



Vive la Crise!: For Heterodoxy in Social Science

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Theory and Society, Vol. 17, No. 5, Special Issue on Breaking Boundaries: Social Theory and the Sixties. (Sep., 1988), pp. 773-787.

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Vive la crise!

*For heterodoxy in social science*¹

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The crumbling of orthodoxy and its legacy

When I was invited to take part in the creation of *Theory and Society*, I saw in the advent of this new journal, which made a first dent in the monolithic bloc of the sociological establishment, a symptom of a profound change in the social sciences. In point of fact, *Theory and Society* was to become the global rallying point of all the dominated and marginal sociological currents, some of which have since undergone a spectacular and healthy development. As one might gather, I did not despair over what some described as a crisis, namely the destruction of the academic temple, with its Capitoline triumvirate and all its minor gods, which dominated world sociology during the fifties and early sixties. Indeed, I think that for a variety of converging reasons, including the desire to give sociology a scientific legitimacy — identified with academic respectability and political neutrality or innocuousness — a number of professors, who held the dominant positions in the most prominent American universities, formed a sort of “scientific” oligopoly and, at the cost of mutual concessions, elaborated what Erving Goffman calls a *working consensus* designed to give sociology the appearance of a unified science finally freed from the infantile disorders of the ideological war of all against all. This fiction of unanimity, which some today still strive to restore, resembled that of those religious or juridical orthodoxies that, being entrusted with the preservation of the symbolic order, must first and foremost maintain consensus within the community of doctors. This *communis doctorum opinio*, a social fiction artificially created and supported, is the absolute antithesis of the agreement, at once full and provisional, over the body of collective achievements of a scientific discipline — principles, methods of analysis, procedures of verification, etc. — which, far from serving to produce a sham consensus, make possible the merciless and regulated

confrontations of scientific struggle, and thereby the progress of reason.²

Thus there is no reason to mourn the crumbling of an orthodoxy. At the same time, however, one must recognize that the complementary oppositions, the oppositions within complementarity, which were the pillars of the old division of the labor of scientific domination can survive the waning of the fiction of synthesis that crowned it. The gap between what in the United States, and in all the countries dominated by the American academic model, is called theory and what is called empirical research has perhaps never been wider than at present. Although the greatness of American social science lies, in my eyes at least, in those admirable empirical works containing their own theory produced particularly at Chicago in the forties and fifties but also elsewhere, as with the spate of remarkable studies now coming from the younger generation of social scientists and historical sociologists, the intellectual universe continues to be dominated by academic theories conceived as simple scholastic compilation of canonical theories. And one cannot resist the temptation to apply to the “neo-functionalists,” who today are attempting a parodic revival of the Parsosian project, Marx’s word according to which historical events and characters repeat themselves, so to speak, twice, “the first time as tragedy, the second as farce.”

Such “theoretical” theory, a prophetic or programmatic discourse that is its own end, and that stems from and lives from the confrontation with other (theoretical) theories (as in its French neo-Marxist version, which reduced it to a pure exercise in the reading of canonical texts), naturally forms an “epistemological couple,” as Bachelard would put it, with what in American social science is called “methodology.” This compendium of scholastic precepts (such as the requirement of preliminary definitions of concepts, which automatically produce a closure effect) and of technical recipes, whose formalism (as, for instance, in the presentation of data and results) is often closer to the logic of a magic ritual than to that of a rigorous science, is the perfect counterpart to the bastard concepts, neither concrete nor abstract, that pure theoreticians continually invent. Despite its pretense of utmost rigor, this formalism paradoxically abstracts from critical assessment the concepts used and the most fundamental operations of research, such as data coding procedures and choice of statistical techniques of analysis.

Thus, if you will allow me to plagiarize Kant’s famous dictum: theory without empirical research is empty, empirical research without theory

is blind. There would be no need reasserting such truisms if the division between theoreticist theory and empiricist methodology were not sustained by extraordinary social forces: it is in effect inscribed in the very structure of the academic system and, through it, in mental structures themselves. So that even the most innovative and fruitful attempts to break free from this dualism end up being crushed by the pincer of abstract typologies and testable hypotheses.

I see yet another manifestation of the final revenge of this infernal couple constituted by scholastic theory and positivist methodology in the recent development of a form of critique of anthropological practice whose major function seems to be to allow its authors simply to recount their lived experiences in the field and with the subjects studied rather than critically examine what the study should have taught them, when it does not take the place of fieldwork pure and simple. Having relentlessly worked to uncover the implicit presuppositions of the position of the observer who retires from practice in order to reflect on it (particularly in *Outline of a Theory of Practice* and *Le métier de sociologue*),³ I will not, I hope, be suspected of scientific complacency if I deplore these sudden fits of indiscriminate reflexivity that have led certain anthropologists to follow philosophical essayism in its endless fight against the very possibility of a science of man. Such falsely radical denunciations of anthropological writing as “poetics and politics” have nothing in common with the most radical critique of the presuppositions and prejudices of a scientific methodology that unthinkingly obeys the reflexes of techniques learned or the personal biases of the researcher. (I think for instance of the devastating critique by Aaron Cicourel of bureaucratic statistics.)⁴ In fact, these rhetorical ruptures with rhetorics leave untouched and *undiscussed* most of what can be brought to light by a reflective return on scientific practice and its instruments that is not an end in itself but genuinely aims at improving this practice.

To strip my remarks of the sovereignly programmatic and thereby deliciously gratuitous air of so-called “theoretical” discourse, I illustrate with an example from my recent research on the French *Grandes Ecoles* how the exclusive attention to the methods of data collection and analysis promoted by the dominant conception of science fosters a sort of blindness for the operations, most often unconscious, by which a research object is constructed. Owing to their offering a particularly favorable opportunity to capture the contribution that “elite schools” make to the reproduction of a dominant class, the various *Grandes*

Ecoles have been studied profusely, by historians as much as by sociologists, both French and American. Now, of these very numerous investigations, many with apparently impeccable “methodologies,” every last one begins by an extraordinary *petitio principii* by taking as its object *one and only one* particular school, considered diachronically or synchronically. (This would be analogous to studying Princeton University independently of its position within the Ivy League and, through it, within the broader system of American universities.) By bracketting the crucial fact that each school is situated in the space of French institutions of higher learning and that it owes a number of its most distinctive properties to the set of objective relationships it holds with other schools, i.e., to its position within the *field* of tertiary education and the subfield of *Grandes Ecoles*, the initial definition of the object nearly completely destroys the very object it pretends to grasp.⁵ I need not add that no one has ever taken exception to what, in my view, constitutes a major theoretical and empirical mistake, about as glaring as the idea of studying a heavenly body without considering its relations to other such bodies in the solar system. This is the kind of mistake that even the most supercilious of “methodologists” themselves are inclined to make every time they forget to pose explicitly the question of the construction of the theoretical object that governs the construction of the empirical object (population, body of texts, etc.) through which the latter can be grasped, or when they dispose of this problem with those falsely self-conscious decisions labelled “operational definitions” (“I shall call ‘intellectual’ ... I shall define the ‘middle class’ as ... I shall consider as ‘deviant’ ...”) that consist of settling on paper issues that are not settled in reality, where they are the stake of ongoing social struggles.

To understand why, contrary to all expectations, such trivial questions are so seldom asked, we need only note that the choices of objects of study have all appearances in their favor when they simply take over the constructions of common sense and the definitions of everyday discourse, which designates and assigns to so many researchers so many of their objects. A social reality, whether an agent or an institution, presents itself all the more easily, provides all the more readily what are called “data” the more completely we agree to take it as it presents itself. Documents, starting with official statistics, are the objectivized product of *strategies of presentation of self*, which institutions, like agents, perform continually, though not always consciously. Thus the primary (mental) representations we have of institutions are for the most part nothing but the product of the work of (theatrical) representation that

they spontaneously stage and that a good many sociologists do nothing more than record at great expense.

Social science must break with the preconstructions of common sense, that is, with “reality” as it presents itself, in order to construct its proper objects, even at the risk of appearing to do violence to that reality, to tailor the “data” to meet the requirements of scientific construction, or simply to be faced with a sort of empirical void, as when the requisite information is incomplete or impossible to compare, or, worse, does not exist and cannot be produced. One of the major obstacles to progress in the social sciences no doubt resides in this formidable gap between strict compliance with the rules of proper scientific conduct, as they are defined by the methodological *doxa* taught in universities, and true scientific virtues. Indeed, it is not uncommon for the requirements of real rigor to force one to violate the most apparent forms of positivist rigor that are the more easily applied, the more fully one accepts the common vision of social reality. In short, studies that simply confirm the constructions of common sense and ordinary discourse by transcribing everyday assumptions into scientific definitions have every chance of being approved by the scholarly community and its audiences, especially if they comply strictly with the more superficial rules of scientific discipline, whereas research that breaks with the false obviousness and the apparent neutrality of the constructions of common sense — including scholarly common sense (*sens commun savant*) — is always in danger of appearing to be the result of an act of arbitrary imposition, if not of ideological bias, and of being denounced as deliberately producing the data fit to validate them (which all scientific constructions do).

Beyond the false antinomies of social science

The opposition between empty theoreticism and blind empiricism, however, is but one of the many antagonistic pairs (*couples ennemis*), or antinomies, which structure sociological thought and practice and hinder the development of a science of society capable of truly cumulating its already immense achievements. These oppositions, which Bendix and Berger called “paired concepts” (object/subject, materialism/idealism, body/mind, etc.), are ultimately grounded in social oppositions (low/high, dominant/dominated, and so on.) Like any institution, they have a double existence: they exist first in objectivity as academic departments, professional associations, scholarly networks,

and individual researchers committed to, or identified with, different theories in -ism, concepts, methodologies, paradigms, disciplinary sub-fields, etc.; and they exist also in subjectivity, as mental categories, principles of vision, and division of the social world. In the case of academic life, the production and reproduction of these categories obtain mainly through course offerings, assigned readings, and lecture materials that are tailored to the divisions that professors establish, for the sake — or under the pretext — of clarity and simplicity.⁶

These paired oppositions construct social reality, or more accurately here, they *construct the instruments of construction of reality*: theories, conceptual schemes, questionnaires, data sets, statistical techniques, and so on. They define the visible and the invisible, the thinkable and the unthinkable; and like all social categories, they hide as much as they reveal and can reveal only by hiding. In addition, these antinomies are at once descriptive and evaluative, one side being always considered as the “good one,” because their use is ultimately rooted in the opposition between “us” and “them.” Academic struggles are only a particular case of the symbolic struggles that go on in everyday life, though strategies of academic domination generally take on a more disguised form. In the scientific field, insults are highly euphemized, transformed into names of concepts and analytical labels, as when, for instance, a critic says that I hold a “semi-conspirational, semi-functionalist” view of society. In academic debate, symbolic murders take the form of snide comments, essentialist denunciations (akin to racism) couched in classificatory terms: so and so is a Marxist, so and so is a “theorist” or a “functionalist,” etc. Suffice to say here that manichean thought is related to manichean struggles.⁷

Let me examine some of these antinomies that, in my view, are profoundly harmful to scientific practice. First, there are the *oppositions between disciplines*. Take the opposition between sociology and anthropology: this absurd division, which has no foundation whatsoever except historical and is a prototypic product of “academic reproduction,” favors uncontrolled borrowing and generalization while forbidding genuine cross-fertilization. For instance, I believe that I could not have understood all that I now express with the concept of “symbolic capital” if I had not analyzed honor strategies among Algerian peasants as well as the strategies of firms competing in the field of high fashion.⁸ Similarly the sociology of modes of domination and group formation can be thoroughly transformed by applying to the analysis of classes the results and methods of the cognitive anthropology of taxonomies

and cultural forms of classification.⁹ Cross-fertilization, however, must not in this case be confounded with what I call “anthropologism”: the simple projection onto advanced societies of such half-mastered notions as ritual or magic, as is done when the annual Christmas office party is described as a “bureaucratic ritual.” Rather, a rigorous analysis of such phenomena as the label (*griffe*) of the fashion designer or the signature of the great painter reveals that the real principle of the efficacy of the magical power that Marcel Mauss was tracking in his *Essay on Magic* lies in the field of the agents and institutions involved in the production and reproduction of the collective belief in their value.¹⁰

The same argument could be made about the divisions between history and sociology, or history and anthropology, not to mention economics. I think that the inclination to view society in an ahistorical manner — which is the hallmark of much American sociology — is implied by this simple division. Many scientific mistakes would be avoided if every sociologist were to bear in mind that the social structures he or she studies at any given time are the products of historical development and of historical struggles that must be analyzed if one is to avoid naturalizing these structures. Even the words we employ to speak about social realities, the labels we use to classify objects, agents and events, like the names of occupations and of groups, all the categorial oppositions we make in everyday life *and* in scientific discourse are historical products. Durkheim wrote in *The Evolution of Educational Thought* that “the unconscious is history” and this is especially true of the scientific unconscious. For this reason, I think that the social history of science — in the tradition represented in France by Gaston Bachelard, Georges Canguilhem, and Michel Foucault — should be a necessary part of the intellectual tool-kit of all social scientists.

Among the antinomies that divide every discipline into specialties, schools, clans, etc., one of the most senseless and ill-fated is the *division into theoretical denominations*, such as Marxists, Weberians, Durkheimians, and so on. I am at a loss to understand how social scientists can indulge in this typically archaic form of classificatory thinking, which has every characteristic of the practical logic at work in primitive societies (with the founding fathers acting as mythical ancestors), and is essentially oriented toward the accumulation of symbolic capital in the course of struggles to achieve scientific credibility and to discredit one’s opponents. It is difficult to overestimate all that is lost in such sterile divisions and in the false quarrels they elicit and sustain. For me, the question of allegiance to the founding fathers of the social sciences is

reduced to the following: whether or not to be a Marxist or a Weberian is a religious alternative, not a scientific one. In fact, one may — and should — use Weber against Weber to go beyond Weber. In the same way, one should follow Marx's advice when he said "I am not a Marxist," and be an anti-Marxist Marxist. One may think with Weber or Durkheim, or both, against Marx to go beyond Marx and, sometimes, to do what Marx could have done, in his own logic. Each thinker offers the means to transcend the limitations of the others. But a "*Realpolitik* of the concept" capable of avoiding eclecticism presupposes a prior understanding of the structure of the theoretical space in which fictitious antinomies emerge in the first place.¹¹

If space permitted, I would discuss a whole series of secondary oppositions that haunt, like theoretic ghosts, the academic mind: micro-versus macro-sociology, quantitative versus qualitative methods, consensus versus conflict, structure versus history, etc. Extreme posturing within the academic field around such paired oppositions seems to appeal to rigid, dogmatic minds and, like in politics, sudden conversions from one extreme to its opposite frequently occur. (It is not uncommon to see a scholar shift, in the course of a career, from blind scientism to irrationalist nihilism, the former paving the way for the latter.)

But all these oppositions remain external to the core of scientific theory. I want to come now to the rock-bottom antinomy upon which all the divisions of the social scientific field are ultimately founded, namely, the *opposition between objectivism and subjectivism*. This basic dichotomy parallels a whole series of other oppositions such as materialism versus idealism, economism versus culturalism, mechanism versus finalism, causal explanation versus interpretive understanding. Just like a mythological system in which every opposition, high/low, male/female, wet/dry, is overdetermined and stands in homologous relations to all the others, so also these scientific oppositions contaminate and reinforce each other to shape the practice and products of social science. Their structuring power is the greatest whenever they stand in close affinity with the fundamental oppositions, such as individual versus society (or individualism versus socialism), that organize the ordinary perception of the social and political world. Indeed, such paired concepts are so deeply ingrained in both lay and scientific common sense that only by an extraordinary and constant effort of epistemological vigilance can the sociologist hope to escape these false alternatives.

Let me now address briefly some aspects of this basic “theoretical” opposition in order to show how it may be overcome. At the most general level, social science oscillates between two apparently contradictory perspectives: objectivism and subjectivism, or social physics and social semiotics or social phenomenology. On the one hand, sociology can follow the old Durkheimian precept and “treat social facts as things.” Such an approach leads to ignoring all those properties that social facts have by virtue of being objects of knowledge, true or false, of recognition and misrecognition, in reality itself. This objectivist position is represented today in American social science by functionalism, evolutionary and ecological approaches, network theory, and dominates most of the specialized subfields dealing with institutions (such as formal organizations or stratification) from an external standpoint. At a more “methodological” level, this structuralist point of view is oriented toward the study of objective mechanisms or deep latent structures and the processes that produce or reproduce them. This approach relies on objectivist techniques of investigation (e.g., surveys, standardized questionnaires) and embodies what I call a technocratic or *epistemocratic vision* in which only the scholar is able to gain a complete picture of the social world, which individual agents apprehend only partially. Durkheim expresses this view in paradigmatic form when, in a typically objectivist manner, he counterposes the scientific vision of the whole to the private, partial, particular, and therefore erroneous, vision of the individual lay person.

On the other hand, sociology can reduce the social world to the mere representations that agents have of it; the task of science then becomes one of producing a meta-discourse, an “account of the accounts,” as Garfinkel puts it, given by social agents in the course of their everyday activities. Today this subjectivist position is represented mainly by symbolic anthropology, phenomenological and hermeneutic sociology, interactionism, and ethnomethodology. (Admittedly, these two opposing perspectives are very rarely found in the pure form I am describing.) In terms of method, this point of view is generally associated with the so-called “qualitative” or naturalistic methods, such as participant observation, ethnography, discourse analysis, or self-analysis. In the eyes of the objectivist or “hard” social scientists, it represents the quintessential expression of “fuzzy-wuzzy” sociology. Ironically, though, this academically derogated manner of looking at the social world is generally closer to reality, more attentive to the concrete and detailed aspects of institutions than is the objectivist approach. Moreover, this “soft” sociology is often more inventive, imaginative,

and creative in its investigations than is the hard machinery of these survey bureaucracies that, on behalf of a division of labor that gives the questionnaire to professors and relegates the questioning to students or to professional interviewers, hinder direct contact between the researcher and this reality he or she claims to describe empirically. To take up the point of view of the agent makes the subjectivistically-inclined sociologist less prone to indulge in those all-encompassing and arrogant visions of social life that place the scientist in a position of divine mind.

As I have tried to demonstrate throughout most of my work, I believe that true scientific theory and practice must overcome this opposition by integrating into a single model the analysis of the experience of social agents and the analysis of the objective structures that make this experience possible. To unpack this statement fully would require that I explicate here the social philosophy implied in the notion of *point of view*. In a nutshell: the agent's point of view that science, in its subjectivist moment, must take up, describe, and analyze can be defined as a view taken from a point; but to understand fully what it means to be located at this point and to see what can be seen from it, one must first construct the space of the mutually exclusive points, or positions, within which the point under consideration is situated.¹² Because this may sound a bit obscure, I restate my position as follows. On the one hand, the objective structures that the sociologist constructs in the objectivist moment by sweeping aside the subjective representations of agents (which Durkheim and Marx always do) provide the foundation of these subjective representations and determine the set of structural constraints that bear on interactions. On the other hand, however, these representations themselves must, in a second moment, be reappropriated into the analysis if one wants to account for the everyday struggles in which individuals and groups attempt to transform or preserve these objective structures. In other words, these two moments, the subjectivist and the objectivist, stand in dialectical relationship.¹³

It is this dialectic of objectivity and subjectivity that the concept of *habitus* is designed to capture and encapsulate.¹⁴ The habitus, being the product of the incorporation of objective necessity, of necessity turned into virtue, produces strategies which are objectively adjusted to the objective situation even though these strategies are neither the outcome of the explicit aiming at consciously pursued goals, nor the result of some mechanical determination by external causes. Social action is guided by a practical sense, by what we may call a "feel for the game."

Even when practice appears as rational action to an impartial observer who possesses all the necessary information to reconstruct it as such, rational choice is not its principle. Indeed, *social action has nothing to do with rational choice*, except perhaps in very specific crisis situations when the routines of everyday life and the practical feel of habitus cease to operate. (As Leibniz said in opposition to Descartes, who was the first proponent of Rational Action Theory: “We are empirical — i.e., practical — in three quarters of our actions.”) Consider the case of a tennis player who suddenly “decides” to rush the net, or the quarterback who “decides” to pull out of the pocket and scramble, to understand that action has, in practice, nothing in common with the “theoretical” (*theorein*, it may be recalled, means to see, to contemplate) reconstruction of the play by the coach or the TV commentator after the game.

The conditions for rational calculation almost never obtain in practice where time is scarce, information limited, alternatives ill-defined, and practical matters pressing. Why, then, do agents “do the only thing that is to be done” more often than chance would predict? Because they practically anticipate the immanent necessity of their social world, by following the intuitions of a practical sense that is the product of a lasting subjection to conditions similar to the ones they are placed in. It is this conception of social action as the product of a *practical sense*, as a social *art* (i.e., “pure practice without theory,” as Durkheim puts it), that I try to elaborate empirically in my book *Distinction: A Social Critique of the Judgement of Taste*, which some critics, such as Elster,¹⁵ have thoroughly misconstrued. In that book, I argue that members of the dominant class, being born into a positively distinguished position, appear as distinguished simply because their habitus, as a socially constituted nature, is immediately adjusted to the immanent requirements of the social and cultural game. They can thus assert their difference, their uniqueness, without consciously seeking to do so. The hallmark of naturalized distinction is when appearing distinguished amounts to no more than being oneself. The sort of conscious search for distinction described by Thorstein Veblen and postulated by the philosophy of action of rational choice theory is in fact the very negation of distinguished conduct as I have analyzed it, and Elster could not be farther from the truth when he assimilates my theory to Veblen’s. For the habitus, standing in a relation of true *ontological complicity* with the field of which it is a product, is the principle of a form of knowledge that does not require consciousness, of an intentionality without intention, of a practical mastery of the regularities of the world that allows one to

anticipate its future without having to pose it as such. We find here the foundation of the distinction drawn by Husserl, in *Ideen I*, between *protension* as the practical aiming of a future-to-be inscribed in the present, and thus grasped as already there and endowed with the doxic modality of the present, and *project* as the positing of a future constituted as such, that is, as something that can happen or not. It is because he fails to understand this distinction, which defies that between the conscious and the unconscious, that Elster smuggles back into the social sciences, under the revamped label of methodological individualism and rational choice, that old philosophy of the free subject and, along with it, an imaginary anthropology no different from Sartrean intellectualism.¹⁶

I could develop in depth the analysis of the two-way relationship between habitus and field, where the field, as a structured space, tends to structure the habitus, while the habitus tends to structure the perception of the field.¹⁷ But I would prefer, by way of conclusion, to emphasize the main practical consequence that can be achieved by transcending the antinomy between objectivism and subjectivism. It lies in the possibility of overcoming the opposition between objective observation or measurement on the one hand, and subjective participation or self-analysis on the other. Social analysis must involve more than merely combining the statistical objectivation of structures with interpretive accounts of the primary experiences and representations of agents. To capture the gist of social action necessitates what I call *participant objectivation*: to realize not only the objectivation of the object of study but also, as I have tried to do in my own work, whether it be on French peasants or on French academics, the objectivation of the objectifier and of his gaze, of the researcher who occupies a position in the world he describes and especially in the scientific universe in which scholars struggle over the truth of the social world.¹⁸

By turning the instruments of social science back upon himself, in the very movement whereby he constructs his objects, the social scientist opens up the possibility of escaping yet another fateful, and apparently insuperable, antinomy: that between historicism and rationalism. A genuinely reflexive social science, then, gives its practitioners appropriate motives and appropriate weapons for grasping and fighting the social and historical determinants of scientific practice.

Notes

1. This article is based on notes prepared for an invited lecture entitled "Beyond the False Antinomies of Social Science," delivered at the Department of Sociology at the University of Chicago in April 1987. Since these notes were originally prepared for oral presentation with the intention of provoking discussion rather than for publication, certain passages have been omitted and others added to elaborate briefly on remarks that could only be sketched out in the public presentation. The translation and bibliographic notes are by Loïc J. D. Wacquant.
2. For an elaboration of this argument, see Pierre Bourdieu, "The Specificity of the Scientific Field and the Social Conditions of the Progress of Reason" in Charles C. Lemert, editor, *French Sociology: Rupture and Renewal Since 1968* (New York: Columbia University Press, 1981, 257–292) and "The Peculiar History of Scientific Reason" (forthcoming).
3. Pierre Bourdieu, *Outline of a Theory of Practice* (Cambridge: Cambridge University Press, 1977), *Le sens pratique* (Paris: Editions de Minuit, 1980), and Pierre Bourdieu, Jean-Claude Chamboredon, and Jean-Claude Passeron, *Le métier de sociologue. Préalables épistémologiques* (Paris and The Hague: Editions Mouton, 1968, 2nd ed. 1973).
4. See Aaron V. Cicourel, *Method and Measurement in Sociology* (New York: The Free Press, 1964), *The Social Organization of Juvenile Justice* (New York: Wiley, 1968), and *Theory and Method in a Study of Argentine Fertility* (New York: Wiley, 1974).
5. A fuller demonstration of this point is found in Pierre Bourdieu and Monique de Saint Martin, "Agrégation et ségrégation. Les Grandes Ecoles dans le champ du pouvoir," *Actes de la recherche en sciences sociales* 69 (September 1987): 2–50, particularly part I, "La méthode en question." For other applications of the *relational mode of thinking* called for by the concept of field (as a structured space of objective relations between positions defined by force lines and struggles over specific stakes or forms of capital), see: on the religious field, Pierre Bourdieu, "Genèse et structure du champ religieux," *Revue française de sociologie* 12–3 (September 1971): 295–334, "Legitimation and Structured Interests in Weber's Sociology of Religion," in Sam Whimster and Scott Lash, editors, *Max Weber, Rationality and Modernity* (London: Allen and Unwin, 1987), 119–136, and Pierre Bourdieu et Monique de Saint Martin, "La sainte famille. L'épiscopat français dans le champ du pouvoir," *Actes de la recherche en sciences sociales* 44–45 (November 1982): 2–53; on the literary and artistic field, see Pierre Bourdieu, "The Field of Cultural Production, or the Economic World Reversed," *Poetics* 12 (November 1983): 311–356. "The Historical Genesis of a Pure Aesthetic," *The Journal of Aesthetics and Art Criticism*, special issue on "Analytic Aesthetics" (1987): 201–210, and "Flaubert's Point of View," *Critical Inquiry* 14 (Spring 1988): 539–562; on the field of law, "The Force of Law: Toward a Sociology of the Juridical Field," *The Hastings Law Journal* 38 (1987): 201–248; and on the philosophical field, "The Philosophical Establishment," in A. Montefiore, editor, *Philosophy in France Today* (Cambridge: Cambridge University Press, 1983), and *L'ontologie politique de Martin Heidegger* (Paris: Editions de Minuit, 1988).
6. For an exploration of these categories and their reproduction in the French educational system, see Pierre Bourdieu et Monique de Saint Martin, "Les catégories de l'entendement professoral," *Actes de la recherche en sciences sociales* 3 (May 1975): 68–93, and Pierre Bourdieu, "Systems of Education and Systems of

- Thought,” in Roger Dale et al., *Schooling and Capitalism* (London: Routledge and Kegan Paul, 1976), 192–200.
7. For an illustration of this logic of opposition, orthodoxy, and heresy in a scientific field, see Sherry B. Ortner’s account of the conflict, at once mental and social, between symbolic anthropologists and cultural ecologists in American anthropology (“Theory in Anthropology Since the Sixties,” *Comparative Studies in Society and History* 26 (1984): 126–166), and Pierre Bourdieu: “Scientific Field and Scientific Thought: Some Notes on Sherry Ortner’s Article,” paper read at the Annual Meetings of the American Anthropological Association, Chicago, October 1987 (forthcoming).
 8. Pierre Bourdieu, “The Sense of Honor,” in *Algeria 1960* (Cambridge: Cambridge University Press, 1979), 95–132, and Pierre Bourdieu and Yvette Delsaut, “Le couturier et sa griffe. Contribution à une théorie de la magie,” *Actes de la recherche en sciences sociales* 1 (January 1975): 7–36. On the concept of capital and its uses, Pierre Bourdieu, “Three Forms of Capital,” in John G. Richardson, editor, *Handbook of Theory and Research for the Sociology of Education* (New York: Greenwood Press, 1986), 241–258.
 9. Pierre Bourdieu, *Distinction: A Social Critique of the Judgement of Taste* (Cambridge, Mass.: Harvard University Press, 1984), esp. 466–484, and “What Makes a Class? On the Theoretical and Practical Existence of Groups,” *Berkeley Journal of Sociology* 22 (1987): 1–18.
 10. See Pierre Bourdieu, “The Production of Belief: Contribution to an Economy of Symbolic Goods,” *Media, Culture and Society* 2 (July 1980): 261–293, and *Language and Symbolic Power* (Cambridge: Polity Press, 1988).
 11. The basic contours of such a theoretical space are mapped out in Pierre Bourdieu, “Symbolic Power,” *Critique of Anthropology* 13/14 (Summer 1979): 77–85. For an account of Bourdieu’s strategy of synthesis between different theoretical traditions, see Axel Honneth, Hermann Kocyba, and Bernd Schwibs, “The Struggle for Symbolic Order: An Interview with Pierre Bourdieu,” *Theory, Culture and Society* 3 (1986): 35–51.
 12. See Bourdieu, “Flaubert’s Point of View,” *Critical Inquiry*, for an exemplification of the mode of analysis implied by this conception.
 13. A more detailed statement of this position is presented in “Social Space and Symbolic Power” (lecture given at the University of California at San Diego, April 1986, forthcoming). See also Pierre Bourdieu, “Social Space and the Genesis of Groups,” *Theory and Society* 14 (November 1985): 723–744, and *Choses dites* (Paris: Editions de Minuit, 1987).
 14. Habitus may be defined as a system of durable and transposable *dispositions* (schemes of perception, appreciation and action), produced by particular social environments, which functions as the principle of the generation and structuring of practices and representations. The philosophical genealogy and theoretical purposes of the notion of habitus are outlined in Pierre Bourdieu, “The Genesis of the Concepts of Habitus and Field,” *Sociocriticism*, 2 (December 1985): 11–24.
 15. Jon Elster, *Sour Grapes* (Cambridge: Cambridge University Press), especially 69, 76, and 105–106, and *Ulysses and the Sirens: Studies in the Subversion of Rationality* (Cambridge: Cambridge University Press, 1979, rev. ed. 1984).
 16. See particularly *Le sens pratique*, chapter 2.
 17. On this meeting of “objectified history” and “embodied history,” of position and disposition, see Pierre Bourdieu, “Men and Machines,” in Karen Knorr-Cetina and Aaron V. Cicourel, editors, *Advances in Social Theory and Methodology: Toward*

- an Integration of Micro- and Macro-Sociologies* (Boston: Routledge and Kegan Paul, 1981), 304–317, “Flaubert’s Point of View,” and *L’ontologie politique de Martin Heidegger*, particularly chapter 2.
18. Pierre Bourdieu, “Célibat et condition paysanne,” *Etudes rurales* 5–6 (April–September 1962): 32–136; “Marriage Strategies as Strategies of Social Reproduction,” in R. Foster and O. Ranum, editors, *Family and Society: Selection from the Annales* (Baltimore: The Johns Hopkins University Press, 1976), 117–144; “From Rules to Strategies,” *Cultural Anthropology* 1 (February 1986): 110–120; *Homo Academicus* (Paris: Editions de Minuit, 1984), and “Preface” to the English translation, (Cambridge: Polity Press, 1988), and “Objectiver le sujet objectivant,” in *Choses dites*, 112–116.