Law and Social Enquiry: Case Studies of Research

Edited by Robin Luckham

Scandinavian Institute of African Studies, Uppsala
International Center for Law in Development, New York
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Robin Luckham

Scandinavian Institute of African Studies, Uppsala
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"genuine blasphemy... is the product of partial belief, and it is as impossible to the complete atheist as to the perfect christian".

T.S. Eliot (Selected Essays, 1950: 78)

So mak’st thou faith an enemy to faith,
and like a civil war set’st oath to oath,
Thy tongue against thy tongue.

Shakespeare (King John, III, 1)

Myself when young did eagerly frequent Doctor and Saint, and heard great argument about it and about: but evermore came out by the same door as in I went.

Omar Khayyam (Rubaiyat no. 27).

Induction

A paper on one’s own research is also something of an autobiography and self-portrait. Literary hermeneutics distinguish between autobiography and self-portrait. While the former narrates “what I have done”, the latter narrates “what I am” (Beaujour, 1977: 448). They have also different time structures: diachronic and synchronic. Though the distinction seems on the face of it clear it is indeed very complex, much beyond the commonly acknowledged fact that what you do portrays what you are. One can say that the self-portrait writes the autobiography. What I am is in a sense the last chapter of what I have done, but a last chapter which is contradictorily present in writing all the previous chapters. Saint Augustine was very much aware of this problematic as he opposed what he had done to what he was while writing the Confessions (Saint Augustine, Bk. X, Ch. 3).

In writing the present paper I have tried to keep inside the autobiographical model whilst remaining aware that the temptation of self-portrayal is intrinsic to it. This raises the question of this paper’s specific constitution: literary or scientific? Further, this question raises a much broader issue about the relations between social science and autobiography or, even more generally, between science and literature. Both literature and science transform empirical facts into artifacts. Clearly enough the literary construction of artifacts differs from the scientific one and the difference has been much emphasized, particularly since the scientific revolution of the sixteenth century. It should, however, be borne in mind that such difference is based upon an equally crucial similarity, that is, the fact that both literature and science possess constructive structures to build upon the “factual”. With the increasingly
visible crisis of the scientific paradigm we have probably reached the time to emphasize (and to clarify) similarity rather than difference. After centuries of obfuscation this is not an easy task. It is probably easier if we start by studying the borderline cases, that is, autobiography in relation to literature and social science in relation to science.

The literary constitution of autobiography has been much disputed in view of the relative predominance of nonfictional (empirical) elements in it (Renza, 1977: 1). On the other hand, in the positivistic tradition the scientific constitution of social science has also been disputed in view of the relative predominance of fictional (personal, political, value-laden) elements in it. By itself this positional similarity does not clarify the structural similarity but it shows how the "ideal types" of literature and science developed respectively by literary theory and by epistemology have probably left out mixed entities in which literary and scientific elements merge together. It is conceivable that one among these mixed entities is precisely the autobiographical essay on one's scientific history. Here, in the borderline of the borderline, the mixture of elements may reach such a complexity that it constitutes a tertium genus between science and literature.

Whether or not the present paper succeeds in this respect, I would emphasise the importance of developing an autobiographical method in the social sciences as a way of testing new answers for questions that are common to both science and literature: such as the relation between truth and design, between memory and invention, and between description and imagination; the question of time structure; and finally the question of the author. Besides, the development of such an autobiographical line could lead to the emergence of new styles and types of self-publication; to syncretic/synthetic forms in which scientific and literary expressions converge. In the following pages I will raise some topics for discussion based on the "autobiography" of the present paper.

I wrote this paper somewhere between memory and invention and yet I was always aware that these two extremes are at one and the same time one single site from which one has to exile oneself in order to be able to write. Indeed neither memory nor invention offer a secure shelter for a writing enterprise of this kind. Memory is full of dark holes to be flown over by the wings of imagination. Kafka was acutely aware of this when he wrote in his Diaries (1910–13):

In an autobiography one cannot avoid writing "often" where truth would require that "once" be written for one always remains conscious that the word "once" exploded that darkness on which the memory draws; and though it is not altogether spared by the word "often" either, it is at least preserved in the opinion of the writer, and he is carried across parts which perhaps never existed at all in his life but serve him as a substitute for those which his memory can no longer even guess at.

On the other hand, self-invention, if authentic, is never arbitrary, is the memory of memory, the reconstruction of a melted away memory. As Saint Augustine puts it:

I say "memory" and I recognise what I mean by it; but where do I recognise it except in my memory itself? Can memory itself be present to itself by means of its image rather than by its reality? (Bk. X, Ch. 15).

According to Renza (1977: 9),

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(. . .) a given autobiographical text normally manifests the writer’s spontaneous “ironic”, or experimental efforts to bring his past into the intentional purview of his present narrative project. The autobiographer cannot help but sense his omission of facts from a life the totality or complexity of which constantly eludes him, the more so when discourse pressures him into ordering these fact. Directly or indirectly infected with the prescience of incompleteness he concedes his life to a narrative “design” in tension with its own postulations the result being an autobiographical text whose references appear to readers within an aesthetic setting, that is, in terms of the narrative’s own “essayistic” disposition rather than in terms of their nontexual truth or falsity.

I would suggest that this problematic is also common to science and particularly to social science. Indeed “prescience of incompleteness”, the “sense of omitting facts” is the original matrix-phantom of scientific research. Such prescience, though suppressed or explained away for a long time, has been the major force behind the struggle against the positivistic conceptions of science, from Kuhn to Bachelard. The notion that scientific truth is always paradigmatic truth, that is, in the last instance, conventional truth or the notion that facts are manufactured, points to the priority of theory in science and such priority is the structural reverse of the prescience of incompleteness. The theory is the ever missing decisive fact.

In any case, what begins to emerge is that, similarly to the autobiographical text, the scientific text is constituted by a set of references that appear in a specific setting (a scientific setting), that is, in terms of the scientific narrative’s own “essayistic” disposition rather than in terms of a nontexual truth or falsity. In these precise terms it is conceivable to view all science as science fiction, or rather, as reality fiction.

A further exploration of this topic would probably lead to the conclusion that the scientific text’s specific setting is not monolithic. On the contrary, the relation between the act of signification (the text itself) and the object of signification (memory, reality) varies inside it. The instances of tension between the two are not equally distributed throughout the scientific narrative. Accordingly the distance between the “fictional” and the “factual” may vary widely inside the same text.

For more than one reason the prescience of incompleteness does not exhaust the question of the factual/fictional value of the autobiographical or scientific text. Firstly, to speak of omission of facts is probably inadequate since it presupposes the possibility of a total coverage of the author’s past or of the reality he studies. Indeed, the question is not how many facts are omitted but rather how transparently accessible is my past or social reality to myself. Secondly, the text, as a specific medium or setting, conditions the ways in which such a question may be answered. Since the medium both unifies and separates and the setting both connects and disconnects, I am left with the possible discrepancy between what my scientific text publicises about my past (or about the social reality I study) and what this past (and this social reality) signifies to myself. Thirdly, the question of how true or factual a given text is should always be complemented by the question: compared to what? Indeed there is a (political?) economy of the scientific narrative’s content in which terms the latter is hierarchically disposed along a scale of relative importance. This scale is the code of the scientific text. Rousseau raises this problem in his Confessions when he says:
I may omit or transpose facts, or make mistakes in dates; but I cannot go wrong about what I have felt or about what my feelings have led me to do; and these are the chief subjects of my story (7,226).

The time structure of a paper such as this is very much related to the question of the author. The specific temporal dialectics of the autobiography lies in that the author though writing about the past attempts to elucidate his present not his past. But, by doing this, he creates a distance vis-à-vis the present and indeed writes in the name of the future. Thus, the text, though much time conscious, becomes relatively timeless. In my own case such timelessness reveals itself in the fact that no matter the appearances to the contrary, I am longing for a future that is meant to take place inside the “world of science” and, thus, the paper contains a message on social science research which aspires to be read outside the personal and temporal context in which it has been written. In a word, there is a pedagogy hidden in this text, or even a kind of underground proselitism.

With this, the question of the author is already being raised. Is the I that did the social research narrated in this paper the same that wrote the present narrative? And the I of the text that follows, is he the same of this induction, where the autobiography of that text (the autobiography for the autobiography) is attempted? Roland Barthes wrote:

When a narrator [of a written text] recounts what has happened to him, the I who recounts is no longer the same I as the one that is recounted (1975: 140).

The author’s discontinuities are not exclusive to the autobiographical text. They also occur inside the scientific process. The scientist’s personal time is not a homogeneous sequence. It is rather intrinsically irregular and inconsistent and this reflects upon his scientific development. Thus, the scientific formation is discontinuous both as it occurs and as it is remembered. That is why any piece of writing is always a bridge between two times (at least).

In the present instance the narrator is not me as myself; but rather me as a surrogate Everyman of social science (one is here reminded of Walt Whitman, that master-definer of self-identities). Here lies the above-mentioned pedagogy. I write to an “internal audience” (to my “implicit reader”, as literary theory would have it), an audience of social scientists who have undergone or will undergo experiences very similar to the ones I describe here. The objective is to attack established paper tigers that are the source of much suffering and person degradation. The objective is to provide a rational meaning (and thus the limits) for the breaking of established rules so that no one in good faith will shout while reading this paper as Austria does in Shakespeare’s King John (III, 1): “Rebellion! Flat rebellion!” Radical criticism has nothing to do with anarchism. The pedagogy chosen here implies a choice between two autobiographical models and thus two author types. On the one side, the total recognition of the author’s discontinuities, investing the text with brutal frankness and even with scandal—that is, Rousseau’s model in his Confessions. On the other side, the last I’s (the writing I’s) full control over his full genealogy, as well as over the global narrative, thus ending in a self-censored quasi-public relations text—that is, Henry Adams’ model in The Education of Henry Adams.

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As will be evident in the text, I have followed Rousseau's model. Some readers will at times find this text immoral, of an immorality similar to that of Rousseau's while referring to his masturbations. And yet it would be foolish to deduce from here that I have been unconditionally free while writing this text. St. Teresa says recurrently in her Life that the authority of the Church suppressed the free expression of her private (and wicked) life. As in St. Teresa's time there are today many authorities and many churches hovering above (and settling inside) us. If one is at least aware of this one can be sure of trailing the right path, Kierkegaard's path when he writes in The Journals (1834-42):

The majority of men are subjective toward themselves and objective toward all others, terribly objective sometimes — but the real task is to be objective toward oneself and subjective toward all others.

On the Rise...

I am married to an old lady in whose shop I have worked since 1970 (at least). This is a work report. In the shop I met a beautiful girl whom I am in love with. This is a love report. I live with this girl in Politeia, a suburb outside Scientiapolis, and I commute every day. This is a traffic report. I write on the road. Never sure precisely where. This is a report on the writing site.

...and the Fall of Metaphor

The critique of method cannot be accomplished without a critique of style. Style is not simply the gown and method is not simply the monk. Both are both. However, no matter how frequent and intense the critique of scientific method has become in the last decade, virtually nothing has been accomplished in terms of critique of scientific style both in discourse and in behaviour and attitudes. This is probably due to the fact that the critique of science has been made mostly by scientists writing in scientific journals which tend to be more indulgent with violations of method than with violations of style.

More generally, since the XVIth century learned discourse in Europe has been waging a sacred war against poetic discourse and its most important tool, the metaphor. As a consequence few people in our time grow metaphors in their gardens. Some lack the seeds, others the tools; most of them lack the gardens themselves. For them I will translate the first paragraph of the previous section: the writing site is the site of epistemology; the work is the work of science or rather the work of the scientist while doing science; the love is the love of political action; the traffic is the social line that connects (and disconnects) work and love.

All these topics will be touched upon in this paper in reference to the discussion of my research on patterns of dispute settlement and legal pluralism in a squatter settlement in Rio. The field research was conducted in the summer of 1970, and Pasargada is the fictional name of the favela where I worked.
On the Writing Site

The subtitle of the present section should be: *the struggle against archeological positivism*. To write a *scientific* paper about what one actually did while doing *scientific* research raises complex epistemological questions, as is demonstrated by the number and magnitude of the assumptions underlying such task. Firstly, it assumes that there is a gap between what one actually did and what one should have done if one were to respect the accepted rules of scientific work. Secondly, it implies that social reality creates obstacles to the smooth operation of scientific standards. In other words, reality is at fault, not science. Thirdly, it suggests that such obstacles can be eliminated or circumvented as one's retrospective writing in itself demonstrates. Fourthly, it presents the writer, no longer innocent, as a more or less mature social scientist free from fanatic proselitism as well as from radical deviationism. He has reached the positive age in Comte's evolutionary scale. Finally, it assumes the continuation of a difficult but on the whole rewarding marriage with social science as existing.

It is not the purpose of the present paper to specify the different questions raised by the different assumptions. Much less to answer them. Moreover, would it be possible to write a report on empirical research without accepting in one way or the other the above mentioned assumptions? I just want the readers to be aware of the epistemological dogs as they walk down the centre of Scientiapolis. However, three general points should be borne in mind. Firstly, the realm of the relevant experiences in the field research is determined by the researcher's conception of science both while doing the research and while writing his research report, the latter being the most determinant. Secondly, the presentation of such experiences is determined by both the rules of the dominant scientific discourse and the rules of public discourse in general (which determine, for instance, if and how personal or semi-intimate matters are to be presented). Thirdly, though a report of this kind tends to reflect an antipositivistic stand, it may hide, at a deeper level, elements of positivism, running uncontrolled. This will happen either because the report questions social reality and not science or because, though refusing the positivistic conception of the subject/object distinction underlying the scientific prescription, it accepts such conception in the analysis of what actually was done in violation of the prescription.

As far as my own report is concerned it is only marginally on what I "actually" did while doing sociological research. On the one hand I did so many things so important for both my personal life and my scientific formation that it would be impossible to remember all of them and if possible the description would appear as utterly irrelevant, absurd, ridiculous or even improper to most of my readers. On the other hand, since it is through science that something is made unscientific, if I were to restrict too much the realm of relevant experience I would be condemning myself for having adopted an inadequate concept of science in my research. In such a case I could be criticized not for having departed from scientific standards but rather for not having departed enough.
Given that each past has its own present, I write on events in 1970 and onwards which, seen from today, were of greatest importance for my present conception of science. I write this from a post-Kuhnian perspective, from the notion that the pre-paradigmatic (rather, nonparadigmatic) nature of social science allows for a scientific radical alternative to established bourgeois science. According to this perspective the relation of one’s actual scientific work to the established rules of scientific method may either be conceptualized as an accidental deviation, or as a more or less conscious attempt to build a scientific alternative. The decision is, in the last instance, a political one.

On Traffic

I graduated in law at Coimbra University in 1963. In 1963–64 I did post-graduate work at the Free University in West Berlin, specializing in criminal law and the philosophy of law. From 1965 to 1969 I was an assistant professor at the Coimbra Law School, having meanwhile returned to West Germany for a short period to prepare a comparative criminal law study at the Max Planck Institut in Freiburg i. Breisgau. In 1969 I went to the USA to get an LLM at Yale University with the intention of then preparing a doctoral dissertation on the insanity defence.

When I left Portugal I was a frustrated legal scholar who having refused to participate in the money machine of law practice usually engaged in by law professors — by writing well paid opinions (pareceres) on important cases, that is, on cases involving important (powerful) people or groups — had not found intellectual satisfaction in the established science of law, that is in legal dogmatics. Indeed, I had ceased, by that time, to see in legal dogmatics a science in any reasonable sense. To my mind the scientific study of law had to be organized from a perspective external to law. Such a perspective I then found in psychiatry and psychology. It was broad enough to include questions of legal philosophy which I was well acquainted with (guilt, free will, etc.). At the time, due to the opposition of the Portuguese fascist regime to the development of the social sciences, I was not prepared to consider the sociological perspective as an alternative. My stay in Germany had not been of much help in this regard; German law schools were then actively opposed to the social science approach to law.

Politically speaking, when I left Portugal I was a very moderate leftist. Making my way up from a working class family, I had always been haunted by the fear of being prevented, for political reasons, from making true the family’s dream: to become a lawyer. The Berlin period contributed only partly to my political clarification. Though I organized colloquia against the fascist regime and its colonial policy and discussed such topics with many students, members of the SDS who later were to become the leaders of the student movement in Germany, I was at the same time traumatized by the daily contact with the stalinist regime of Walter Ulbricht, in the Democratic Republic of Germany. Confronted with crude forms of intellectual control (such as the Haveman affair) and with political repression, and unable to
conceive the regime as a degenerate form of socialism, I was prevented from developing a coherent socialist political attitude.

When I arrived in the USA, the student movement was finally breaking its way into Yale. It was a period of political awareness and of anti-establishment radicalisation: Vietnam, the Cambodia invasion, Kent State, the Chicago Seven, the Black Panthers trial in New Haven, *The Greening of America* by Charles Reich (a Yale law professor), teach-ins, the first students’ strike in Yale’s history, professors on trial for their racist behaviour in student controlled courts. It was also the period in which the “invasion” of the Law School by the social sciences was reaching its peak. So much so that when I was caught by the social sciences’ epidemic and decided to specialize in the field of sociology of law I didn’t feel the need to abandon the law school for the sociology department.

I was soon convinced that the psychiatric approach to crime had its foundations in the sociology of deviance and that the latter had its foundations in the sociology of law. It is amazing how fast I took all these steps. But still more amazing is how I failed to take the “natural” next step: that sociology of law had its foundations in the sociology of the State. As will be seen in the following, this was due to the two theories that dominated the field of sociology of law at Yale at the time, neither of which questioned the nature of State power: the anthropological theory of dispute settlement and the Weberian theory of modern law. The missing link was to take shape much later, under the impact of Allende’s experience in Chile and of the Portuguese revolution of April 1974.

Sociology of law at Yale was studied under the (dis) joint guidance of socio-legal lawyers, as I would call them, on the one hand and sociologists on the other. The former based their teaching either on anthropology of law or on Weber's sociology of law. The sociologists tended either to adopt a somewhat crude behaviourist and positivist position; or to be over-eclectic in their approach to law. And in any case all were trapped by the need to gain respectability inside the law school. The competition and rivalry between sociological lawyers and sociologists was hardly disguised. The former criticized the latter for not knowing enough law and the latter criticised the former for not knowing enough sociology. The usual thing.

Institutionally, the centre of the sociology of law was the ambitious Law and Modernization Program. The seminar on law and modernization, taught by the director of the Program, was the platform for exciting discussions on law in society. The aggressive Yale style of discussion I found most congenial. Compared with the feudal intellectual relations at Coimbra Law School, the liberal free market of ideas was an intellectual liberation.

However absurd it may appear the study of sociology was combined in my case with a process of political radicalization. Exposure to the Vietnam war, to American imperialism in Latin America and to social inequalities and political corruption inside American society broke the blocking effect, which Ulbricht’s regime had produced in me and thus became the objective conditions from which a radical critique of both capitalism and imperialism could develop.

It was in this intellectual and political context that early in 1970 I applied for a Law
and Modernization grant to do research in Brazil, after having read on the announcements board that the Program was funding research on legal services for the poor in Brazil. I had always wanted to go to Brazil, the promised land of both my grandfathers’ stories in my childhood. Besides, the research topic sounded “leftist” and seemed adequate for a critical theory of law and society which I was groping for. Finally I thought that in order to establish my credibility as a social scientist I should do some empirical research. Indeed, though I still had not completely abandoned my plans to write a doctoral dissertation in the insanity defense, all my energies were devoted to an almost obsessive reading on general sociology, sociology of law, and anthropology of law. My sociological training became crucial at the time mainly because I thought that the analytical tools developed by bourgeois science could be used outside their “natural setting” in a radical critique of capitalist society. The political contradictions of established social science were then more clear to me than its relative theoretical shallowness and methodological barrenness.

The further inside the established social science I moved whilst preparing my research project, the more of an outsider I became. A vacuum was created which Marxism gradually (and never fully?) filled. An early manifestation of this intellectual process was the complex experience of conflicting identification which I underwent while reading the empirical and theoretical writings in my chosen field. Sometimes I read the material from the perspective of the social scientist — the view from the top — adopting, as a consequence, the persona of the subject of science. On other occasions, on the contrary, I identified myself with the “victim”, the object of science: the view from the bottom. As my research went on the latter identification became dominant. The more credible I became as a subject of science the deeper I experienced as an object of science. In an Alice-in-Wonderland fashion, I climbed up the ladder that took me down. This was due to the fact that the bulk of my reading was on social anthropology and basically on research done by British anthropologists in Africa and by American anthropologists in the “Third World”. The imperialistic nature of bourgeois social science emerged gradually in my scientific consciousness. Coming from an “underdeveloped,” “peripheral” country — probably, not enough so, however, to be an interesting target for social scientific hubris — I could witness, while reading the material, the development of the process of my own scientific (and political) underdevelopment. But besides the political content (and the political form, as I came to conclude much later) of such studies, what struck me most was that they sounded as false, magnificent networks of misinterpretation, monuments of trained and specialized ignorance.

I became as arrogant vis-à-vis these studies as only a New Christian could be. My legitimacy was grounded on untrained knowledge emerging from sheer experience. My revolt was the revolt of the object against the subject. And when the object revolts against the subject he tends to become a supersubject, in this case, a superscientist. Indeed, to my original motives for undertaking research in Brazil a new one was added: to demonstrate through my empirical research how wrong American legal anthropologists and legal sociologists were in their analysis of law
in "the Third World". The immoderation of my ambition was the counterpart of my resentment. And it could not stop there.

As I said before, the Law and Modernization Program centered around two areas: dispute settlement studies; and law and development studies. An oppressive Weberian atmosphere dominated the latter area. On the political side, the Kennedy heritage was too obvious. The political project underlying law and development studies was hardly processed by sociological theory. There is nothing wrong in presenting law as a positive factor of development as long as the latter is specified and contrasted with alternative types of social transformation, such as social revolution, in which law usually functions as a negative factor. However, revolution was taboo, the non-dit of dominant discourse on law and development. Under such circumstances, law and development studies were bound to overemphasize the positive role of law – an ideological bias in favour of lawful social transformation and against revolutionary processes. And thus they became, whatever the intentions of their cultivators, little more than a rhetoric of legitimation which could be appropriated by the more liberal factions of the national bourgeoisie both in the US and in the “Third World”. In the case of Brazil, law and modernization scholars were trying – having given up the attempt to "civilize" the military dictatorship in power since 1964 with active American support – to create the institutional conditions for a bourgeois democratic regime, stable enough to offset the revolutionary potential meanwhile created by the dictatorship.4

One of the privileged areas of law and modernization was research on legal services for the poor. I read extensively on legal aid in America and visited some offices in the New Haven area. I even attended a meeting on law and poverty organized in Chicago by that champion of social transformation through law, the American Bar Association. Given the differences in scope and political intent among legal aid projects I soon retreated from my initially overoptimistic view of them. Nevertheless I was impressed by the socialist conviction of some of the activists working in the more advanced projects. Indeed it was in view of their activities that I came to anticipate a rather negative picture of legal aid for the poor in the Latin American context. Only a democratic regime with a stable class support – non-existent in Latin America – could allow the oppressed class to be taught the use of law as a weapon of defence (whatever its shortcomings) without thereby undermining the institutional foundations of class domination and State power. Though this line of reasoning proved later to be somewhat simplistic I was unable to control my arrogance and promised to myself that my research would bear witness of the ideological bias underlying law and development studies.

In the light of the precedent, my sociological background when I began the field research comprised two convergent ill-integrated areas of interest: the dispute settlement/informal justice area and the access to law/legal aid area. Though my research project was basically focussed on the latter my scientific interests bent toward the former area not only because it seemed theoretically more fruitful but also because it was the least politically determined of the two. I tried, at first, to pull them together in a topic on "attitudes of the poor towards law" but the naïve
conceptualization of law underlying such a topic faded away as I became more conscious of the class content of the official legal system in Brazil.

On Love
The first important factor in my field research was that I loved Brazil from the very beginning. I loved the people almost as much as I hated the government. It was like coming home after having spent one year among the super-natives of North America. It did not take much time to settle down and two weeks after my arrival I was already living in the *favela* of Rio where I were to conduct my field research. Both the language and my class origin facilitated the adaptation: although in the event things turned out not to be that simple.

On the Same Language
Before settling down in the *favela* where I conducted my field research I visited several other *favelas* as well as other types of working class and lumperproletariat residential areas in the suburbs of Rio. I used to go alone (since I had no “research assistant”) talking to people as I met them. This easiness in face-to-face relations was part of my personality. But it was also due to my innocence (in spite of the literature) vis-à-vis the complexity of these social micro-cosmos. The following incident showed me, in a traumatic way, that talking to people, let alone doing social science, is not as simple as it seems to be.

In order to get the “taste” of the different types of *favelas* I visited one of the poorest ones, built on stilts and squeezed between the backyard of a factory and the Guanabara bay. I had never seen before neither did I see thereafter “living” conditions as inhuman as those prevailing there. I asked to be taken to the shack where the president of the residents’ association lived. We talked for a while on *favelas* and on Portugal and I asked a few questions on that particular *favela* as, for instance, on whose land it was built. At last he asked me: “What are you doing precisely in Brazil?” and I answered: “I am doing research on *favelas*”. The man stared at me, his face livid, his eyes bulging. He suddenly stood up and shouted: “Get the hell out of here!” I could not understand what was going on and was paralysed by surprise and fear. “Get the hell out of here!” he said again, and pushed me to the door of the shack. Scared as I was, I still tried to say something: that there must be a misunderstanding, that I didn’t want to offend him. But he kept shouting. By that time a lot of women and children had gathered around the shack. The man said to them in a shouting voice and pointing me “This guy is a *portugai*, a *cagüete*. Came here to spy on us” and addressing himself to me again: “I have nothing to say to the police. If you don’t move on...” and rushed inside. A woman came to me and said: “In your place I would go right away”. I tried to explain, to say that I had nothing to do with the police. But he came back holding a rifle. The woman approached him: “Be careful. Let him speak.” “Move” was his answer pointing his rifle at me. I looked into the crowd, looking for a friendly eye. My eyes fell down. Slowly I turned around and
moved away. Very slowly and right in the middle of the street. Behind me I could
hear a favela of voices. Were they following me? Some four hundred meters of doubt
and anxiety. Then, a turn to the left and I looked behind me. And ran like crazy till
I reached the highway that connects the airport to the city. Direct to the bus stop.
I got on to the next bus and started to think as the bus started moving. But, first, I
wanted to make sure that I was still alive.

What had happened? When I got home I suddenly found one possible key to
explain such an absurd quiproquo. When I said that I was doing research on favelas
I used the word “investiga��o”. In Portugal’s Portuguese the term research can be
rendered both as “investiga��o” or as “pesquisa”, though the former is more commonly
used. However, in Brazil’s Portuguese and particularly in ordinary language
“investiga��o” means police investigation. Having inadvertently used this word, I led
my interlocutor to think that I was working for the police.

This however could not provide a full explanation for such a violent reaction. I
decided then to discuss the incident with a friend of mine who knew Rio’s favelas very
well, as he had been involved in the self-help projects in squatter settlements in the
early 60’s. It came out that the favela in question was being threatened with removal.
The factory wanted to expand and was pressing the State administration to find
“legal grounds” upon which the favela could be removed. This was known in the
favela as its residents had often been harassed by both the police and the “jagunços”§
paid by the factory owner.

I must confess that I was traumatized for a couple of days. But my analysis of the
incident was very superficial. Though I could see that the favelados’ reaction was the
most reasonable one from the point of view of their interests, I tended to
concentrate my analysis on the risks one runs when one fails to control both the
language and the social context in which one operates. I couldn’t see at the time that
the polysysemy of the words involved was not at all accidental, that there was a
structural semantic relationship between “investigation” as the work of social
science and “investigation” as the work of the police: two different forms of social
control and class domination. I questioned my behaviour (I had made a mistake) not
the science in whose name I acted. Had I not failed, the scientific method would not
have failed me. In other words, my criticism of bourgeois science was abstract and
idealistic; the scientific praxis was viewed uncritically and accordingly the problems
which it raised were conceived as my personal problems or problems of the social
context. And as I conceived the incident so I learned from it. The incident had been
an accident; I learned to be more proficient in science, as the only available
insurance policy against research risks. I didn’t revolt against the insurance
company. On the contrary, I was grateful to it for making insurance policies
available.

On Being Portuguese

The subtitle of this section should be: on the almost genetic incompatibility between being
Portuguese and being a social scientist in Brazil. As I said before, while at Yale preparing
for my field research in Brazil I came gradually to read the bibliography from the point of view of the "victims" of social science, the view from the object. In a sense this was too easy as I was not actively involved in producing science. I was consuming it and the way I elected to do it was little else than a consumer protection strategy. In Brazil, however, it was different since I was there to produce science and to produce it inside the dominant mode of science production in a capitalist society. At first, and as the language incident described above shows, I felt the need to assert myself as a subject of science, as a social scientist. This need was probably exacerbated by the urge to fight against a complex set of stereotypes about the Portuguese in Brazil which made it quite absurd to think of a Portuguese as a sociologist or an anthropologist, particularly if he were to do research in fields up until then almost entirely monopolized by American social scientists. I knew of the stereotypes but I was not aware of their pervasiveness and deep-rootedness. In the following I will show their operations across the class structure of Brazilian society: at Ford Foundation headquarters in Rio; among Brazilian lawyers; among favela residents. Such stereotypes are worthy of study in their own right: though not class neutral in their origins, they tend to operate across different classes, which makes them privileged instruments of ideological discourse.

Given the links between the Law and Modernization Program and the Ford Foundation I was supposed to contact this institution in Rio. The first contacts were disastrous as it transpired that the big boss didn’t think the research project was worthwhile; even if worthwhile, it shouldn’t be conducted by a Portuguese. He is thought to have said that he had “the worst impression of the Portuguese people”. Evidently at the Ford Foundation in Rio I was neither considered to be a competent subject of social science; nor to be a reasonable object of social science as presumably I was not underdeveloped enough. I was merely a Portuguese, a zoo category. This was not an isolated view – for in subsequent years I found the prejudice against Portugal and the Portuguese extremely pervasive among American social scientists doing research in Brazil. Was it that Portuguese colonization was conceived as the “natural cause” of Brazilian underdevelopment? Was it that these scientists unconsciously transferred to the excolonizer their guilt complexes about American imperialism in Brazil of which they were, willy nilly, an integrant part? Was it that Brazilians and Americans having in common a colonial past were able to minimize their present unequal relations by joining in the same anti-colonial attitude?

In relation to my friends among the practising lawyers, the stereotype functioned in a different way. Coimbra Law School where I had graduated and taught had a great prestige among them as one of the oldest universities in Europe and one of the European centres of legal science. I was for them part of this tradition and whatever contribution I could make to science would be in the field of legal science. The sociological and anthropological studies of law were an “American luxury” and should be left to them. Some of them had tried such studies but had soon abandoned them in favour of the more prestigious and financially more rewarding study of legal science. Working on (and even worse, living in) a favela was particularly offensive to my status. It involved a real declassement subtly expressed in the
paternalistic and half-joking way they talked about my research. They accepted it, or rather, tolerated it as another of my eccentricities (besides poetry writing, way of dressing, hair style etc.)

Favela residents had already shown their prejudices against the Portuguese in the language incident. In the favela where I settled down people didn’t understand, at first, that a Portuguese – by definition, a shopkeeper – could be doing sociological research. Even community leaders were puzzled. For them, sociology and anthropology research in favelas was by definition American. And indeed the favela had been since the early 60’s quite polluted (as they used to say) by American social scientists. When I tried to explain that I was doing the same and, at the same time, something quite different, they could hardly understand and were, at first, quite distrustful. As they told me much later, they were afraid that I, like most American social scientists, might be involved in “urban development”, the euphemism used by the State bourgeoisie to threaten favelas with removal.

But, dialectically, the stereotype also worked to my advantage in more than one way. To begin with, it protected me from the stereotype of the American social scientist. As I became more familiarized with community people they would talk (and relate) to me as they would never do to a “real” social scientist. The social space opened to me by counterveiling stereotypization enabled me to know how favela residents viewed American social scientists. They knew the sorts of things social scientists were interested in (and wanted to talk about) and reacted accordingly.

One day a friend of mine introduced me to a person “who knew a lot about the favela and the Americans”. Without waiting for my questions, he started talking about the favela, its geographic setting, types of houses and shacks, residents occupations, etc. It was an articulate discourse moulded in a kind of popular science language, revealing a specialized knowledge of the community. I was amazed and sure that he wanted to impress me. He ended his speech with this astonishing statement: “You are doing research in the favela, aren’t you? The Americans wrote their books on my shoulders.” I couldn’t help laughing. But my friend told me later that, true or false, he was thought to have made money talking to American social scientists. I met him again later and he never showed great enthusiasm in talking to me. Most probably because I was not enough of a social scientist to deserve his specialized services.

This encounter was very important to me for two main reasons. Firstly, it revealed the shortcomings of most books on methodology which, though concerned with the different techniques to avoid answer inducement, left out the founding source of such inducement, the social scientist himself as a living stereotype reproducing an horizon of expectations. Secondly, the informant I met was a trained and specialized object of social science who by that process had become a quasi-subject (or a primitive subject) of social science – an object rising to the status of a subject. Thinking deeper (ad absurdum?) one could imagine how further development of social science could lead to a correspondent development of its object. The group of trained and specialized objects (informants) could, if united, act as a pressure group upon science, bargaining for a share in the profits of science production or
even for participation in shaping the results of scientific research. In time, bourgeois science would reach subject/object integration by means of training and finally coopting its object. The positivists who had fathered the rigid subject/object distinction would have brought it to its fullest development—which also meant its complete annihilation. It would be then up to the anti-positivists to start again looking for new, fresh, uncoopted objects of social research or, if minimally conscious of their class position, to look for an authentic supercession of the subject/object distinction. The latter would involve not only the cooptation of the object (that is in objective terms, the individual’s upward mobility in a class society) but rather the destruction of the subject as a separate class concept. This process against separation, however, could not be carried out as a separate scientific process but rather as a part of the class struggle.

As my research progressed, the stereotype about the Portuguese became less determinant in my relations with people and the initial need to assert myself as a social scientist was secondarised. Incapable of seeing myself, without a great dose of hypocrisy, or schizophrenia, as an object of social science, I ended up with a compromise position, locating myself half-way between the object and the subject of science (and thus in an intrinsically ambiguous position). I felt like a kind of errand boy trying to settle a long-term dispute between object and subject, a dispute being fought since the nineteenth century when the bourgeoisie institutionalised social science as a part of its ideological apparatus. This awkward position was the foundation for the development of what I would call transgressive methodology.

On Transgressive Methodology

The relative freedom from the scientist's stereotype helped me to create the persona best suited for my research objectives. I developed a less than moderate respect for the rules of established science, particularly for those that filled the fat manuals on participant observation, then the most fashionable method of empirical research. I was led to believe that it was through violation of the rules that I understood social reality: the greater the violation, the deeper the understanding. Nevertheless at the same time I followed the golden rule of participant observation and did so in an almost compulsive way: I wrote about my everyday life down to the smallest detail. I kept the traditional distinction between the research cards and the diary, leaving the latter for the more intimate, less “scientific” matters, but, in fact, one medium prolonged the other almost without transition. I could reproduce everything (words included) so vividly that my writing was an authentic transcript of my life.

I changed my original research project soon after I started living in the favela. I became convinced that the attitudes of the “poor” towards law were the product of the “attitudes” of the law towards the poor. Moreover, framed in the way it was, my research question had been totally ideological. Firstly, “attitudes” were the subjectivistic disguise of the objective conditions under which the legal apparatus of the capitalist State operated. Secondly, the legal system was fetishized at the same
time as its power base, the State, was left outside the analytical framework. Thirdly, the research topic was a law and poverty topic and, as such, based upon a social stratification model of social inequality. Finally, the project centered around State legality, whereas focus on legal pluralism as a form of class conflict, seemed more appropriate to my scientific interests at the time.

Though I was at the time fully aware of the first two objections I was not adequately prepared to answer them. I did not know nor am I sure I now know either how to integrate law and the capitalist mode of production in a minimally coherent theoretical framework or how to analyse the complex relations of the legal system with the State apparatus as a whole without falling into demagogic sloganizing. I was, however, well equipped to conceive the "poor" as the class or classes oppressed by a ruthless bourgeois power and to analyse the "informal" law developed within *favelas* as a survival strategy in view of structurally unavailable or hostile official legality. A complete study would have required both an analysis of the community law-ways and an analysis of legal aid offices in Rio. Lack of time, however, forced me to concentrate on the former based largely on a dispute settlement approach.

This choice, however clear cut, was made only gradually and with a great amount of personal anxiety and soul searching. The choice was in fact never fully accomplished as I was basically (and immoderately) interested in a general coverage of the class determined operation of the legal system in Brazilian society. I continued to pay close attention and to observe in detail the Free Justice services in Rio. I was impressed by some of the lawyers working there under strict institutional limitations. Fully aware that they could not change the system of class oppression as reproduced by law, they were as sensitive as possible in the circumstances to the needs of their clients. They showed a practical knowledge of law in society that was indeed much superior to that of those ambitious young Brazilian lawyers who were being sent to the U.S.A. to study the most sophisticated law and development theories. To the despair of their well-intentioned teachers, the great majority of the latter showed a quite remarkable incapacity to learn anything beyond that which was strictly necessary to earn skills and degrees that would entitle them to work at high profit for American multinationals operating in Brazil.

The difficulties in shaping the research project produced at times a paralysis which could have become dangerous had I not meanwhile found an alternative project, a political one, as an outcome of the political radicalization which had occurred as a result of my contact with a fascist regime much more ruthless even than the one I had known for more than twenty years in my own country. Brazilian social scientists, most of them ousted from the universities, helped me much with their writing to understand Brazilian society. But it was through long conversations with radical leaders of the *favela* that I learned most. I was at the time, far more than I could ever be aware of then, under the influence of the distinction between science and politics, and thus, I kept the two projects relatively separated. Anything I could not use in my scientific project I would integrate in my political project that was basically my own political education and that of those whom I was in contact with.
My scientific project thus advanced parallel to my political project. I had ceased to be an one-dimensional scientist, so I thought. And indeed it was an illusion produced by two one-dimensional projects and personalities nailed together in a very precarious way. Only later (and never fully) did I understand how the two projects should feed upon each other if I were to avoid the all too pervasive schizophrenic syndrome of radical social scientists of our time: to be revolutionary as political activists and reactionary as scientists.

The construction of an alternative social praxis justified for me the inevitable violation of some of the rules of the scientific method. As an illustration I will mention two questions: the purposeful orientation of verbal interaction in the field towards the research topic; and change of the field while observing it.

As to the first question, I was reluctant to bring out the topic of my research as long as I felt that it would be a unilateral decision, foreign to the context of the verbal encounter and forced upon the person I was talking with on the basis of my higher social status. This attitude was grounded on my refusal to see the subjects of open intercourse as objects of a secret intercourse (between me and the “world of science”). It was also grounded on my self-consciousness about their “refusal” to see me as a “real” social scientist and thus, on their different expectations about my relation to them. I started thinking later that the social control function of bourgeois science begins with the repressive nature of the verbal discourse it forces upon its objects in both questionnaires and interviews. Based on the same premises as material production—that is, private property and profit oriented productivity—the production of scientific research appropriates the autonomous discourse of everyday language from its objects to build its own estate of scientific discourse which then is used as a form of social power. While analysing the data it seemed that even science had benefitted from my transgressive methodology. As friendships developed I was given such information as people would never dream of giving any scientist. Indeed, the burden not to violate this friendly and trustful relationship became an obsession later on when I was forced into the strait-jacket of science while writing my doctoral dissertation. In any case, the knowledge that was kept secret was crucially important to the construction of the knowledge that I allowed myself to publicise.

The way I dealt with the question of changing the field in the process of observation was in part the result of commonsense and in part the result of an overriding social or political purpose. When during the settlement of a dispute between neighbours, the president of the residents’ association requested the opinion of “our Portuguese lawyer”, I could not refuse, in many cases, to give it without offending all the others by what would be interpreted as an arrogant attitude. If a group of leaders having a meeting with “asphalt candidates” before the national elections asked my opinion about some point of political strategy, they would at least be surprised if I refused, after having had so many hours of political discussion with them. If a “marginal” — a scared-to-death, middle-aged man, hidden in the favela and feeling like returning to a “softer and more honest activity” (for example as a drug dealer) upon having heard that his name was on the death squad
list for the killing of two policemen – asked me, after having been introduced to me by a friend of mine, to help him find a good lawyer, I could not refuse without breaking the rules of friendship and solidarity.

Whatever the immediate motives the decision “to change the field” always had political implications. As my research proceeded it became clear to me that in some instances indeed the correct political decision would be not to change the field. As an illustration I will mention my relations with social workers operating in the community. At the time of the field research there were great tensions between the residents’ association and the Social Centre of Fundação Leão XIII run and staffed by social workers. The tensions emerged with the resistance of the residents’ association to the newly enacted laws that gave the social workers control over it. The social workers who were totally insensitive to the crudeness of their intrusion on the autonomy of the association tried to use my authority to influence the president of the residents’ association and “lead him to change his stupid attitude”. I had to repeat to them that as a social scientist I was not supposed to change the field – an argument which overwhelmed them even if they could not understand it. Indeed I made available to the residents’ association my knowledge of the social control strategy of the Centre and encouraged them to keep resisting.

Other experiences of a more personal nature – which could hardly be conceived as “changes of the field” – arose in a very unequal way which in itself reflected, at the deepest level, the ambiguity of my attitude vis-à-vis the “field”. In other words, they betrayed both the residual scientist living in me and the classist nature of my presence in the community. The best illustration is given by my “religious experiences” and, in particular, by my participation in Umbanda sessions. However, ritual rather than the class nature of religion as a social phenomenon was more determinant in revealing the internal contradiction of my presence in the religious circle. In the Umbanda sessions political solidarity was mediated by religious identification and the adequate medium for the latter was not the verbal discourse to which I was used but rather a “total discourse”, a kind of apocalyptic experience which involved the individual personality as a whole and merged it into the collective personality of ritual as an ongoing living process. The retreat into observation, that is, into science was a defence mechanism against my fear of losing control. But I was clear-headed enough to observe that my efforts to keep in control were considered by the crentes precisely as my being out of control, since only a person out of his mind could refuse the invitation of the Pai de Santo to join the group in the collective praise of God and His Saints. There was, thus, no room for half-way involvement or partial participation. I was either paralysed in my arrogance or kneeling for mercy. In a situation such as this the structural mystification upon which participant observation is based was bound to reveal its dilemma in full clarity: if you observe you don’t see; if you participate you don’t remember. This dilemma was, indeed, an integral part of my religious experience. As a result, there were some Umbanda sessions in which I chained myself to the observation pole while in other sessions I burned the pole, the scientist, the chains and let the ashes disperse in a collective orgy of harmony. In the latter case the transgressive methodology was
not part of my plan: it happened and I “happened” with it.

In the sessions I “observed”, the group intimacy and the authenticity of the religious event was disrupted by the intrusion of the social scientist. I was not a neutral observer, I was a policeman – the more so when I pretended to emphasize my “neutrality”. By my physical presence, clothing, posture, etc., I was a foreigner and a spy. A dissenter and a troublemaker and only because I belonged to the hegemonic class was I tolerated. Oftentimes I saw myself as a tourist in a sexy bathing suit visiting the village church during religious services. Indeed everything I did connoted the presence of an intruder: my place in the room, my relative immobility, my aloofness in particularly crucial moments, my refusal to participate in specific acts of the ritual. My whole posture was a kind of abstract uniform as awkward as the habit of the missionary observing the religious celebrations in the “jungles” of Africa. And, indeed, symbolically speaking, my posture was as white as the missionary’s habit (or the doctor’s gown). It represented both the aseptic position from where I, like Pilate, could observe the dirty underworld and the mechanical whiteness of the bride chained, like Prometheus, to her arrogant virginity. It was also a triumphalistic whiteness since I apparently saw no danger of being contaminated. Like the doctor or the hegemonic priest I was there to cure or at least to collect the data upon which a cure could be planned.

My “white” posture and presence, however, had nothing to do with the white robes of the médios and of the mãe-de-Santo. Theirs was a whiteness offered in holocaust, ready to get dirty – which literally happened during the sessions. On the contrary, my whiteness concealed its weakness behind its hegemonic right to establish the rules of the game and to do so in such a way as never to lose. The radical conflict between the two modes of being white was a kind of religious chasm which barely disguised the class conflict underneath. The fact that, though an intruder and an absolute minority in the group, I could be tolerated by the latter is evidence that the religion to which I belonged was also the religion of the hegemonic class. Accordingly, though observation was reciprocal, I observed the group arrogantly (imperialistically) while the group observed me powerlessly.

The above shows that my observation was only apparently neutral. In real terms it was a hostile observation. I interpret as hard evidence of this the fact that during the sessions, and irrespective of the place I occupied in the room, I tended to be surrounded by the quasi-marginal elements in the religious group: younger people always ready to make jokes about the ritual; less motivated people who were there out of curiosity; new converts who were still afraid of getting too much involved. It may have also been that it was I, rather than they, that took the initiative of standing close to the elements that represented the best chances of discrediting and challenging the religion under study. In either case, it is now clear to me that my observation became associated with the weakest link in the social process being observed. That is, my “neutral” observation was disruptive and knew how to maximize disruption. The neutrality of the social scientist was a mode of neutralizing social reality.

What was tragic about all this was that at the end of the observation sessions I
could fill in my check list very neatly, but could hardly see the relationship between my notes and what had really happened in the session. More tragic still is the fact that this did not surprise me at all. During the sessions the observation was reciprocal not only because the group observed me but also because I, myself, observed my own observation. In so doing I sabotaged it. My observation was powerless vis-à-vis its own arrogance.

In the Umbanda sessions in which I participated I became a more or less indistinct member of the group. There were some disturbing class-related factors such as my clothing and the colour of my skin. But these elements, which in the sessions which I “observed” were part and parcel of a coherent whole and were not individually noticeable, became in the “participating” sessions awkward appendages, anachronistic accessories, floating around in a frantic search for a lost identity. I had to forget them before the other members of the group could do the same. Sometimes I succeeded, sometimes I did not. Reified as I was by the division of labour in the techno-scientific society, I tended to see myself, in these sessions, as vacationing, doing therapy, or simply attending the services of my religion, but I could never see myself working or studying. I must confess that I considered the “participating” sessions as lost for my research.

Indeed when after such a session I tried to go through my check list the effort seemed ludicrous or even macabre. I did not remember at all (or only vaguely) the items which I was supposed to check. The more “important” the item, the more total the blank. The effort was also macabre: after participating in a love experience I was dissecting corpses in the anatomic theatre. The richness of the experience had nothing to do with the rigid dead words of the check list.14

The types of rule violation which were made possible by the transgressive methodology show that the latter was, in the last analysis, an attempt to liberate the object of science by liberating the scientist from the illusion of self-control. It is now very easy to speak of transgressive methodology. But at the time I was ending my field research things looked rather less clear. Confronted with pressure (both internal and external) to show that I had deserved the money invested in my research, I felt very anxious and lost. When I left Brazil I very much doubted that I had enough data to write an acceptable paper. I even doubted that I had data at all. I only knew that I had gone through a personally and politically relevant experience. But even that I tried to forget in order to be able to adapt myself again to life and work in the headquarters of central science.

On Work

The social scientist’s work is determined by a wealth of factors, such as class origin, professional training, institutional setting, ideological atmosphere and so forth. All these factors bear a complex and contradictory relation to the labour process of scientific production itself. As a result the integration of this labour process into that of the capitalist mode of production as a whole may seem somewhat problematic. For instance the ideology of the autonomy of the scientist which for many years
dominated liberal bourgeois science contributed, in its practical operation, to
guarantee the invisibility of the class nature of both the form and the content of
scientific production. The credibility of this ideology arose from the fact that it bore
some genuine correspondence to the process of scientific production prevailing at
the time. In recent years, however, important changes have taken place in this field
as a consequence of the changing structure of capital accumulation on a world scale,
involved changes both in international relations and in the nature of State power.
Among other things the massive intervention of both State and industry in the
institutional setting of scientific production in advanced capitalist societies had
tended to shake the ideological credibility of the notion of a class neutral labour
process in science. These changes have also facilitated the radicalization of scientists
as scientists.15

Back in the USA and deprived of the daily contact with the fauela, my written
record gradually became the main controlling instance of my reference to the past.
The “data” began then to emerge from what had been an “undatable”, total
experience. As if science, like Phoenix, arose from the ashes of passion. But the open
space thus created for scientific development was shaken to the roots by a particular
event.

It so happened that almost by chance I came to know that the Law and
Modernization Program was, like many other programs throughout the country,
funded by the State Department. This was a great shock to me and to some of the
other foreign graduate students. It had never occurred to me to ask about that and
in retrospect I now felt naive and stupid for having thoughtlessly assumed that the
fat money involved could possibly come from nowhere. This naivety and stupidity,
however, were not “innate” traits of my personality but rather the result of my
scientific socialization in a country where the social sciences had been banned for
many years and where whatever real scientific process was carried out seemed to
be dominated by pre-capitalist relations of scientific production, inside which the
scientist could indeed be credibly thought of as an autonomous producer of science.
I personally had never questioned such ideology and had in fact always felt myself
to be an autonomous producer of legal science, who was paid to teach but not to
do research. Indeed, the class determination of my labour process as a “legal
scientist” was so complex and contradictory that my autonomy was a convincing
appearance and as such a lived experience. Probably, this fact also accounted for the
contrast between my strong, outraged reaction and that of other leftist students
from “more developed” countries. The latter, in fact, were more prepared to accept
the facts cynically and to exploit them to their own advantage.

My previous scientific background and socialization also accounted for my
dealing with the whole issue as an ethical question, leaving in the penumbra the
material base of the scientific process in which I was involved. Accordingly, my
moral outrage was targeted at the patient director of the Program. My central
criticism was that he should have let us know from the start about the financial
structure of the Program. The director, though a good friend, was puzzled and
offended by my reaction. In his opinion one should accept as a given fact that social
science today cannot be pursued unless it is funded. Thus, the question concerns mainly the conditions imposed by the funding institution; it makes no difference whether such an institution is Yale University (which gets its money from stock market operations) or the Ford Foundation or the State Department. And he took great pains to show me that in this case no strings had been attached to the funding and I was even given a copy of the funding agreement.

I was not really convinced and kept thinking that the source of the funding had been hidden from us in order to avoid our reactions. Long discussions then took place both with the director and with other Yale professors involved in the Program, on the one side, and with foreign graduate students and scholars, on the other. The latter had, as it turned out, the greater influence on my subsequent reactions.

We were a very heterogeneous group in terms both of our countries of origin and of our intellectual interests, but most of us shared left-wing political attitudes and a critical stance vis-à-vis American imperialism. After much discussion we were able to clarify our views on the imperialistic use of the social sciences and to define our position vis-à-vis the Law and Modernization Program. Firstly, we contended, established social science in advanced capitalist societies reproduces, in a very specific way, the structure of class domination both internally and internationally and the Program was part of this process. Secondly, such reproduction, far from being limited to the political use of scientific results, involves the theoretical apparatus of social science, the methodological tools, the conceptualization of social reality, and probably even the epistemological foundations. Thirdly, under such circumstances, the question of the strings attached to the funding of specific research projects is at least in part a false one, since it confines the question of political determination to the realm of scientific results alone. It plays, however, an important role insofar as it establishes the conditions under which liberal ideology in science makes itself credible within the dominant mode of science production.

Fourthly, the ideology of liberalism is internally contradictory and it is through its contradictions that radical science may establish its practice in class societies. In other words, the residual autonomy granted to the scientist by bourgeois science may be used to build up a radical alternative to bourgeois science itself.

Since we had been granted liberal scientific autonomy inside the Law and Modernization Program — in that we had “chosen” our research topics, even if within the pre-announced limits of the Program; that some of us had changed our research projects once or twice; and that no one had controlled our scientific results or pressed us to produce policy recommendations
d the conditions were there for us to convert our moral outrage against scientific imperialism into a purposeful scientific and political energy. A few of us started reading and discussing Marx in a more systematic way and a kind of counter-course on the Marxist analysis of imperialism was organized. Two of us followed the only “official” course on Marxism offered then by the Yale Graduate School and taught by Leon McBride. As I was convinced that Hegel’s logic was more important than anything else in the understanding of the roots of Marx’s dialectical method, I also attended a seminar on Hegel’s logic taught by J. Finlay — a distinguished Hegelian teaching his last year
before retirement—and spent a great part of the semester reading *The Science of Logic*.

The subsequent theoretical clarification made it easier for me to distinguish between the institutional set-up and the people who ran it. Some of the latter respected my feelings, tolerated my occasional arrogance and eventually became my friends.

How to integrate the new theoretical developments with the empirical data from my research in Brazil was much more difficult. One such problem was of a directly political nature and concerned the fear that my research data, once beyond my control might be put to an imperialistic use. At a distance now, this almost obsessive fear seems quite disproportionate in view of the nature of the data themselves. But at the time my anxiety was only appeased by a change of names, numbers and locations so as to prevent the identification of the community, and further by a careful selection of the data I would permit to be used in the analysis—which meanwhile had expanded into a doctoral dissertation on sociology of law.\(^{17}\)

In the light of the new position I had taken on bourgeois science as a possible instrument of imperialism my data changed their political and scientific status or nature. The most interesting data according to my original theoretical purposes became the most politically delicate and were eliminated from the analysis. For instance, though I was familiar with anti-fascist activities inside the community, I would not deal with them irrespective of their relevance to understanding the operation of the community legal system, which constituted the topic of my research. Indeed, I had to exercise a double control over my data, since Pasargadians had provided me with such information as they would have withheld from someone fitting their stereotype of a social scientist.

The priority given to political criteria in the selection of the data was ambitiously conceived as part of the anti-imperialist struggle at the level of social science. The implementation of such a priority, however, was a recurrent source of psychological stress which, at times, led to paralysis. In a sense I knew too much to be able to write but in another sense, and particularly when comparing my usable data with the data my friends in the Program were using, I knew too little to be able to write a publishable paper.\(^{18}\)

As a matter of fact these problems constituted a preliminary and, to a great extent, a negative confrontation between the data and the theory. At a much deeper level, the question was: how could the theory *fight against* the data without becoming self-destructive?

In view of the nature of the my theoretical development after my field research was completed, the data "suffered" several deconstructive and reconstructive transformations, which were also made possible by the transgressive methodology I had adopted while in the field. This process, however, involved a double integration between theory and data. On the one hand, my transgressive methodology had been based on a "spontaneous", hidden, undeveloped, and to a great extent "intuitive" transgressive theory. As I developed the latter it became necessary not only to reconstruct the data but also the methodology through which they had come about. The transgressive methodology had to meet the transgressive
theory at a higher level of coherence. The temporal structure of this process was a very complex one, since the theoretical development undertaken in the present called for an imaginary (but nonetheless real) continuation of the field research conducted upon the written record according to an enlightened transgressive methodology. The record thus became the record of the past (as written) and of the present (as rewritten).

On the other hand, given the limitations of data reconstruction by this process – data are collected inside a given theoretical object, in this case dispute settlement patterns; changes inside the same object only lead to changes inside the same data – the integration was also between the different theoretical levels called for by the data. More specifically, the question was how to integrate a broad Marxist theory with the theories of dispute settlement. This question was gradually but only partly solved by the data I had collected on the operation of the State legal system vis-à-vis squatter settlements – another instance in which the open-endedness of the field research proved beneficial. It was thus possible to integrate the narrow dispute settlement object into the broader legal pluralism object and to open upon this middle ground the theoretical space for a Marxist analysis of law in a capitalist society.

In the first paper I wrote on my research there was juxtaposition rather than integration of the different theoretical objects. In the first part of the paper I tried to develop a theory of the evolution of State legislation on favelas. The theory was aimed at explaining how the State intervention had not sought to solve the structural problem of urban squatter settlements but had rather tried to control the social tensions arising from the continued unsolution of this problem.

This theory, which I presumptuously called the negative dialectics of law, was my first (and so far the only) attempt to offer a radical alternative to the law and development theories: I proposed a theorization of law as an obstacle to social change (Santos, 1971).

In subsequent drafts of my doctoral dissertation I tried a fuller integration among dispute settlement patterns, legal pluralism, and Marxism without ever fully succeeding. This failure was due to a complex set of reasons.

Firstly, there was (and is) no coherent Marxist theory of law in society. Marx’s fragmentary references to this topic are exclusively concerned with the State legality of modern capitalist societies; there is virtually no Marxist theorization of “informal”, “unofficial” legality in capitalist societies, or of legal pluralism, or of law in pre-capitalist social formations. Secondly, though the legal anthropological theories of dispute settlement had meanwhile become unattractive for their failure to locate communities in their broader political context, I remained committed to a detailed analysis of community legality, as I felt that such analysis could lead me to sociological insights of a much broader scope. Such a strategy, however, collided with an emphasis on legal pluralism, the ground upon which I had chosen to build a Marxist theory of Pasargada law.

Through much trial and error I reached an unstable compromise. I commenced by analysing dispute settlement and prevention patterns through study of legal
rhetoric – the latter seeming to be the most adequate strategy to unveil the basic structure of Pasargada law – and then resorted to the analysis of legal pluralism whenever this helped to illuminate the operation of legal rhetoric in Pasargada. The resort to legal rhetoric also symbolized my personal revenge against the elitist training in legal philosophy I had received in Portugal and in West Germany. Indeed, I tried to apply the most sophisticated philosophical reconstructions of highly developed continental legal systems and legal dogmatics in a socio-legal context which, from their points of view, was an illegal setting of marginal and deviant groups living on the fringes of society.

On the other hand, the situation of legal pluralism was conceived in Marxist terms as an unequal exchange between a dominant (official) and a dominated (unofficial) legal system reproducing, in a specific way, class relations and conflicts in Brazilian society. But I failed to theorize the impact of this legal pluralism on the operation of legal rhetoric in Pasargada law.

On Traffic 2

In 1972 I was invited by some friends then working at the Catholic University in Rio to teach a course on the sociology of law there. I carried with me a secret project: to go back to the favela and to discuss with the residents the results of my research (probably by organizing a public meeting at the residents’ association). The idea was, thus, to return the study back to the community – the most cherished dream of radical social scientists in the 1960s.

On my first visit to the community my first contact was with the police at the favela’s main entrance. They checked my passport and interrogated me.

I was later told that police raids in the favelas had become a daily experience. It didn’t take long to reach the conclusion that my dream, if not impossible like any dream, was absurd. The most obvious reasons were of a political nature. Since 1970, and in spite of the official rhetoric to the contrary, there had been an increase in political repression, and the search for “communists” in favelas was a daily experience and anxiety. Local associations had become privileged targets of police infiltration and political repression and thus the topic of community organisation had become highly explosive. Under such conditions it was utterly impossible to have a public discussion of my research topic.

But even if possible, it would have been an absurd exercise. In the course of the few private discussions I had with my friends in the community it became clear that my findings were either obvious or irrelevant to them. On the one hand, when “expelled” from their theoretical cave and exposed to the light of ordinary language, my data vanished, dissolved in the uninteresting web of my friends’ daily experiences, hopes and frustrations. On the other hand, my theories – presented as my interpretations – were utterly irrelevant for the needs of the community and did not meet the difficult conditions under which the strategy of community survival was being carried out. In other words, my theories silenced the ever present
question of what is to be done. And my efforts to answer it on the basis of my political commitments were met with scepticism as my "radicalism" was of little use for people fighting in a situation of fascist political repression. In other words, I was not part of their struggle.

In trying to analyse the failure of this rather naive attempt to wash out the original sin of established social science, I came to the following conclusions. Firstly, having decided to avoid policy analysis for the fear that my recommendations once taken out of context might be used against the favelados, I thereby eliminated the only ground upon which my research findings might have been understood and discussed in concrete and practical terms inside the favela. Secondly, the impact which the community had on me during the field research withered away as I returned to the US and took shelter in the temple of science. As I retreated to science the favela retreated to the object of science; as I became a scientist, favelados became objects. This clearly indicated that the method used for the field research (transgressive methodology) was probably more radical than my subsequent theoretical development, in spite of appearances to the contrary. Such appearances derived from confusing true radicalism with Marxism. Indeed, although Marxism has the potential to build up a truly radical alternative to bourgeois science most Marxist science production stops short of that. This is due not to social scientists' subjective deficiencies but rather to the objective conditions of the scientific process. In trying to understand the hidden positivism of much Marxist theorizing I later came to the conclusion — long after I had finished the Pasargada law research — that as long as the division between manual and intellectual labour is uncritically accepted, the subject/object distinction will remain uncontrolled inside the scientific process and will neutralize any subjective intention to use science as a liberating force against class oppression, no matter in how radical (or how Marxist) a guise the discourse presents itself.

Notes

1. My discussion in the Induction above should have made it clear that I am aware that this analytical platform involves two risks. Firstly, the risk of infinite regression: as the (scientific, political, and social) conditions change, it will always be possible to write a report on what one actually thought while writing about what one actually did while doing empirical research. Secondly, the risk of relativism: to assume that all the actual experiences in the course of the empirical research were equally determinant for the construction of a scientific (and political) alternative. To a great extent it is impossible for the reader to evaluate if and how I have tried to avoid such risks in the present paper.

2. I was then crossing the Wall every week to visit my girl friend in East Berlin.

3. As this Program's organisers described its objectives and focus: "Modern laws and legal institutions may be essential to the modernization of developing societies. But despite the belief that law reform is essential for developing nations and growing evidence that effective change through law is an extraordinarily complex process, little systematic research has been undertaken on the role of law in modernization. Although some social scientists have recognized the importance of legal systems in development, they have not been sufficiently interested to explore thoroughly the operation of legal institutions. At the same time academic lawyers have generally emphasized the conceptual problems of the legal systems of developing countries while focusing only peripherally on related economic, political
and social issues. Little joint work has been attempted by lawyers and social scientists. To help fill this gap in research and teaching, the Law School of Yale University has instituted a Program in Law and Modernization. The Program will support theoretical research as well as empirical studies of the social, political and economic dimensions of the legal systems of specific developing societies, of legal barriers to change, of crosscultural comparison of the interaction of legal systems and modernization and of strategies of planned social change in specific societies. Empirical research focuses on legal systems in developing countries, but the Program will also support work on basic legal and social science theory necessary to further comparative study of law in society. Empirical research is currently underway on East Africa, Brazil and India."

Taken from a public relations brochure this quotation does not explain the Law and Modernization Program, its real objectives and underlying strategies, its conditions and limitations. But it reveals its ideological background which is also relevant for the purposes of this paper.

4. Though I could not question the personal honesty of the scholars involved, some of them good friends, I could never understand their naivete and blindness vis-a-vis the objective conditions of the historical process they were living through both in the US and in Latin America.

5. Portuga is a pejorative name for a Portuguese in Brazil. Cagute means police informant in Rio slang.

6. Jagunco is a private policeman used mainly in the hinterland by plantation owners or big land owners.

7. This scenario is not as absurd as it may appear. In anthropology, the social science that has most developed the scientific category (and has most exploited the social person) of the informant, field workers have since long been confronted with problems that point in the directions projected in the text. Max Gluckman writes that "the tribal peoples who have provided anthropologists with so much of their data are now often able and eager to read what is being said about themselves. They are likely to protest if they think that they are being misrepresented and their lives may be affected by the publication of facts that hitherto have been known only to a privileged few, or that everyone whispered about or knew half-consciously but no one admitted openly(...) Those anthropologists who have worked among literate peoples have often found that their informants are only too well aware of the hazards of publication" (1969: XVIII). On profit sharing by informants Richard Abel reported to me in a personal communication that after the Watts (Los Angeles) riots of 1965, the first major race riots in the U.S., social scientists descended on Watts like a plague. Eventually the blacks began, successfully, to charge US $100 on interview. And in Kenya some potential informants for an oral history research refused to talk unless they were paid Kshs 1/1 (U.S. $.14) per word. In response to demands of this sort, the University of Nairobi attempted to organise a price fixing conspiracy among researchers, both to avoid inflation and control the market, and to prevent Americans from outbidding everybody else for data!

8. I attended the appeal trial of Caio Prado Júnior, one of the most distinguished Brazilian social scientists. He had been convicted of incitement to subversion on the basis of an interview given to a student magazine of the University of São Paulo in which he tried to demonstrate that there were no conditions for armed struggle in Brazil at the time. The subversive character of the magazine was "proved" by the prosecutor by reading from an article in which "the Vietnam War was explained from an anti-American perspective". It was an eloquent demonstration of American imperialism in Brazil.

9. As a matter of fact my attitude contained a high dose of dilettantism. Was it not after all the desire to be above classes (Karl Mannheim's free floating intellectual) that led me sometimes to leave the favela and go to Copacabana beach, get me a nice meal in a nice restaurant in order to go on enduring the diet of rice, beans and rabada at Dona Aurora's?

10. An expression meaning that the politicians were outsiders: they didn't belong to the community though they campaigned there.

11. At the time of my research the Fundação Leão XIII, originally a Church organisation, was already integrated with the State social services agencies.

12. Once free from his neutrality bias the social researcher is forced to recognize that his observation inevitably changes the field. See Gluckman op.cit. In my view, it is not enough to recognize (apologetically?) the inevitability of such changes. One has to orient their production according to rational criteria be they political or ethical.

13. Umbanda is an Afro-catholic cult widely practiced in the favelas of Rio that in a certain sense may be considered a "religion of the oppressed" (Lantemari, 1965).

14. Indeed the implicit criterion of observation in most check lists I have consulted tends to orient the
researcher's attention toward the technical dimension of social life, to the external apparatus with which things confront other things, and these are the aspects that become least important once participation assumes its own dynamics. Checklists are mechanistic in their construction and tend to impose a mechanistic view of social reality. The social scientist's quest for neutrality and for keeping in control is the structural equivalent of the technical dimension and external apparatus of social reality mentioned above. And as much as any mechanistic perspective involves an expansionist ideology and a will to dominate so is the researcher's neutrality a way of neutralizing the social reality under analysis. Moreover, the researcher controls himself only by way of controlling others.

15. I deal with these questions at greater length in another article (Santos, 1977).

16. The only attempt at direct political control happened much later when one of the readers of my doctoral thesis insisted that I eliminate a reference to the Marxist theory of the withering away of the State and State law in a communist society. But even in this case the ideology of liberal science was at work as the "recommendation" was justified on scientific and not on political grounds. And, consequently, it was on these grounds that I insisted on keeping the reference.

17. The title of the dissertation was Law Against Law: Legal Reasoning in Pasargada Law, and was published by Ivan Illich at CIDOC, Cuernavaca, Mexico, in 1974. A much shortened and revised version of the dissertation was published under the title "The Law of the Oppressed: The Construction and Reproduction of Legality in Pasargada" in the Law and Society Review 12 (Fall 1977), 5ff.

18. Max Gluckman, writing about the problems confronting the fieldworker, points out that "he [the anthropologist] has continually to clarify his own role in the society he is studying so that he is neither left completely in ignorance on the outside nor completely captured at the centre where he knows so much that he can publish nothing" (1969: XVIII). This quotation, as well as the whole piece from which it was taken, shows that Gluckman, an undisputed authority in the field, was acutely (even if powerless) aware of the dilemmas and ambiguities of established methods of field research as discussed above in the section on transgressive methodology. To be or not to be "captured at the centre" is not merely a political dilemma as the foregoing discussion could imply. It is also a knowledge dilemma, and while Gluckman is concerned with publishable knowledge I am concerned with "writable" knowledge. This dilemma wears at least three difference faces: the face of time, the face of ignorance, and the face of perspective.

The face of time: written knowledge is a ruminated knowledge, or rather a delayed knowledge. It is based upon a temporal distance between the knower and the known object and therefore it lacks the intensity of instant knowledge (practical knowledge as practiced). When at the centre — and the centre is both a spatial and a temporal category — of a given social practice one needs instant knowledge to orient one's action at any moment and tends to be impatient with any form of a posteriori knowledge which purports to know everything only after everything has become nothing in terms of the ongoing social action. To establish the limits of ruminated knowledge, however, does not mean to consider the "skill to ruminate" as useless in general. On the contrary, one could say of writing what Nietzsche wrote in The Genealogy of Morals on the reading of his own writings: "One skill is needed — lost today, unfortunately — for the practice of reading as an art: the skill to ruminate, which cows possess but modern man lacks. This is why my writings will, for some time yet, remain difficult to digest" (1956: 157).

The face of ignorance: there is a critical level of ignorance below which it becomes impossible to write. In order to be able to write about something one has to ignore it to a certain degree. To write is to objectualize and this both presupposes and creates ignorance about the object. On the contrary "to be at the centre" means supreme identification (superecession of the distinction subject/object of knowledge) and thus to write from there implies a process of deviation, a kind of reason. The French philosopher Gilles Deleuze refers to this problematic in a much more radical way (and from a different perspective) when he writes: "How can one write except about what one does not know too well? Only then does one imagine to have something to say. One writes but at the limits of one's knowledge, at the far limits that separate one's knowledge from one's ignorance and that makes one into the other. Only thus is one determined to write. To defeat ignorance is to postpone writing or even to make it impossible" (1968: 4).

The face of perspective: to write about something means to write from the side of it never from the centre. That is why the perspective is the essence of writing. On the contrary, the centre is perspectiveless as the evidence of totality subverts any attempt to define profiles. The one at the centre is in the agonic position of Lewis Carrol's captain in the Hunting of the Snark:
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'What's the good of Mercator's North Pole and Equators, Tropics, Zones and Meridian Lines?' So the Bellman would cry: and the crew would reply 'They are merely conventional signs!' ***

'Other maps are such shapes, with their islands and capes! But we've got our brave captain to thank' (So the crew would protest) 'that he's bought us the best — A perfect and absolute blank!' (1946:236).

19. Two major factors, one intellectual and one political, were determinant in this process. Firstly, my long discussions with Ivan Illich in 1972 and 1974 at CIDOC in Guernavaca. It was he—who himself refused a Marxist framework—who taught me how to use theoretical tools to reach beyond the "normal" areas of Marxist thought. In this respect I must also mention discussions I had in 1972 with Vieira Gallo (at the time Secretary of Justice in Allende's government of Unidad Popular), and in 1974 with Andre Gorz (French philosopher and politician) and Francisco Juliao (organizer of Ligas Campesinas in Northeast Brazil). The other determinant factor was the revolution of April 25, 1974, in Portugal, and my subsequent political action both at the University and in peasants' cooperatives.

Publications Arising from this Research


Other References


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