I thought was so.
E. So why do you think, doing it with children, that I bother to do it with adults?
S. I'm not sure ... get different responses ... because you know that the child would say there is less in a pint glass ... Generally an adult would be able to carry forward.
E. Carry forward?
S. Information that it knew it was the same.

Protocol

On the table were three glasses, two small, one pint tumbler. The former held equal amounts and the pint was empty. More to this experiment than meets the eye? Next, the water of one glass was transferred to the large tumbler. Would I say there were equal amounts in these two now? - Yes (obviously). But would a four year old think so? Probably not.

When asked what I would have expected having been shown and asked the same in a pub, I said I would expect some sort of trick, which might have made it a bit more interesting.

To do an "experiment" without knowing either the aim or result was ever so slightly boring ...

Subject 6 (male, about 24 yrs, Chinese)

Transcript

E. What do you see on the table?
S. There are two cups of water ... and that empty cup [glasses]
E. Q1?
S. Compared with this? They're the same
E. Watch [pours B into C] What did I do?
S. You poured the water from one cup into another.
E. Q2?
S. They're the same ... Because you just ... because they're the same.
E. Now if I told you this was the end of the experiment, what would you think?
S. I would think this is for 3 or 4 year old children.
E. Are you saying that because you know something about me?
S. No, No, No. - I think that the ... I don't know why you do this with adults because the other can know ... because there is no problem. They can get the correct answer.
E. How do you know? What is the correct answer? For instance the first question (Q1). What basis did you answer that on, and how did you know it was correct? [I pour C back into B].
S. It depends how big the cup is.
E. But why did you say that they were the same?
S. Because I think they are the same.
E. A sort of guess then?
S. Yes a guess, because they look the same, you see ... and you get the same things.
E. Do you think I made sure that they were the same?
S. No. But I was sure before the finish of the experiment ... but now I think they are not the same [laughs]
E. Whether the same or not, do you think I tried to make sure that they were the same, or make sure that they were not the same?
S. I don't think you made sure of that. I think you use a different cup and make sure they have the same height of water in each ... But now I think that the diameters of both cups are different.
E. Do you think I was trying to confuse you, or what ... ?
S. To confuse me.
E. So what do you think a child would do here? [at Q1]
S. A child? I think he would never answer the same.
E. And now [after pouring B into C]?
S. The child would say that this one is less [C], and this one is more [A].
E. If this took place in a pub, would it make any difference?
S. No, no difference I think. I would answer in the pub with the same answers ... it depends on knowing the people [doing it]. I think there are two parameters to determine how much water is inside. One is the diameter, the other is height ... But when I guess, I would just use one of them in most situations.
E. An informed guess? [agrees]. When I said that it was the end of the experiment, what was your reaction? [S is puzzled]
E. Do you think I might have been about to do something else?
S. I don't think so.
E. Do you think I'm an idiot? I just wondered if you were disappointed?
S. No, not disappointed ... Because I knew it is a psychology experiment, and [therefore] you don't know where it starts and when it stops. Everyone can create different ways of doing things.

[He was a post-graduate psychology student]
S. Well if there was the same before, like you said, I suppose there's the same now [doubtfully, still waiting]
E. That's the end
S. [Amazed] What's the point?
E. To see if subjects can conserve amounts of water. See that it is unchanged, even if its appearance changes.
S. [Incredulous] Does anyone ever get it wrong?
E. Children of four for a start.
S. Do they? Well that's interesting isn't it?
E. Not particularly.
S. No, I suppose not.

Subject 8 (male, 40 yrs, English)

Transcript

E. What do you see in front of you?
S. [Looking in front of him, out of the window] I see the bracken, the river, pretty low...
E. And inside?
S. A table with objects. Wine bottles with candles...
E. What did I just do?
S. You put three containers on the dirty table. Two were filled with a substance or substances, one or other or both which could have been water... or neither.
E. Q1?
S. More or less what?
E. Liquid.
S. Insufficient evidence. I haven't a clue.
E. [Helping me out] Would you like me to use my judgement and make a guess [I nod]. I haven't got my glasses on, but I'll say there's a bit more here [B]. I'll guess that they're the same now [after he had poured some from B to A]
E. Now watch carefully [I pour B into C]. What did I do?
S. You poured some of the liquid in one of the smaller containers on to the table, and some into the large container. Most into the large container.
E. Q2?
S. I still haven't really got a clue. But assuming that I guessed right before, and disregarding the gunge at the bottom of the big glass, I'll say they're still the same.
E. O.K. That's it.
S. [Pause, disbelief]
E. What do you think was going on?
S. You were probably doing something with observation. The way people observe.
E. Suppose all this had taken place in a pub with beer. What would have been your impression?
S. You'd be drunk.
E. Supposing I wasn't?
S. You'd be trying to rip me off. Some sort of trick. Anyway it would be a waste of beer ........................................
You're seriously doing a PhD on that!?
APPENDIX II

Phenomenology's Critique of Piaget

A. Introduction

In an expanded version of this critique (paper in preparation) I trace out and link Merleau-Ponty's criticisms of rationalism, cognitive approaches in psychology and Piaget. The existential phenomenological alternative for psychology is also discussed. Here I will move straight to the implications for most effectively exposing the deficiencies in Piaget's genetic epistemology and child psychology.

In chapter 2 I briefly discussed the two biases that Merleau-Ponty (1962) identifies in rationalist epistemology:

(a) its "prejudice in favour of the world"
(b) its "intellectualism"

The first is a prejudice shared with empiricism (see Merleau-Ponty, 1962, p.39). The second is what Kestenbaum (1974) calls an unwarranted "prejudice in favour of mediated experience". Summarising Merleau-Ponty's comparison of empiricism and rationalism, Spurling (1977, p. 13-14) says

In spite of their apparent differences, empiricism and rationalism converge to present a remarkably similar picture of the world. Both suffer from what Merleau-Ponty terms a 'prejudice in favour of the world.' Empiricism stops there; it has no conception of consciousness or subjectivity, but only of an objective world. Rationalism conceives of consciousness as occupying some place above and beyond the objective world, which somehow endows the inherently senseless physical world with significance. For empiricism there is no subjectivity. For rationalism, subjectivity occupies some ethereal realm where it operates on the world from a distance. Neither can conceive of any living dialogue or dialectic between the subject and the world. Both are inherently dualistic, relying on the rigid distinction between subject and object ... Both are also atomistic, decomposing
experience into little bits, to be joined together either by random association, or through explicit and conscious acts.

There is plenty more to be said here. But the immediate aim is to show how the two criticisms apply to Piaget's genetic approach to thought and knowledge.

Cognitive approaches, separating subject and world, tend to view the latter as a problem and the former as a problem-solver. Cognitive interpretations of conduct then only work when questions or problems encountered by the subject can be identified - if not prescribed by the investigator, then posed by the world or environment. The following quotation (Piaget, 1980, p.79) is instructive:

If it were not for the multiple problems raised by the environment or outside world, both organism and subject would remain conservatively oriented and incapable of new invention.

In equating the relationship between, on the one hand, organism and environment and, on the other, subject and world-to-be-understood, Piaget is putting forward the view that cognition is to be seen as a refined form of biological adaptation (also, in effect, that biology can be viewed in cognitive terms - see Chapter 9). The view expressed in the above quotation is "basic to his whole approach to genetic epistemology ... [and the source of] some of the difficulties in which he gets involved" (Goodwin, 1982). In Chapter 9, section B, I point out that it is Piaget who might be and has been seen to remain unduly "conservatively oriented" and incapable of adequately addressing new invention.

Piaget's view of the natural world underpins his theory. As indicated in chapter 2, that it can be challenged by natural scientists should make us circumspect about his genetic epistemology. But it is his faith in the approach of the natural sciences which suggests that Piaget may be charged with a distorting "prejudice in favour of the world". 
B. Piaget's Prejudice in Favor of the World

Basically the criticism is that, in defining his project of genetic epistemology, Piaget allows a naive realism into his approach. Any naive assumption about what is to be known is inappropriate for a supposedly fundamental investigation of the processes of knowing.

While Piaget finds the child-world-knowledge relationship problematic, he does not appear to find his own methodology so. If, as we are led to believe, Piaget undertook his psychological research in the hope of solving epistemological problems, then he seems to see science and the scientific method as some firm anchor point from which to set off. But then his methodology, in a sense, begs the question, since it assumes that which is, in part at least, at issue and which he is trying to explain. The clear danger is that, by treating his experiments with, and observations of, children as providing archimedean neutral points, Piaget is failing to recognize the fact that his own observations are not neutral but radically theory laden. It is as if to accept a naive realism which is taken as an a priori of his work ... This is not specific to Piaget; if true it would undermine the whole of science as a valid enterprise as distinct from a game engaged in by consenting adults in private. The world is the way our conceptual structure maps it, and the idea that by examining the world we find out something about it is mistaken; all that we find out about is the map which we have imposed on it, in this instance a particular map of cognitive development.

[Garfield, p.184]

Rotman (1977), a mathematician, also emphasizes Piaget's "visionary" faith in the objective reality prescribed by the natural sciences and guaranteed by its underpinning of mathematics. Significantly, the title of his book is "Jean Piaget: Psychologist of the Real". In it he especially challenges Piaget's misguided notions as to the supposedly timeless and perfect truths of mathematics. He objects to Piaget's portrayal of mathematics as "a form of perfection: the culmination of adaptation to reality, the most perfect instance of biological knowledge whose congruity with the real is absolute" adding that "Perfection is the stuff of visions and Piaget's dream here is the dream of a rational mystic" (p. 171). Rotman points out that Piaget overlooks the arbitrary influences that have determined the course of the history in that area of knowledge, and neglects the social context of understanding in general - a widely expressed criticism of Piaget.
The "prejudice" noted here then is that in favour of the world of objects. As this is not the world we actually think, learn and understand in - or at best is only one aspect of it - distortions will arise if we neglect other aspects. After referring to the visionary side of Piaget, the "mystic", Rotman acknowledges a more down-to-earth aspect.

Piaget speaks as a plain man, a solid empirically-rooted realist. A man possessed by the Kantian preoccupation with ineluctable necessities, of what must be the case, and not what by some imagined or synthetic construction might or could be so. Against what are often uncertain metaphors, flimsy speculations and dialectical leaps he offers facts, experiments observations and predictions ... But Piaget's evolutionary view of man, with its dedication to the idea of intelligence as mediation of the real, exacts a price. His chosen (biological) starting point ... and his resulting logic of the real leaves culture, language, and social formation in blurred penumbra ... his theory is most impoverished and unconvincing in its treatment of language ... He neglects that viewpoints are within language and that language is a social relation that constructs an individual's categorisation of the world. And in doing this he replaces human rationality with the logic of objects.

(Rotman, 1977, pp. 179-81 - my emphasis)

Piaget, of course, resists being pinned down on any reductionist charge by invoking his famous "circle of sciences", which presumably includes the social sciences and certainly includes psychology. But when he says that physics presupposes psychology just as psychology presupposes physics we have a right to be sceptical. As Wiggins (1981) says, it is absurd to claim that physics "presupposes" psychology in anything like the same way that the reverse is the case. Referring to Piaget's "circle", Rotman (1977, p.172) says that "at this level of enigmatic abstraction answers are as mysterious as questions". To seek the guarantee of scientific knowledge - and thus to try to justify a scientific approach to epistemological questions - from within a scientific perspective is what Husserl would call scientism. Merleau-Ponty would identify it as the manifestation of an unwarranted "prejudice in favour of the world".

Merleau-Ponty does not seem to take Piaget seriously as regards the ontological framework operating as the background for the latter's investigation of the child. But he takes the investigations themselves
seriously. And in doing so he criticizes Piaget for (1) importing an "adultocentric" bias when he purports to be investigating the child's world, and (2) of holding to an untenable intellectualism in addressing the processes of understanding involved.

C. Piaget's Intellectualism

(a) Phenomenology's defence (?) of Piaget

There is an ever-growing body of authors who have noted convergences in the projects of Piaget and phenomenology. Thus Marbach (1977) compares the former's genetic epistemology with Husserl's genetic phenomenology, Bolton (1977, 1978) also commenting on the relationship between the two approaches. Gurwitsch (1964), Zaner (1964, 1966) and Wiggins (1977, 1981), amongst others (see below) were struck by the links that could be discerned when the works of Piaget and Merleau-Ponty are compared. However, most have found that Piaget's intellectualism rules out pursuing the convergence beyond a certain point. This is not before the possibility of giving Piaget the benefit of the doubt has been explored.

Bolton (1977, 1978), Rojciewicz (1987) and Kestenbaum (1974) each suggest that there are grounds on which Piaget might protect himself from the charge of intellectualism as it applies to straightforward cognitive approaches to the construction of perceptual experience and the interpretation of conduct:

There are surely a number of resemblances between Merleau-Ponty's analysis and Piaget's. There is ... an implicit recognition of the pre-reflective in Piaget's idea that the original relationship between subject and environment is one of lack of differentiation between the two ... In this respect at least, then, Piaget's theory is not to be characterized as intellectualistic in Merleau-Ponty's sense; rather, his position is more in accord with Gibson's than with any theory that urges that stimuli have to be mediated by interpretations in order for meaning to appear.

[Bolton, 1977, p.30]

Both Bolton and Rojciewicz see Piaget's notion of sensori-motor intelligence as a potential step towards overcoming the difficulties associated with constructivism's dichotomy of subject and world.
It could be maintained (that) Piaget has provided us with a description of pre-reflective experience in his account of the sensori-motor stage of development. He would surely agree with Merleau Ponty's (1962) view that the primary fact of consciousness is not 'I think' but 'I can'...

[Bolton, 1978, p.218]

Along the same lines:-

For Piaget, perception is to be understood in terms of what he calls sensori-motor intelligence. At first blush, this looks promising. To join the sensory and the motor already implies the surpassing of the traditional dichotomies between thing and idea, physical and psychical. For ... if the sensori-motor is a unitary phenomenon, then the sensory cannot be understood as simply an act of consciousness, the contemplation of a content, the possession of a quality; and the motor cannot be understood as simply a physical, third person event. For ... understood in these traditional ways, the one is of the order of consciousness and the other is the order of things and there is no internal connection possible between them. Also, to speak of sensori-motor intelligence implies (rightly that) there is a kind of knowledge at a level beneath intelligence proper ...

[Rojcewicz, 1987; his emphasis]

Kestenbaum (1974) points out that Piaget does not deny the existence of lived experience and quotes Piaget (1972 b) as saying: "I have tried to preserve as much as possible the character of a lived experience" (p.215-16) and agreeing that the human subject "is engaged in the world and attributes everything a 'meaning' from vital, social or personal, as well as epistemological points of view" (p.225). Kestenbaum even admits:

It could be argued that Piaget's entire genetic epistemology is an attempt to explain how the immediate, unordered phenomena of experience are progressively and developmentally organized into cognitive structures.

However, he goes on to say that:

[This] begs the question since it assumes what I am trying to deny, i.e. that meanings immediately experienced are simply or only the occasion for cognition and are incidental to cognition's elaboration of distinct and ordered objects of thought. [In trying to hold such a view we see] Piaget's confusion concerning the nature of immediate, lived experience.

Bolton (1977, p.31) is making the same point when he says that:
For Piaget our transactions with the world at the sensori-motor level attain their significance for thought only as a necessary first step towards the development of logico-mathematical operations.

Likewise, after his own attempt to give Piaget the benefit of the doubt, Rojcewicz goes on to find Piaget guilty of a distorting intellectualism. For him and "for Merleau-Ponty ... Piaget does not fulfil the promise contained in the notion of sensori-motor intelligence but falls back into intellectualism". Such support as there is can thus be seen to be severely qualified. And the reservations are mutual. Piaget is highly critical of phenomenology's project while being manifestly under-informed about it.

(b) **Piaget's criticisms of phenomenology**

In "Insights and Illusions in Philosophy" (1972 b) Piaget purports to give phenomenology a hearing. In doing so, however, there can be no doubt that he is referring to the "illusory" rather than to the "insightful" side of the coin.

Firstly, Piaget criticizes phenomenology for starting its analyses from the meaning given in immediate, lived experience. Insofar as we are to accept it, he says, we also have to accept that it has a history. An account of its genesis is called for if we are to fully understand it and its mode of functioning. Bolton (1978, p.218) paraphrases Piaget's (1972 b) criticism of Merleau-Ponty's notion of pre-reflective experience:

> As soon as we take up a developmental position, the concept of 'the originary experiences of lived consciousness' is seen as mythical since all structures have a history, that is they have to be related to a series of preceding structures.

(If we find we cannot accept such a concept as mythical, this might contribute to the argument for refraining from taking up a developmental position where possible - see main text)

Secondly, setting the genetic question aside, Piaget maintains that, insofar as immediate experience of the world is to be accepted it is something to be overcome by cognition, not implicated in or directing it, apart from in an energizing capacity (Kotman, 1977, pp. 99-101).
Immediate experience and feeling "supplies the energy for action, while knowledge impresses a structure on it" (Piaget, 1968, p. 4). For Piaget:

[There can be] very little relationship between the confused meanings of immediate experience and the logically ordered meanings of ... mediated experience. In other words, if it is mediated knowledge which provides us with our most accurate and reliable accounts of reality, why not escape the clutches of lived experience as quickly as possible, instead of wallowing in it as certain existential phenomenologists are inclined to do?

[Kestenbaum, 1974]

What goes for everyday cognition goes for scientific thinking only, apparently, more so. Thus we get the most startling and emphatic statement of his position, and rejection of the basis of phenomenonology when he says (perhaps a little carried away by the attractions of "counterintuitive" scientific explanations) that:

the aim of scientific thought is always to get further away from this lived world, contradicting it instead of utilizing it.

[Piaget, 1972 b, p.87]

In effect then, Piaget is saying (1) that if we are going to take the meanings presented in immediate experience seriously, we have to address the question of genesis. but (2) we do not have to take them seriously as they only present misleading "appearances" and the clues or energizing stimuli for real cognition to get at the real reality behind those appearances. The only interesting genetic question then concerns the origin of those organising structures or schemata that have the capacity to yield true understanding.

Piaget then, seemingly without qualms, adds a further dichotomy to the traditional ones by irrevocably splitting off the "epistemological subject" from the "psychological subject", the former being the source of all knowledge about reality, and the latter knowing nothing except how to provide the spark in the quest for knowledge.

In the above-mentioned book, Piaget (1972 b) gives us no reason to suppose that he is equipped to evaluate phenomenology's project. For one thing he appears to confuse its methods with those of introspectionist psychology. This represents a serious if all-too-common fallacy (Giorgi, personal communication).
(c) Criticism of Piaget

In relation to the above points, the most telling criticism of Piaget here is that it is he who leaves a crucial gap in his genetic enquiry. In seeing the birth of meaning as the exclusive result, in any particular instance, of internal interpretative acts performed by the epistemological subject, he starts too late and fails to see that there must be an ever-present continuity between a subject's immediate sense of something and his/her conception of it. There is a rejection here of Piaget's view that anything that appears pre-cognitively for the subject has no more status than the energizer for cognition:

Piaget considers lived, immediate experience to be an incidental factor in knowledge, ... providing simply the 'trigger' ... which starts cognition. Once cognition is started, immediate experience is, so to speak, jettisoned, left farther and farther behind as assimilation to reason picks up greater and greater speed. Dewey, on the other hand [pointed out that] ... one's immediate sense of a problem is not merely a 'spark' igniting cognition: it is the context out of which reflection grows and discriminates distinct objects or texts of knowledge. [It] guides the movement of cognition's construction, and in turn is itself modified by the emerging texts or objects of knowledge. ... [Thus] Dewey considers the mediated and the immediate to be in continuous interaction throughout the entire process whereby an object of knowledge is constructed.

[Kestenbaum, 1974]

This last comment goes some way to answering Piaget's "genetic" challenge to the notion of immediate experience. It becomes obvious that the way we initially encounter the world, in any instance, will be influenced by the consequences of previous encounters, including our rationalizations of them. But the meaning arising in the immediate encounter should still remain the starting point for analysis. More important here is the "genetic" challenge to Piaget, as a result of the identification of what can be called his "prejudice in favour of mediated experience".

We cannot talk about mediated experience without at least implicitly referring to what is mediated. In the end this has to be allowed to be un-mediated, i.e. immediate experience, or something indistinguishable from it. Phenomenology makes a point of demonstrating that there is nothing less "real" about the originally given - even when this is totally confusing and problematic, or in the case when it
subsequently turns out an error has been made. And it also demonstrates that the directly given is presupposed by any knowledge of reality arrived at through the subject's conceptual operations. The latter meanings have to be seen in any concrete instance as logically and temporally, posterior derivatives or elaborations of the unmediated level of meaning-giving. Thus we cannot understand the work of cognition in resolving confusion, solving problems or correcting for an error unless we acknowledge the prior "registering" somewhere of something in reality confusing, an actual problem posed, something really given with an element of doubt and needing to be checked.

As I indicated above, it is a feature of intellectualism to reduce the world to the problematic and understanding to problem-solving. Without invoking pre-cognitive, immediately-given meanings for the subject (or his/her cognitive apparatus), we are left having to admit that the subject lacks the necessary means for (1) seeing/registering that there is a problem, (2) sensing the parameters of the problem, (3) sensing the possible ways for interpreting and resolving the problem, and (4) registering when the problem is solved. More generally, Kestenbaum quotes Dewey (1925, p.21-22) as saying:

Unless there is something immediately and non-cognitively presented in experience so that it is capable of being pointed to in subsequent reflection and in action which embodies the fruit of reflection, knowledge has neither subject nor objective ... [To elevate the cognitive to a privileged position is to ignore that] context of non-cognitive but experienced subject matter which gives what is known its import.

Kestenbaum draws attention to the implications of Piaget's genetic lapse.

What is remarkable is that [Piaget] apparently does not see why such continuity (between immediate and mediated experience) is essential for a psychology purporting to be a genetic epistemology. This blindness is serious, because if it is the case that immediately lived experience is necessarily and not merely incidentally related to the genesis of knowledge, then the plausibility of Piaget's entire genetic epistemology is critically weakened.

Piaget of course does give a genetic account as to how those internal organising structures or schemata come to be there - albeit in terms of an idiosyncratic and eminently challengable notion as to the processes of biological adaptation. In chapter 2 I mentioned the
widespread scepticism about the role that the supposedly explanatory concepts of assimilation, accomodation, equilibration, conflict, etc., play in his theory. The point is that, however the schemata come to be there, in the light of the above, we are unable to conceive of how they actually work in any particular instance of meaning-giving on the part of the subject (or his/her cognitive apparatus).

How then do [Piaget's] schemata operate? They regulate the sensory data; they are applied whenever the subject is presented with raw data which are analogous to the data which the schema has already organised in the past. With age and practise this process becomes automatic and scarcely noticeable.

As this author (Rojcewicz) says, it now becomes obvious why Merleau-Ponty calls this a classical intellectualistic account of perception. Rojcewicz goes on:

The regulation by the anterior schemata is like a projection of memories, an application of an ideal meaning on to contents which are meaningless in themselves ... They are meanings or organizations which ... derive from an active intellectual [synthesis] (a synthesis posterior to the apprehension of the raw contents) ... Merleau-Ponty's criticism is this: in order for the imposition of meaning on to raw data to be possible, there must be something in the present perception, in the data, which tells us we have a case before us of something already encountered in the past and to which the already acquired schema would be applicable ... The present perception must already be organised like the perception in the past to have the same contents as the past perception and thereby make the application of the schema possible. But then the application of the schema is rendered superfluous ... Its work is already accomplished, the appearances already look like schematized appearances, they are already organized and they do not need to await the imposition of a meaning from the outside, from a source beyond the faculty of grasping the contents ...

The notion of raw data or raw contents is fundamentally unsound. A level of contents, sensations, meaningless sense qualities, is an abstraction. In the first place, if knowledge began with such a layer, meaning would never arise, for we would never be able to recognize in the contents any meaning which we would subsequently want to impose on it. There would be a layer of chaos and a layer of pure meaning, and the two layers would not communicate. There must be something in the layer of contents which is the point d'appui for the recognition of meaning; but then this point d'appui is enough, and the application of a schema is superfluous, its work of organising the data would
already be accomplished. Secondly, a meaningless layer of contents is nowhere to be found in experience. Whether or not knowledge could be constructed starting with such sensations (we have just seen that it could not) they nowhere occur in experience. Matter is not indifferent to form (when we reverse our perceptual figures and grounds, we do not merely obtain the same data, now synthesized one way and now another, but a "whole new world". That is to say both form and contents change); ... matter depends on form, matter is not given without some form or other.

Now Merleau-Ponty has no wish to argue out of existence the useful functioning of an organising intellect. So we must accept the alternative, i.e. that Piaget's (or any) account of perceptual experience as the product of the projection of memories, or imposition of order, on to unorganized contents must be wrong. Instead we have to accept some sort of immanent organisation of reality prior to that following from an intellectual act. This level of meaning, in any concrete instance, may or may not be taken up and elaborated on by the intellect.

From a correspondingly critical standpoint, Bolton (1978, p.219) says:

The critical realism of Piaget, like that of Neisser, remains thoroughly constructivist. I have no knowledge of the world, they tell me, which could both precede and transcend interpretative knowledge.

Because Piaget's project is to provide an explanation of meaning and knowledge in terms of their genesis, if the above criticisms are valid, there is a serious lacuna in Piaget's genetic epistemology. Piaget never got close to addressing the charges, and it is difficult to see how he would - without having to radically water down the claim as to what was being explained by his theory. Certainly the critical authors mentioned in this section are in agreement that, in the end, the notion of sensori-motor meanings in Piaget's edifice does nothing to paper over the difficulties of his intellectualism. Bolton (1977, p.31) contrasts Piaget and Merleau-Ponty in this way:

For Piaget the achievements of the sensori-motor period are stages on thought's way (that get left behind in the developmental process), whereas for Merleau-Ponty the pre-reflective is the ever-present source of the mind's creativity.
Piaget, at this point, would probably draw a sharp distinction between scientific and philosophical enquiries, dismissing the above criticisms as the unverifiable products of dubious philosophical speculation. By contrast, he, in his capacity as an empirical psychologist, had the scientific evidence to back up his claims as to the nature of understanding and its genesis over the course of development of the individual. In the preface to "The Thought and Language of the Child" (Piaget, 1959), Claparede says that:

Piaget's thorough mastery of other spheres of knowledge, far from luring him into doubtful speculation, has on the contrary enabled him to draw the line very clearly between psychology and philosophy, and to remain rigorously on the side of the first. His work is purely scientific.

But, Piaget does not get out of it that easily. However impressive his evidence may be, it has, as I indicate in chapter 2, to be viewed in terms of some philosophical position. As Zaner (1966) points out:

All such 'distinctions' [as Piaget's, between scientific and philosophical enquiries] and discussions pertaining to them are philosophical.

And this author further notes that Piaget elsewhere acknowledges this.

It should be clear now (1) that there are crucial, identifiable philosophical assumptions propping up Piaget's approach to understanding and experience, and (2) that in two important respects we are able to refine a challenge to them - i.e. in relation to both, (a) the view we take of what is to be known, and (b) how knowledge and the processes of knowing are to be viewed, and hence investigated.

However, the most telling criticism in the end comes from doing precisely what Piaget insists we do - i.e. from looking at his empirical evidence. But, apart from noting the difficulties involving the interpretation of Piaget's facts, I show that we need not rest content with those facts, no matter how robust his results might be. Further, awareness of the two prejudices indicates the sort of descriptive difficulties liable to arise.

D. Piaget's Empirical Evidence: The Relevance of Merleau-Ponty's Criticisms Extended

Having followed through Merleau-Ponty's insights this far, I see the need to go one step further than that author. And it is with respect to the potentially distorting "developmental assumption" that I
set out to take the radical implications of his dual criticism to their logical conclusion. But first let us see where Merleau-Ponty is led vis-à-vis his critique of Piaget's framework for investigating the child.

Merleau-Ponty (1964 b), as we have seen, argues convincingly against Piaget's view of perception. In particular he objects to Piaget's view of the perceptual world of the child. For him Piaget seemed to be denying that the child had a perceptual world. Piaget and Inhelder (1969, p. 14, 26) state:

"The universe of the young baby is a world without objects, consisting of shifting and unsubstantial tableaux ... [But through a process of decentration] the little child gradually ceases to relate everything to his states and his own actions and begins to substitute for a world of fluctuating tableaux without spatio-temporal consistency or external causality a universe of permanent objects structured according to an objectified and spatialized causality."

Rojcewicz points out:

What Piaget fails to recognize in the child, in original perception, is the phenomenal realm, an organization or structure which is situated in between sheer chaos and ideal meaning. For Merleau-Ponty, the child's perception, like all perception, is always organized and never consists in chaos or mere shifting tableaux. To be conscious of pure chaos is impossible so long as consciousness is always consciousness of something [see Husserl's Intentionality Thesis]. This does not mean that the structure of the child's perceptual world is the same as the adult's ... Nevertheless, right from the start, there are ensembles which deserve to be called things and which constitute a world. This would express the ultimate sense of Merleau-Ponty's statement that Piaget misses the world perceived by the child. For Piaget, the child perceives nothing like a world.

I point out in chapter 2 that, like the optimist critics of Piaget that I consider, Merleau-Ponty accepted that Piaget's famous experiments (notably the C.T.) raise interesting developmental questions. Like them, though more radically in that he would attempt to escape the restrictions of a truncated intellectualist view of experience, he goes on to dispute Piaget's interpretations as to the developmentally significant differences between children and, say, adults revealed. What guided his interpretative project is the attempt to avoid an adultocentric bias. The child's world is not the same as the adult's;
and in describing aspects of the former, care should be taken to avoid allowing assumptions only applicable to the latter to intrude.

While not taking issue with this statement, frequently recurring in Merleau-Ponty's child psychology lectures at the Sorbonne (see 1964 a, 1964 b), the statement: "my world is not the same as my wife's/friend's", is equally true. Adopting the phenomenological attitude teaches us to start off by putting out of play presuppositions and avoiding challengeable generalisations. Thus, before deciding how to - and how not to - investigate something, e.g. intelligence and its development, the would-be investigator should (1) question his/her own assumptions as to what is being investigated and what is expected from the investigation; and (2) work from concrete examples.

In drawing on empirical work in the developmental literature, as he constantly does, Merleau-Ponty shows that he would agree that a fundamental approach requires that we look afresh at these developmental issues. Yet such a return to beginnings entails that one of the first questions asked should be: is this a developmental issue? The concrete example here is the case of the C.T.; and Piaget's starting point was to establish findings that necessarily pose a developmental question. Merleau-Ponty seems to accept that they do.

But, by interrogating some basic assumptions, I argue that they cannot be presumed to do so. And it is by carrying through Merleau-Ponty's insights to the level of empirical description that I am able to mount and support the challenges that lead to this conclusion. Combatting the "prejudice in favour of the world" entails looking beyond the investigator's privileged perspective of what he/she might have intended to occur. Counteracting the intellectualist "prejudice in favour of mediated experience" enables a view to be taken of what the subject is required, and to be seen, to do which minimises the mediating operations of an organising intellect that need to be invoked. The view of the C.T. episode that emerges is one that takes into account the perspective disparity between investigator and subject. In the process the mixed bag of C.T. findings are able to be explained, and what looked like a developmentally interesting incorrect response on the part of the young child (a) no longer can be assumed to reflect an error, (b) no longer looks particularly interesting, and (c) no longer can be assumed to pose a developmental question.
**BIBLIOGRAPHY**


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