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George Brown

Life story interview with Paul Thompson

2000

Principle investigator's thematic highlights

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Professor George Brown
Interviewed by Paul Thompson
6 & 18 April 2000, 12 May 2000

1: Anxious Parents

[My father was a] rather upright, working-class, skilled worker kind of chap, completely ill-educated, as it were. ... My mother was, at one point, a waitress. They started work, of course, 13 or 14, and they met quite early on, and stayed together the whole of their lives. Father, I think, you could say was of a nervous disposition. Any rate, just before we were born, he had a perforated ulcer, so it was really a tremendous crisis, he was rushed into hospital with a haemorrhage....

When you say your father was a nervous kind of person, were there other manifestations of that?

No. Except in the War, as I recall, he reacted to the bombing. Well, it might have been to the bombing, it might have been, it might well have been pressure of work. There was a great deal of pressure because he was producing war material for the War Effort, as it were. But he had, at one stage, looking back, a vomiting fit, which was rather nasty, but that was all. But I saw that, and I'm doing it retrospectively, perhaps, as some kind of nervous response. You would say he was, perhaps, a bit highly strung. It's difficult to put that into words. He was a very very pleasant, nice man. And …

So how close would you say you were to your father and to your mother?

I would say not all that close. Now, in retrospect, going over things, they were highly anxious, well, my mother was highly anxious about us.... My memories are more of my father being somewhat distant and working hard, and not seeing him a great deal, I suppose. And my mother being nervous and protective, rather than that warm. (pp. 2-3)

2: Education and Class

In adolescence, as I grew up, there was a whole problem of social class difference as I became educated to some degree, though not that well! They [my parents] were a bit mystified. I went to grammar school. ... I don't think we had one book in the house. I think there might have been some self-improvement books they got through the newspaper or something, but there was, essentially, no books.

The 11+, I was just told, the day before, to bring a pencil and a ruler. So there was no fuss. I didn't even know [what it was about]. And they didn't know what the 11+ was about. And there was a large class, there must have been 35 or, you know, quite a large class in those days, this is in the War, and the teacher, I remember, said, "Stand up those who are going on to grammar school", and I think three boys stood up. And he said, "I don't know what the world's coming to. George Brown, you getting through". ...

But one was aware that there were these class issues. I was the only boy from my whole area to go to grammar school. It was, essentially, a working-class area. I mean that literally, there was no one - so I would walk to Kilburn, from Kensal Green, to get to school, but my memory is of hiding my cap on the way home, because it was, you know, not something … So one was brought up in a way, not that one would reject one's parents, but there was something about their way of life that wasn't quite you. (pp. 3-4)

3: Suicide in the Family

I was very isolated, very isolated in that sense. I think the only person I was close to at all, and I didn't see him very much, my cousin Frank was in the Air Force in the War, and he'd married a pretty secretary who had more education than anybody in the whole thing. And she was very nice to me.... She was very nice, and would come to see me. I wasn't intimate or confiding in her, but felt close.

Again, anticipating - one of the crises in my life was when she committed suicide, and I suppose this had got some influence on my research.... It must have been just about the

time I started research, when I was about 26, and it was post-partum depression, and everybody was ashamed. Her mother, particularly, was ashamed that her daughter should be [mentally ill].

I was very moved when [I found out] and felt that I could have done more, if I'd known what was going on. And this was all in retrospect that we learnt that her mother was [so ashamed] I don't think she'd even gone to see a doctor about it, and she went into - jumped in the canal, the Grand Union canal, and the baby was only a few months old.

This was important to me, intellectually, because although I partly gained my reputation from arguing the social causes of depression. I had no reason to doubt that it was a happy marriage. I'd be very surprised if it wasn't, because they were such extraordinarily nice people. And again, jumping ahead a long way, I've always been aware that you can't reduce psychiatric disorder to only social factors or psycho-social factors, you know, that one has to struggle with the fact that this was almost certainly essentially a biological response to the birth, although subsequently, in my research, I've shown that most post-partum depression is psycho-social. (pp. 8-9)

It was appallingly tragic, and what happened, [my twin sister] met someone from the neighbourhood who, again, was essentially working-class, but on the make, he was moving up after the War, and he'd got a job in West Africa, in Nigeria, [helping] running a large store. And every two years he came home for three or four months leave. And she met him at the beginning of one leave, and he swept her off her feet, as it were, and saying - he had a car, and, you know, he dressed well... he pressed all the right buttons for the family, good job, and dressed well and a pleasant enough man.... They got engaged, and the idea was that he would come back and marry her, and she would probably go out there, and she spent her time getting a trousseau together, and things like that. She had an essentially isolated life then, because she didn't go out with boys and so on, she was engaged. Then, not long before he was due back, he just stopped writing. And an appalling disaster. You know, I suppose part of my interest in life events might come from this. And she got very depressed, extremely depressed.

I was about 25 when this happened, 26. I was just about to begin research. ... But the psychiatrist [who saw her] felt that she needed psychoanalysis, of all things, and she couldn't have this, so there wasn't too much they could do. She was given barbiturates, and I think she got addicted to them, and a bit of a mess all round. She stayed in a so-called Neurosis Unit for a very long time, and did very well there, a very skilled potter and so on, very popular. But it took her years to get over the whole thing.

Of course, it devastated my family, I mean, this crisis, that she should attempt suicide, and it was a quite serious attempt. And in those days, of course, it was 'illegal'. And a policeman came to her bedside. (p. 11)

4: Bookshops as an Intellectual Opening

I was out of a job, and one of the big influences in my life is that I was in Notting Hill Gate, and I saw a placard on a second-hand bookshop, saying, "Boy wanted, for 30 shillings [a week]". And it was [run by] Peter Eaton, who became very [well known] later on, very rich … It was in Church Street, there was an old fish shop, and they would put the books on, throw the books on, shilling and sixpenny books, on a big [marble] slab. And I went to work there for three months.

It must have been one of the key experiences in that in some way, I began to get turned on. I met David Glass, in fact, who - people would come in. I must have been quite an interesting chap, because there was an inner office, and they would come and confide in me. So a prostitute came in, and a businessman, and so on, and they would talk to me. And David Glass, saying he was highly critical of America and sociology, and he brought in a lot of books on populations, he didn't really want them. And so that was a very important experience. ...

The other experience, at that stage, there was a Times Book Club, a wonderful bookshop, any rate, which has now gone, in Wigmore Street, and I would look in the window, and I remember I got Breasted's *Ancient Egypt* there, at great cost. It took a lot of my salary. And then - this is the key intellectual occurrence of my life, I saw a book by Will Durant, *The Story of Philosophy*, American, which I got. ... When I read this book, it just

transformed me. I actually started taking notes and I think it's the first time, and I was 18, that I'd been intellectually turned on. It quite transformed [me]. (pp. 14-15)

5: National Service in the RAF

I eventually landed up in a place where they had people interested in IQ testing, and so on. There was a training camp. It was in Hereford. And there was some psychologist there who'd taken degrees and so on, and I actually got very involved and made friends, (p. 16)

My first posting was where Thatcher comes from, where's that? Yeah. Grantham, which was an Officer Training Camp. And I was put in the Accounts Department, which I loathed, and I was horrified that, every morning, we would have to get dressed up, and clean our belts and brass, to go on parade with a band. And I was already a rebel. I didn't approve of this. And at one key point, I'd only been there about a fortnight, I was marched behind the flagstaff with two soldiers [airmen] either side [with rifles], because of the disreputable state of my equipment and so on! (LAUGHS) And I was obviously rebelling at the authoritarianism. And at a day's notice, they sent me to Hereford. They got rid of me. So I'd only been there about three weeks.

It was a very long torturous journey through England, and it had been winter, snow and the ice, and I remember arriving at Hereford. It was just incredible, because there was the office and so on, the Headquarters, and it had roses all round it! And there was a [retired] local farmer who'd [been looking after it] So I kind of landed on my feet. And it was a totally different atmosphere than this authoritarian [one at Grantham]… So the Air Force was a tremendously important experience, and I got friends that I kept for many years, and began to have a different perception of myself. (p. 17)

6: Industrial Operations Research Unit

Then, out of the blue, came a letter from someone in the Civil Service, in an Industrial [Operations] Research Unit, saying my professor had recommended me for a job with

him, which was an incredible relief, because things were getting pretty tense, to say the least! So I went into the Industrial Operations Unit, which was a fascinating experience, where we went round the country, really seeing how we could improve practice. And there were quite a lot of interesting people there, and very bright people....

This was 1955. People were aware of the inefficiency of our industries, so we were called in to study what was going on, for two or three months, and make recommendations. And what was amazing was the sheer amount of inefficiency, mind-boggling! But someone quite ignorant, like me, could go in, and show what they could do to change things.

So how were you studying them, though?

Oh, just observation, time and motion kind of thing, it was that kind of tradition, and we really observed what they were doing. I remember one factory, they were twisting metal rods you put into roads, and the whole factory would stop when they loaded up the lorries, because they had to bring the finished product across all the machines, to load on to the lorry. Amazingly, it's almost as though the whole thing had grown up [without much thought]. People get set in their ways, and they don't think of changing things. (p. 23)

7: Into Social Research in Medicine

I got this letter from Morris Carstairs, who was in the MRC Social Psychiatry Research Unit, though it wasn't called that in those days, at the Maudsley [Hospital], saying he was looking for an anthropologist, and my professor recommended me. So Darryl Ford was tremendously important. Although I turned down his offer to go to West Africa, he continued to support me.... So he was terribly important. And at that time, there weren't many people in research. There was hardly any research, social research in medicine, at all, and it was a kind of elitist system, in a way: now you'd advertise, and you'd get several hundred people coming in, I was the only one considered through a network of [personal contacts] …

So shall we now switch to focusing on that first major phase of research?

Yes. Well, I was so ignorant. I can't tell you the depth of my ignorance. I had to phone Harold Dale to ask him what schizophrenia was.

So the idea of working on schizophrenia, you were presented with?

Yes. Yes. They were concerned with chronic patients, chronic schizophrenic patients. At that time, something like 30 per cent of all admissions with schizophrenia, were retained in hospital, and on the whole, they didn't get out, once they'd been retained for two years, something like that, or a year. There was talk, at that time, even, of building more mental hospitals. ...

They were appalling wards. I was very shocked when I saw the disturbed ward at Banstead, it was really a madhouse. And there were wards of people [who had been] leucotomised …

Morris [Carstairs] was an extraordinary man. He was a psychiatrist and anthropologist. Tremendous social skills, but a very withdrawn person, who was getting on very badly with Aubrey Lewis ... I think he found Morris too soft on scientific [grounds], and not hard enough. And in the end, before too long, Morris Carstairs managed to move and set up an [MRC} Epidemiological Research Unit in Edinburgh, in Scotland.

I think we had a list of topics that, in part, came out of Morris's initial interview. It was, looking back, no great shakes at all! But they were such extraordinary interviews. These were all patients who'd been in hospital at least two years, and then been discharged. I mean, really, what you discovered, people wanted to talk about it, and they would talk at great length.

So one woman had, for example, the husband had been there 20 years, and was going up to see the Superintendent about getting a divorce. And she saw her husband coming out the boiler-house, and they got friendly, and she thought there was not much wrong with him, and arranged for him to be discharged. I mean this kind of thing. ...

These were very moving interviews, and the sense of, I think the success of what followed, partly came from just that down to earth experience of interviewing patients and relatives at some length. (pp.24-30)

8: Studying Mental Hospitals in Reform

John Wing was doing, I think, very important work, at that point, on social treatment and rehabilitation of patients. And, and we got together and agreed that we wanted to look at the influences [on and experiences of schizophrenic] patients, both inside the hospital and when they got out. What are their experiences? And we started with the Netherne [at Carshalton], which was one of the reforming hospitals. Dr. Freudenberg was Superintendent in Carshalton, and I spent a good deal of time there, sometimes staying overnight, just sitting around the wards, and going into the workshops. And I must say that was a very difficult experience, because nothing really hit me, and it was a slight crisis, because I was collecting [a good deal of] data, routine data, of how long people had been and what they were doing, and nothing was really hitting me....

I think ... there are two kinds of research. There's one kind of research where what you're interested in you can see happening. You can see how a nurse responds to somebody [who is] handicapped, or you can see how a mother is responding to a patient... I think a lot of qualitative work in the social sciences is [often] very, extraordinarily good work, done on this kind of level.

But I think there are many other key things that we're concerned with, that have much longer causal trajectories, and you can't get any confidence [about what might be going on] from just being present at one point in time. You see the end point, but it's very difficult to get any sense of what the hell's going on. So you have to turn to a rather different kind of comparative approach, that can take account of time, of things occurring over time. So although I got some ideas from Netherne, they were pretty thin on the ground.

[At this point we had a piece of luck]. I heard from Russell Barton, who'd treated my sister, and I got to know. Russell Barton was a rather charismatic figure in psychiatry, [who in his time] created a lot of positive and negative feelings. He'd taken over an old hospital, Severalls, in Colchester, and I'd kept in touch with him. And I suddenly [suggested] the idea, to John, `Why don't we go and study this place quickly?' [before much had changed]. And we arrived within a month or so of [Russell Barton's] going in, and they were still knocking down the big [walls], they were still filling in the ha-ha's - where they had the fences. They were still being bull-dozed, filling them in. So it was quite a dramatic time to go. That was a tremendous experience, because we were part of a revolution just beginning, and we really got a clear idea of what hospitals used to be like. By this time, I had managed to develop measures of a very down to earth [kind] - which nobody, I think, had thought of doing before - [for example] of finding out whether the [schizophrenic patients] had a comb, or a toothbrush, or what [else] they owned. And I got a nurse to examine every article of clothing of the women, and John interviewed the patients, in-depth, and I interviewed the nurses.

I'd been extremely sceptical of the whole area of attitudinal research, because I felt you had to feed in clinical data, [supplement it with] objective data about the patient. There was no point in getting [nurses'] attitudes to mental illness in general, you had to get attitudes about particular [patients] that you knew well and could [independently] characterise. And so this was a very important innovation, that we had detailed clinical, in the broadest sense, medical data about individual patients. And we had the nurses' attitudes about them. ... I also got very interested in time budgets, so we tried to get to the nearest quarter of an hour, how [patients] spent their time.

Also, in a very down to earth way, we had a measure of ward regime. It was of how controlling the ward was. And it insisted on getting down to the nitty-gritty. So, I remember one [question] was, "Where was the *Radio Times*?" "Did they have a *Radio Times*?" And I remember one nurse saying, "Yes, we've got a *Radio Times*", and then I'd say, "Where is it?" And she was sitting on it! She didn't allow it out into the ward! And it was this kind of very down to earth - trying to document actual things and possessions and activity, that, I think, [proved] important. ...

Then we went up to Mapperley in Nottingham, to do the third hospital, which was another reforming hospital. It's the main hospital in Nottingham. So we were able to compare the hospitals, and I think it's fair to say we had pretty impressive correlations between the state of the patient and these psychosocial measures. ...

And then, of course, the [important] innovation was to go back later, a year or two years later. And we eventually went back six years, so we could compare [clinical] changes, and changes in the social milieu. And I think it fair to say that psychiatry, as a whole, found our arguments reasonably persuasive, that the social milieu could be important. (pp. 54-56)

In this case, with this earlier research, you said, just now, how it was the experience of doing the interviews yourself, and almost feeling the emotion that led you to the hypothesis. Do you think that that has happened at other times? That the direct experience of field-work has been [the key to interpretation]?

Oh, I think tremendously important. ... Although we were considered reasonably senior, we did all the data collection. There were no hired hands in those days. It was all hands-on. And I think this is a tremendously important tradition. As you become more senior, it's more difficult to do this. But this early experience for really, 15 years or more, actually collecting data, was tremendously important. And I think the insights I got there really carried me through. (pp. 43-45)

9: Focussing on Emotion

I sensed, in a few seconds of going into a home, you could sense a tremendous tension, or more relaxedness. It was something that was almost palpable in the air. And this is what I became interested in. My interest in emotion, which I see as central, and where I think so much sociology has gone wrong, or not done enough, is to leave out emotion, and to see human beings in terms of [only] the spoken word, as it were. And I think this is a tremendous shortcoming, both of psychology and sociology, and psychiatry, for that matter. (pp. 36-37)

We started proper measurement, and it was based on the idea that when you [spent time with] a family, you could sense what was going on, by talking. And it was then I got involved, if you like, in paralinguistic elements of speech, that I sensed that what, in general, social scientists were missing, was the fact that emotion was the key system of meaning. And also, I might be reconstructing a bit, but that it's now clear, in terms of evolutionary evidence, that the larynx is tightly tied into the emotional system, in terms of development of speech and so on. But there's a direct link. And it's extremely difficult to not convey emotion in speech, because of the construction of the system - without going into details. And this partly holds for facial expression. ... I think there's a lot of redundancy in emotional display, but you've probably got more control of facial expression than you have of paralinguistic [aspects of speech]. Often you can pick up clues about, shall we say, criticism, hostility, by facial expression and also by speech. So you've got content [in terms of what is said], you've got the vocal aspect of speech, and you've got facial expression. (p. 50)

10: Expressed Emotion, as New Concept

When I interviewed parents, I sensed there was something else we weren't reflecting, which we called "emotional over-concern", which we rarely saw in wives or husbands [and for which we had not developed a scale]. But it was something, a kind of over-anxiety about the other person, exhibited in various ways, that came over. And this was added to the scales we'd produced with [Michael] Rutter. And to get a neutral term, I think I developed, I think it was me who [first wrote] about "expressed emotion", it might not be. I've forgotten where - But the idea was to get some [term] that was neutral. People have since objected to it, but it was trying to get away from the idea we were just interested in negative emotion, though this is what people have accused us of. But, of course, we did take account of warmth and positive comments and so on. (p. 66)

11: Interviewing

Could you explain how you developed the particular interviewing style that you used?

Well, two basic needs emerged fairly soon. One was to get as accurate an account as possible of things that were occurring. The second was to get people talking openly, and with feeling and emotion. I think I mentioned before that one of the things that became quickly apparent to me, was the importance of para-linguistic elements of speech.

What do you mean by that?

Well, that the voice is a way of conveying emotion, that there's a very close link between the larynx and emotional centres, which is not under conscious control... The voice is a tremendous giveaway, in terms of people's feelings and emotions, and much more so, I think, than facial expression. So if one gets people talking about things that are important to them, they will readily show emotion, both positive and negative. So these two needs came together, that the idea was that we would interview in a conversational way, to get people to relate stories about their lives, and details about what had happened in the previous week [and so], and willy-nilly, in these stories, they would show, convey emotion and feeling....

I don't think there's conflict [about these two needs in interviewing], but shall we say that the conflict that has [sometimes] emerged is how far we could take short-cuts, and do what most people do and ask direct questions.... I was surprised that quite a large number of these comments actually came up in response to standard questions like, "How do you see yourself?"... [So] I don't think one should exaggerate, too much, the point I'm making about spontaneity and coming out in conversation spontaneously.

But, at the same time, I think one's got to keep in mind that we were asking very searching questions, and the questions about, shall we say, motherhood, and how good a mother they were, was in the context of asking about 20 minutes of questions around motherhood. It's a sort of compromise....

in my teaching about interviewing, I say there are about ten tasks that you have to continually have in mind. And one is, have they spontaneously, as it were, answered this question, either emotionally or about a fact, beforehand? That's one of the ten things you have to be evaluating all the time.

What happens if somebody has a huge amount to say, which is partially relevant, and is going on at great length about it?

My experience is, it's not a tremendous problem, that given that our interviews can last three hours, and perhaps two or even three sessions, my sense is that people, rarely, shall we say, when they've got a huge issue, will want to talk more than 20 minutes about it. And there aren't [usually] that many big issues. ... But you must allow people to do that. It's not to say you don't necessarily, as it were, [eventually] gently move them on...

At that point, you would then be using questions which weren't on the interview guide?

Oh yes, very much. You want, for example, to know how secure somebody feels in their marriage, and how much emotional support they receive, how much warmth they receive, how much warmth they feel about the person, and all sorts of things. And there must be, shall we say, 25 ratings about the marriage, and you would have these in mind. You would try to get the person talking in a way that would cover these issues. You know, sense of security would come from [questions about] how regularly the husband comes home, whether he's predictable, whether there's a regular supply of money, whether he's suspicions about her faithfulness and so on. And you would have this idea of what you've got to cover. And, ideally, you would try to do this without asking direct questions, asking, say, whether he's predictable - at least in the first phase of the interview. You would rather ask about his coming home from work and so on. (pp. 166-70)

12: Missing Interviews

We had a policy of calling back 30 times. … We were very aware of what we might be missing if we had a high refusal rate, or failure to contact. And we carried on that tradition. I'm not sure how many times, for example, Tirril would call back. But the art of the business was to - not be bullying or intrusive, but if one sensed that the person didn't want to be interviewed, or was about to refuse, would suggest we came back later. And often people were happy with that. And if we … we sometimes would pop back, and

they would feel in a good mood, and the sun was out, and they would say, "Oh, come in", and we'd start. (p. 159)

You were implying earlier, if you didn't have a high rate of response, then you missed out particularly on vulnerable groups.

Potentially, yes. There is some, I think not from our work, but there is some evidence for that. It's probably not as bad as one might think, but there certainly is evidence. ... It's a fascinating issue, and I think I'd like to know more about it.

And my sense is there are two lots. Some that really are disturbed and don't want to be interviewed ... And certainly some of the most frustrating interviews are those where you do the first interview, we had to go back a second time, and it was quite clear that, at the first interview, the person was depressed and so on, and anxious. And then consistently, refused to see us again.

But also I sense there are some who are really quite well adjusted, and who can see the point of it all. So to some extent it might cancel out. ... In a patient series where you're following up, you can get up to 90 [per cent], with hard work. (pp. 161-2)

13: Conflictual Memories

What if you listen to a story about something that happened, and you just don't really believe it?

It's an interesting question. I'm trying to think of interviews where this has occurred. I don't know. I don't think it's a feeling that I have that often. I'm not saying it doesn't occur. This does happen, but I think it happens particularly, we find it can happen when we're dealing with sexual abuse and physical abuse, and issues of rejection and scapegoating. ...

And the answer is that we would try to stick to actually what occurred, but not convey shock, we'd try not to convey any [negative] emotion about nasty things that are recorded

and spoken about. And certainly it's not uncommon to sense this discrepancy between current feelings and and what is being talked about. You can have amazing discrepancies of a woman who's describing the most appalling childhood, and at the moment how she still wants to [remain] close to the mother. And there is also another element, that there is some suggestion that the ability to work through and forgive parents, can be very important and therapeutic. (p. 173)

14: Against Transcription

No, we never had the resources or the money, although we didn't have the money [to do many], we got some [tapes] transcribed: transcribing on the old typewriters. But the key experience was not recognising an interview [I had done when I read the traanscipt - because something critical was missing] ... when I read it. Because so much depends on, for me, the tone [of voice] and so on. This really confirmed, fairly early on, that … just what people said wasn't just good enough. The content [of what is said] only takes you a certain way. (pp. 51-52)

15: Discovering Statistical Analysis

I had a second Eureka experience! And that was just by accident. In the library next to Aubrey Lewis's room, pretty well, they had a small library and a small section on the social sciences, and I came across a book with a strange title, on *Continuities on the American Soldier*, edited by Merton, as I remember. And there was a chapter in there by Lazarsfeld and his wife on analysis of data, which bowled me over, intellectually. It completely changed everything. It was - I think, without that input, which was completely by accident, my life would have been totally different, in research.

Well, the main point was that by analysing data, you could test hypotheses about antecedent variables, intervening variables and all the rest of it, by cross-tabulation. And this, the logic of it, just bowled me over. I immediately began to think of the data in this way. And I found it tremendously exciting, intellectually, because it was possible to bring vague ideas that one had had, and also kind of serendipitous things that are coming out of

the data, and what was so exciting was that you could pursue a series of hypotheses and generate them and test them, and go on, and it was truly exhilarating. (pp. 32-34)

But, at the same time, I'm still pretty [suspicious of] the computer, in an odd way, because I feel that people who simply work with these marvellous computer programmes, [and produce] incredible statistics, they do things in a few seconds, that in the old days, we would take three or four days over. That is marvellous. But I think if you stay within that, you tend to not know your data, and all sorts of very odd things occur. (p. 181)

16: The Debate over Logistic Regression

When we started publishing on depression, it became a battlefield. I mean, John [Wing] came out strongly against our work on depression... Quite extraordinary things happened. [Paul] Bebbington, for example, who was employed by John, after we'd published our key papers on depression, published a famous paper in *Psychological Medicine*, using a new statistical technique, logistic regression, which we knew nothing about, literally. And he, in so many words, just said, "You've got it all wrong". There wasn't interaction in our data, statistical interaction.... But the editor, Michael Shepherd, sent it to us and said we had two weeks to reply. Two weeks! And I think the paper we replied is not perfect, but pretty impressive, when you realise we didn't know anything about [the core statistical technique he used.]

Basically, I mean, looking back, there are two kinds of interaction. There's additive interaction, and multiplicative interaction, and it's a very complicated issue. And it still is a tremendously complex issue. A lot of the criticism of behavioural genetics is that they're not taking account of interaction in the way they should. I won't go into technical details. ... But the long and the short of it was that logistic interaction only deals with multiplicative interaction, not with additive interaction. (pp. 92-93)

17: Developing Measurement Systems

I thought that the whole [family] measurement system had to be improved, and we had to know more about the families in general. [Michael Rutter] had money, quite a lot, I've forgotten where he got it from, and I persuaded him, or he persuaded me, or we jointly persuaded [each other], that we should spend a year or more, on developing a new measurement system. Now, I think the thing to get over - and the thing I'm highly critical of in modern research - is my experience that [effective] measurement systems take years to develop. And one of the problems is not only is this not recognised, it takes so long to develop, but [the existing measurements are] often geared into so-called hard science, the questionnaires, so you can't change them.

All the measurement systems I've produced, they've been flexible, in the sense we've changed them over time, because they weren't based on standard questions. I need to get into this, because this is vital to understand the work I've been involved in. I've got tremendous antipathy towards the whole tradition of questionnaires, which I think have an appalling influence - wishful thinking involved in the creation of things that don't exist.

What do you mean, when you say "wishful thinking"?

Well, that [researchers] show people these questionnaires, which they sit at home and create, or in the office, and they then pretend they reflect something. But [often] they've got no intensive work to back that up. All they've got is statistical manipulations of the data they produce, and correlational evidence later on when they've related something else.

There's a whole area of quality of life, for example, at the moment, that I find largely meaningless, which is constructed on asking amazing questions of, "What is the quality of your life?" kind of thing! So I think that the questionnaires often are rather silly, but the point is that they're not backed up by intensive work that would generate some insight into their validity.

I can also go on to say, I think questionnaires can play a valuable part in prediction. ... Some questionnaires work. But without that background material, intensive interviewing to back up, I think they're wishful thinking, on the whole, and deeply misleading,

potentially.... One of the things I will emphasise about what I see as important [for] measurement systems, [is] that they should be allowed to grow [over time], in the light that you should be able to have various versions, editions. And all the measurement systems I've been involved in, have been of this kind, the clinical and the social....

The philosophy was that we would try to make the interview - it was collecting detailed information of what went on in the family. And the general idea was that when people spoke in detail about their lives, they showed emotion, willy-nilly. It comes back to this whole issue of the larynx and vocal aspects of speech.... The idea was that two people would always go on the interview, and we would rate what we saw. ... Because the really genuine insights came when you disagreed. And this was terribly important insight. So we encouraged disagreement. (pp. 59-61)

18: Developing Hypotheses

Here I think, [something] Morris Carstairs told me [was important in] the first study we did, [which] was a rather crude study in South Wales. I remember, on the way down there, we talked about research and so on, and he said, "George, the important thing about research, medical research, is you've got something to aim at". And this is what's always, I've found so terribly important in the research in medicine, you have a clear dependent variable, and also one that is publicly seen as important. So you can bring research money to bear on the problem. But it was having that dependent variable which was whether they stayed in or returned to hospital, and with a florid relapse or not, that enabled me, I think, to generate hypotheses and test them, and pursue them.

I found this terribly exciting. To give you an idea - I'd sensed, early on, that there was something about the family climate that might be involved in the relapse. It was a pretty long shot. What turned out was that patients going to close relatives, did worse than those going elsewhere. Or to brothers and sisters, [where] they did much better. Then it was possible to test a series of ideas about what was going on. So, for example, the patients who'd been violent or difficult before admission, did worse than [others] once they got out, and this was often ten years later, than those who hadn't. So then I said, "Well, what happens if they went to another home they hadn't been at?" That sounds pretty crude

now, but it was quite exciting for me. And lo and behold, if they'd gone to another live-in group, their rate of relapse was no different from anybody else's. And it was this ability to generate these ideas. (p. 34)

But what we've agreed upon is that research is a series of stepping stones, where you gradually push into the causal mechanisms that are involved. First of all, you start with crude prediction. You find out there is a problem. You get some crude predictive indicators. But then you've got to move, as Mike Rutter would say, into mechanisms. And that's a tough thing to. It's a very tough step to take. (p. 43)

19: How Ideas Develop: Measuring Life Events

Can you tell about how the idea came into your mind, to do this?

A difficult question, because not a great deal was known. I think my main reason for doing it was to move into the general community. I think I'd made up my mind that depression was a pretty central notion, which would span social science and psychiatry. It came very close to ideas [such as] anomie, demoralisation and so on, but it would give an opportunity to get something pretty hard around these woolly concepts, notions of happiness and all the rest of it.

In my own personal life, I'd come across examples. I'd come across a good deal in studying schizophrenia, some of whom weren't depressed, and the relatives were depressed. And I'd had a certain amount of clinical experience at the Maudsley. But what I found exciting was the idea of moving in and trying to characterise a community, a population in these terms of [such a condition]. There was a very interesting book on the Hutterites, I remember, at that time, which were a rather isolated religious sect in America, which, apparently, had very low rates of depression.

I suppose it was coming back to my background in anthropology, and I was also, of course, very much involved in the issue of social class... And I was amazed at the lack of any insights whatsoever, it seemed to me, in what were producing these class gradients, just simply speculation.

And I wanted to pursue the work on life events, in a different setting, because it was quite clear that they were an important way of beginning to establish something causal that was quite difficult to refute, because you had time order and all the rest of it, and as long as you could get the dating of onset and so on - I think it was only as we began to collect data, actually, that I began to become involved in some of the more theoretical measurement issues.

And because we got a lot of descriptive material, I could think around issues as the data was being collected and rated. First of all, I was very turned on by the work of Rahe and other people, on life event scales. And this, at the time, was the only research going on in life events. And it largely consisted, as you probably know, of ticking off [questionnaire] items, and arriving at an overall score.

This really came out of work by Selye on stress, which really, the bottom line, I think, was that stress was not something to do with meaning, it was to do with habit change, change in behaviour. So they would [give different scores to] going on holiday, changing job, moving house, and having a divorce, they were given scores and [then] added up. But it took me some time to realise that this was a disastrous approach. So from being enthusiastic about the fact that correlations were emerging with onset of physical disorders, I was increasingly shocked by what was going on. ...

I was increasingly aware that it didn't make sense to deal with life events, if you ignore the context. That a change of job, or a broken love affair, was not a change of job or broken love affair. I mean, [as such they are] almost meaningless. A loss of a job would mean entirely something different if you were on a short term contract, or you were well educated or if you lived in an area of high unemployment. I'm saying the obvious. Or a broken love affair could be an entirely different meaning if you'd been insulted, and the chap had said he no longer loved you, than if it was a short term affair …

So it became obvious to me that one of the tremendous shortcomings in much of research in social science was, they would give lip service to the issue of meaning, but they weren't really dealing with meaning at all. On the whole, you can't deal with meaning

unless you can relate it to plans and purposes. So this was the view I gradually developed. And this ran counter, dramatically, to what was going on in the field. (pp. 104-06)

So when did you develop the measures for the life events?

There are a number of key problems in measuring life events, the first problem is what you include as a life event. And the first stage was simply concerned with that. We had to go through hundreds and hundreds of life events, and to establish what goes in and what goes out. And our definition was that, on common sense grounds, they should be capable of creating significant emotion. So we had to develop standards of doing that. And we [produced] a 60-page manual. (p. 67)

20: Measuring Entrapment

We developed a measurement system where we [first] rated [severe life] events in terms of either humiliation or entrapment [and only, if not these, in terms of other meanings like loss.]. The idea was that, really, the central experience [for depression] was not loss, but a sense of being humiliated and trapped in a situation. I won't go into the details, except to say that it came out that while most events that produced depression, were loss events, [it was usually] a particular kind of loss, typically to do with humiliation - like that woman who was put down by the teacher, or an event occurring that confirms you are trapped into a situation that's highly punishing. ...

So our entrapment partly comes out of our culture, our role system. A mother can't [readily] give up [mothering] a baby that she hates. We know that forty per cent of mothers in London don't [much] enjoy [their role of mothering] their children at all. They [usually make clear that they] love them, but they really find dealing with them very very difficult. And you can't [easily] just opt out of a role like that, not in our present culture.
…
This idea of what it is the meaning of life events has been progressing all the way through. And what I think is exciting, is that this result was repeated in a patient series, and also exactly repeated in a study of black women in a township in Harare, Shona-

speaking women, who had a high rate of depression, but [this was] in good part, [provoked by] these humiliating and entrapping severe events.. [The emphasis of the idea on hopelessness is] not particularly original. I mean, it's difficult to come across an idea in this area that someone hasn't had. The challenge has been to produce evidence that would enable you to begin to support it or reject it. (pp. 117-18)

21: The Need for Long-term Research

Once I got the ideas that the patients were peculiarly susceptible to emotional arousal, which is the basic insight, then this would generate predictions within the data. So one of the most interesting, looking back, tabulations, was finding that patients who didn't work, had a much higher relapse rate, which, obviously, could pertain to their handicap from the symptoms and so on. So I argued that there would be a difference, in terms of whether the mother went out to work or not, just taking those [living] with a mother. The bad combination was when they were both home together. But if the patient went to work, things would be all right. If the mother went to work, and the patient stayed at home, it would be all right. And if they both went to work, it was all right. And this is exactly how it turned out.

Now, of course, the snag in all this was that, although it was very exciting, you could always think of alternative ways of interpreting it. That the mother didn't go out to work because she was worried about the son and so on. So there came a point where you couldn't go any further. And I think I've argued elsewhere that it's a bit like backing a horse, research, that really what you're doing, in most projects, is to justify, try to justify further research in pursuing a problem. It's not about getting at truth, or whatever that is. It's about what horse to back....

I'm not saying you can't ultimately get at the truth, or you can get somewhere near it. I think, in the kind of problems, as social scientists, we tackle, they are so complicated that it's almost arrogant to think, in one project, you're going to get any more than some glimmering of where the answer might be.

I think one of the problems today is that people have got a completely wrong-headed idea, I think, of research, and they expect it to be done in two years and coming out with a definite answer. I think this is leading to a lot of trivial research, which is really, basically, unconvincing, or not breaking new ground. ... So I don't think it's cynical or sceptical at all, I think it's facing the true complexity of what life's about, if you can't experiment. And I think where human beings are concerned, you might have to take 20, 25 years, before you can afford to experiment. (pp. 34-35)

22: Controlled Trial Experiments in Socio-medical Research

Tirril and I decided, six or seven years ago [c 1993], that we would experiment, and we would take chronically depressed women [living in Islington], and actually randomly allocate them to befriending, getting other women to come in and befriend them. And this is the only time that we've done it. It was an extraordinary enterprise, in a way, because in order to by-pass this reluctance, we actually had to set up the service ourselves... We met a certain amount of political correctness. For example, at one point, the social workers we'd appointed, wanted to ignore the issue of gender, and allow men to befriend some of these women. And it was obviously -

You thought that might produce florid results!

Well, these were depressed women, and it could have led to difficulties! (LAUGHS)

But as I see it, in broad terms, that all our work has been moving towards the issue of experimentation. And we have, in fact, recently put in three grants, in order, in fact, to have controlled trials [based on ideas] coming out of our research. We've run into difficulty here. We put in one to the MRC six years ago, which I thought was very good. The basic problem has been that we felt that where depression is concerned, it's not like treating cancer or something like that, where you can have one treatment and, quite rightly, find out whether that's having an effect or not. Depression is far too, it seems to me, complex a condition, when seen in the community, to just offer one thing. And what we wanted is to offer a range of things, according to the circumstances and the wishes of

the woman and so on. And this ran counter to the idea of the MRC Committees, that you wouldn't then be able to know what really was important.

But, fortunately, in the last eighteen months, there's been a change, a change in opinion, and people are increasingly coming around [to the idea] that, for certain problems, that [more] complex control trials, random control trials, are highly relevant. And we're just submitting another fairly large application to do something in general practice along these lines, and we'll keep our fingers crossed on that. In fact, Tirril sent off the initial application last week. But this is to make the point that we, at the moment, Tirril and I, after 30 years, really feel, very much, that most of our future work should be in terms of some kind of intervention and experimentation, not only because, ultimately, I think, at one level we've always wanted our work to lead to something, but also, that some of the issues are so complex that, even in scientific terms, we can't at all be sure about certain things without this kind of experimentation. (pp. 74-75)

23: Replication

What do you think, looking back, you're most proud of, as a researcher?

I suppose, looking back, the expressed emotion measures were quite something, because that was, I think, breaking new ground. And I'm proud that [all the main findings have] been replicated. I suppose, yes, it comes to producing things to be replicated, actually, because I've always seen that as the heart of science, that replication really avoided much of what people worry about. They're worried about carrying out too many tests of significance, and correcting for [this and] that [and so on]. And I think just the mere fact of replication, with a different team, and a somewhat different way of looking at it, is crucial. The fact that most of the work has been replicated by other people, and also across cultures, that, I suppose, would be the - because so few people ever bother to try and replicate. And, as you know, things tend to go into fashion for 10, 15 years [or so], and then go out of fashion, and they're not seen again. And that's partly, I think, the measurement systems are partly [a matter of] fashion and fad. (pp. 151-52)

24: Sampling for Mental Illness in the General Population

You moved to a study in depression, and then you did a broad cross-section. Is that right?

Well, there, we did two things. We started off with a patient series, and we needed a comparison group. And we stumbled upon, and I'd always wanted to do it, but it was partly the need for a comparison group that we took a random sample in Camberwell. And what was fascinating there was that what appeared to be, at the time, a very high rate of depression, in this normal [general population] sample, and this came as somewhat a surprise. Because at that time, I think it fair to say that we were the first group, I think that's probably right, to use an accredited psychiatric face-to-face interview in the general community. We were certainly one of the first …

The Present States Examination, which was one of the key instruments to be developed [by John Wing and his colleagues - we were] probably one of the first, certainly we were the first to use that [on the general population]. And there was a good deal of scepticism at that time, about the rates we were reporting. A great deal of scepticism. And so we had the initiative, or courage, to do a population study, [partly] because we needed a comparison group. In this rather odd way [we started] and then we never really looked back. We got so involved in the issue of depression and anxiety, and psychopathology in the general community, that we continued. (p. 157)

25: Resistance to Innovatory Ideas

When, for example - I'm anticipating - we [wrote up] our work on life events and relapse in schizophrenia, the *British Journal [of Psychiatry]* editor said he didn't believe it, and refused to publish it. And it's now a classic paper. So I think I hadn't spelt out the context in which I was working, that there was tremendous scepticism about the role of psycho-social, social factors, at all, in these conditions. And the most that I think the zeitgeist would have [accepted] at the time was that it could influence the course their condition took, and that was really the perspective I was taking, first of all, that the course of disorder could be influenced by psycho-social factors.

And you had a hunch that they were going to be important, yourself?

No, not when we did that work to start with. I was completely - I didn't know anything about the condition. I think it was only, basically, inductively, when I saw these dramatic differences in relapse coming out of the computer, if you like, the Hollerith machine, that that I began to draw on my experience of coming into a home, and feeling the tremendous tension that at times [would be there].

[I had encountered] a whole series of examples like this, where stress, or change in habit, seemed to be important. Now, I got very involved in this. And what was fascinating, there was a time order. These always occurred in a week or two of the florid relapse. …

It was the beginning of the life-event research, and I carried out a study, a small study. And with, to me, unbelievable results. It was so clear that they were important in a number of these onsets. And I waited six years before I published that, because I felt people wouldn't believe it. And ultimately, I did a larger study with Jim Birley, which was published, although initially it was turned down by the *British Journal of Psychiatry*. So I had this sense, all along, that people weren't going to believe what I was finding. (pp. 36-37)

We sent the paper to *British Journal of Psychiatry*, which simply [rejected it] … psychiatry was very authoritarian in those days, you wouldn't do that sort of thing now, but [the editor] conveyed he didn't believe it. So they didn't reject it on scientific grounds, but the fact they didn't believe it. The world changed a bit after that! (p. 68)

I don't want to give the wrong picture, but we did, at times, feel under siege. … We started, I think, the research in 1968, and [for some reason] it was a three and a half year grant, and after [around] two years, we put in a report to the MRC, which essentially, we worked extraordinarily hard, gave the basic results about depression and life events. And the MRC did something I think they've hardly ever done before, they just wrote to say the grant had been terminated, that we couldn't have the last year. And with the implication that there was something very wrong with our research. It was [Martin Roth], a very

[well regarded] psychiatrist who was Chair of the Neurosciences Board at the time. And that was pretty shattering … Certainly, his views, published later, were highly sceptical about generalising the notion of depression in the way we had. But whether he played an important role in [the decision], I don't know. I never really got to the bottom of it.

But it was a hell of a crisis, because we hadn't finished the research, and we'd got very interesting findings. Luckily, I phoned David Mechanic, who was a tremendous support, and he found a Foundation, the Federated Fund for Research in Psychiatry, in the States, and they came over and interviewed us - there were some quite famous people, Neil Smelser was one of them. And they gave us money, to finish [the research]. But what I'm getting at, is that we were pushing at the limits, in some way, intellectual limits, at that time. (p. 94)

26: Laing and Anti-psychiatry

Luckily, I've never gone in for the blandishments of the radical sociology [of the anti-psychiatry movement], which I think are appalling. I've always accepted there's a genetic element to schizophrenia. When I met R. D.] Laing, who came to the Unit, I was shocked by his self-serving [dogmatic ideas] - a very bright man, but completely wrong-headed. And I could see that, although he came in 1957 or so, very early on, I could see how [misguided] he was. And he, of course, had a very bad influence on sociology and psychiatry …

What do you mean when you call him "self-serving"?

Well, I think he was imposing his own experience, and he'd had psychotic-like experiences, and I think a part of his marvellous insights, and those early books are marvellous books, is coming partly out of personal experience. He's a very bright, sensitive man, but in the end, I think he was trying - my interpretation, he was trying to make his own experiences more comfortable by putting them in a certain framework.

And it was nonsense. I mean, basically, they were saying two things. They were saying you could pick up schizophrenia like you could pick up French. It's a stupid notion! And also there was an element of saying it didn't really exist. Which was an appalling position, from which we're still suffering ; [it's still possible to come across vestiges of its negative impact on sociology]. (p. 46)

27: Gender and Feminism

So how did you feel about later feminist scholarship in this area?

In a curious way, they weren't very pleased with our work. I don't know why. I mean, one of my negative memories, is talking about [the Camberwell results] at one of the first feminist meetings, I think it was, and [some] people [in the audience] getting up and shouting me down. But I think, looking back, it was more because I was a man talking about women, than really about the results as such. I think the Women's Movement is terribly important and significant and all the rest of it, but I think, in terms of scholarship, I've always found it rather suspect. ...

Now, I'm almost certain that cultures can push [such tendencies] out of all recognition, to some degree. ... But the idea that there aren't these [basic] differences to be played around with, I found, frankly, rather odd, [highly unlikely]. And to ignore the central role of hormones to do with breast feeding, and the crucial role women have had in trying to cope with children... These are very central issues that one has got to grapple with. (pp. 122-23)

28: Difficulties in Studying Men

I think, often, they don't behave that well within the family, and one senses that they don't particularly want to talk about family life and what's going on. Also, I think they are less interested in talking about themselves in an intimate way. Some, that you're most interested in, are down the pub, or where you will, or working away from home. They are just more difficult. (p. 162)

Women have this, I think, amazing ability, which I think [may be] partly genetic, of recalling almost blow for blow, things that happen in their lives, which I think men don't have. [With men] I would have much [less] detail with gaps in my account. (p. 62)

One sensed that, in a study we did of couples, where we particularly wanted to look at male/female differences in depression, very important. We screened couples, sending a letter to both, whether they would be willing to collaborate in the work. And the sense there was that, quite often, the woman would be willing to carry out the interview, but the husband, or partner, would refuse, and this was really quite a problem. (pp. 163)

29: Tirril Harris

Tirril has been extremely important. In fact, if I hadn't done work, good work, with schizophrenia, I would [have] doubts about my work with depression. You know, we worked so closely that I probably would have got in quite a muddle of how much was me, and so on. And certainly, I think, I probably - certainly have got more credit than I should have, in terms of the contribution of Tirril. And Tirril is a terribly important figure in my life.

She's bright and capable. Tremendous energy. Tremendous memory. And also she brought, totally … invaluable, clinical experience. Her mother was a psychoanalyst, [Tirril]'d worked as a clinical psychologist for a year in York, and had the kind of clinical experience I hadn't had, and gave me confidence... I asked Tirril to join me in writing [*The Social Origins of Depression*], and never looked back. She had been working with me? Yes, as an interviewer. And from that point, we henceforth worked very closely together. Tirril has been extraordinary loyal, and, in a way, people such as myself, males who have done well in research, I think, often need someone like Tirril. Shall we say it's very useful to have someone like Tirril, who is extraordinarily self-effacing and modest ... She's had a great deal of difficulty in publishing, and in spite of strenuous efforts, I never got her to do a Ph.D., although she had the material.

So, in a curious way, I think we've needed each other. I think Tirril, I think I can safely say, has enjoyed, tremendously, working with me, and working over the years got a great deal from it, and I've got a great deal from Tirril, too.

This is a very long, close relationship, isn't it?

Yes, I think it must be - well, since 1969, something like that. Almost married, in that sense! And I don't think we've ever really not got on. ...

You've used the analogy that it was like a marriage. What's the crucial difference?

Yes, I suppose a marriage in terms of intimacy and support, and seeing so much of each other, I probably see more of her than she does her husband, in terms of hours - well, that's exaggerating! But I do think, looking at the field, that women often have taken these kind of roles. If you take Leff and Vaughan, for example, who went on to repeat our work, Vaughan was very much a loyal data collector, full of ideas, though you might argue that Leff took too much of the credit, true as Brown has probably taken too much of the credit. (pp. 100-02)

Could you say something about how you relate to attachment theory, and when you first were interested by it?

I think this is pretty largely Tirril. John Bowlby had been very interested in our work [in Camberwell on depression], and he quotes us in his third volume, I think. It's critical evidence for the importance of early loss at that stage, so he was very interested. I don't think he'd be less happy with our amendment of that.... So it's a bit difficult to say where it really came from, but I'm sure it came largely from Tirril, who, very early on, was interested in notions of abandonment and feelings of abandonment and so on.

Lack of care was just an early version [of our own current measures of neglect and abuse]. It's certainly, I think, a very important achievement of Tirril. One of the risk factors in the early Camberwell work, was loss of mother, and particularly by separation. So Tirril did a large study in Walthamstow, where she took women with early loss of mother, [by

death or] separation. And I think it was a key bit of work, because, really, it goes back to the issue of loss. What came out of it, fairly clearly, [was] that almost all the effect [in terms of adult depression] was due to the quality of care after the loss of mother, rather than the loss of mother as such. (pp. 121-22)

Obviously, Tirril is ... so close and intimate, I can't now conceive of working without her! And Tirril's been central, … always remains critical and open and amazing. I think I conveyed last time, the tremendous importance - one, of her intelligence and stubbornness about issues; the other is her clinical insights from years of psychotherapy. (p. 188)

30: Controversy as a Stimulus and Pleasure in Research

Certainly, when we started publishing on depression, it became a battlefield. I mean, John Wing] came out strongly against our work on depression. Very strongly. And argued against it, very strongly. And it produced a tremendous intellectual crisis, going back over that time. It was really quite fun, but quite extraordinary things happened. (p. 92)

What I want to try and convey was, that it was a very exhilarating time. We fought back quite well. I think we quite enjoyed it. I don't want to complain too much, because one thing I want to convey is, one of the good things about working in medicine, is this opposition. I think one of the doleful things, melancholy things, about social research [in general is that] most people ignore it [even in the discipline]. And it is this sense of taking on people that is so interesting and rewarding. And it's one reason why one raises one's standards, one becomes concerned about all sorts of intellectual issues to do with bias and method and so on, is because you've got to meet this challenge. (p. 96)

31: Research as Sculpture

I still like to go back to certain artistic elements in my nature, particularly, with carving a sculpture. I think it's because I wasn't very good at it, that I often didn't know [very well] what was going to emerge until I got fairly deeply into the product! And then there would be an interplay as I saw something emerging. I could interact with it, and something good

might emerge. I find research very much like this, that I don't know, very often, what's going to emerge, or I've got a very dim idea.

And the idea that you [must] have clear-cut hypotheses and tests and so on, I don't think it's how most research works really, [or how] productive creative work [emerges]. And it is this sense that there's something there, like in the wood or stone, and by interplay, by looking at what's emerging, and feeding more in, it's this that I think I was trying to get at. (pp. 183-84)