ACCOUNTING AS A HUMAN SCIENCE

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Abstract

Accounting may well be on its way to becoming the queen of the human sciences, but, regrettably, if so, this will be because it has taken on all the least desirable features of technocratically defined "human" science. The critics of accounting practice see this and although they differ among themselves, the differences between the critics and the mainstream practitioners (and their "theorists") define a chasm. I try to exhibit this by showing that the chasm stems from different assumptions in the philosophy (or theory) of science and different assumptions regarding the nature of society. Current criticisms suggest to me a profound pedagogic task.

As with all human practices, the ensemble of practices which we call "accounting" involves a collocation of beliefs, prior practices and materials each with histories, some long, some of more recent beginnings. Most of these are utterly taken for granted. For example, there was the invention of writing, as in Aegean Linear B, an invention which, importantly, allowed for a non-syntactical, non-contextual use of language which was admirably suited to the first accounting practices. When humans found a way to represent graphically, out of time and out of space, abstracted kinds, quantities and relations, "communication", "administration" and "surveillance" took a giant step toward what today we think of as accounting (Goody, 1986, p. 54; Giddens, 1985, pp. 41-49; Foucault, 1977; and cf. Arrington & Francis, 1989b). But record-keeping is surely not the whole of it. Any adequate story would, no doubt, include reference to the changing ways things have been measured and exchanged and how money became a representation of value (Tinker, 1985). It would include, then, an account of how these representations were themselves represented in systems of accounting. It would surely include reference to capitalist development, how, fairly recently, we came upon the idea that problems of allocation and distribution were solved by markets, and how conflicts and interests generated by these developments promoted (and still promote) changes in accounting practices.1 And it would include how, even more recently, first in Britain, and then elsewhere, "chartered accountants" rejected the traditional book-keeping role for a new legitimating role as public auditors (Hoskin & Macve, 1986, 1988). To this point, until perhaps only the recent past, no one, I think, would have thought of accounting as a human science. Perhaps even today most people think of the accountant as the book-keeper, the innocent keeper of the records. But surely this is a mistake. Indeed, might it be that

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1 Recent work by Berry et al. (1984) and Burchell et al. (1985) is exemplary concrete "sociology" of accounting and is to be warmly endorsed. So too the "genealogical" inquiries of Hoskin & Macve, Miller & O'Leary, and Hopwood. To these must be added work in the political economy of accounting, national and transnational. As I suggest, these are indispensable and non-competing tasks, for reasons which I hope to make clear. For concrete discussion of some of the stunning interconnections between international finance capital, politics and local accounting practices in a Third World country, see Arroyo (1971). My thanks to Bob Stauffer for the reference.
today accounting is not merely a human science but that it is rapidly becoming the queen of the human sciences, that not only has it become "scientific", but that in today's modern information society, the men and women of Coopers and Lybrand, Price Waterhouse, Peat, Marwick et al. are the people with the most critical expertise and knowledge of society and the modern economic order! To see what is at issue here, we need to say more about the human sciences, but especially to be clear about how, in the modern university, their methods and tasks have been defined and institutionalized.

THE MODERN UNIVERSITY AND THE MODERN SOCIAL SCIENCES

As I have argued elsewhere (Manicas, 1987), what we think of as the disciplines of the modern social sciences emerged only during the two decades at the turn of the last century; and they emerged, for good historical reasons, in the entirely new universities of the United States. Prior to this time, America had colleges — undergraduate teaching institutions — but it had no centers of advanced learning and research, either in the sciences or in the professions. The curricula of America's clerically dominated colleges owed to the British derived (and medievally rooted) division between natural philosophy — physics and mathematics — and moral philosophy. Professors of moral philosophy, often clerics, offered texts which might range from Aristotle's Politics to the political economy of John Stuart Mill. The centers of learning in Britain, Oxford and Cambridge, and in France, the Sorbonne, remained fixed in medieval assumptions about science and learning. Things were different in Germany where the University of Berlin, founded only in 1810, became the focus for Bismarck's accelerated program of modernization "from above". The Germans made the discovery that university-based science, linked to modern industry, could quickly push Germany forward.

As in Germany, post-civil war America underwent a fantastic capitalist development. In marked contrast to Europe, the United States had been "bourgeois" from its beginning and in contrast to Germany, the state in America was weak. But if it thus lacked the legacies of a feudal past, including a traditional class-based system of authority reproduced in a university system which was the instrument of the older order, it could have science. When faced with the consequences of rapid capitalist development — urbanization, immigration, poverty and class war — Americans, proceeding from where they were, generated solutions which were consistent with both middle-class dominance and the middle-class ideology of freedom. Shored by some carefully directed violence, racist and ethnic politics could easily be joined to the politics of economic growth and, in turn, both could be joined to technocratic solutions to the social problem (Weinstein, 1968; Kolko, 1967). The creation of the university and the de novo institutionalization of the new disciplines of the social sciences came to play a critical role in this.

This story, of course, is complex, but the critical ingredient was the opportunity seized by a new breed of "educational managers" who, in cooperation with the corporate liberal leaders of America's largest corporations and financial institutions — the Carnegies, Rockefellers, Cornells, Hopkins, Clarks, Vanderbilts and Stanfords — reconstituted higher education in America (Silva & Slaughter, 1984). In some 25 years, a host of new private institutions came into existence. All of these were universities in the sense that they had graduate programs organized to propel specialized knowledges. And in every case, these knowledges were specifically technocratic in the sense that they were aimed at the prediction and control of the world. Spurred by these changes, the traditional colleges of the East reluctantly but rapidly transformed themselves into modern universities. Nor was interest by government lacking. Specifically designed as centers for technological creation and dissemination, dozens of new land-grant institutions spawned by the
Morrill Act of 1862 came into existence. Undergraduate enrollments escalated, from 154,000 in 1890 to 582,000 in 1920, and graduate degrees, unknown in the United States before the 1860s, went from 2400 in 1890 to 15,600 by 1920 (McClelland, 1980).

This transformation was revolutionary. However, it is usually not seen that this development was more than the indispensable material condition for what John Dewey called "the new body of studies" that emerged out of the traditional curriculum of moral philosophy. Given the conditions of this development it was, as well, a fundamental constraint on what this new body of studies was to become. Themselves "progressives", faculties in the universities shared in propelling the new development. Reinforced by a recently hegemonic philosophy of science (below), moral philosophers were not reluctant to become scientists. But they could do this only if they could strip their inquiries of pre-scientific assumptions and only if they could establish a special domain or central problem about which they could speak authoritatively. Scientific neutrality thus gave them both legitimacy and political impact.

The struggle to mark out territories became easier once it was seen that the game need not be zero-sum. Once boundaries had been drawn, each could professionalize its own curriculum. The awarding of a Ph.D. degree could warrant "experts" and at the same time serve to maintain disciplinary boundaries. No conspiracy was needed. Working with materials at hand, the whole business had all the qualities of the inevitable. The outcome, the joint work of many individuals rationally exploiting opportunities which no one had conspired to design, was the rapid articulation of disciplines and the rapid professionalization of academics.

The development of regulative and humane legislation required the skills of lawyers and economists, sociologists and political scientists. ... Controversy over such issues created a new market for the books and magazine articles of experts and engendered a new respect for their specialized knowledge. Reform brought with it the brain trust (Hofstadter, 1955, p. 155).

The "Disciplines" of the Social Sciences

America got its first psychological laboratory sometime in the 1870s, and by 1904 there were 48. By 1917, Who's Who in Science could report that America had more psychologists than England, France and Germany combined. At the 1904 St Louis Congress (itself an expression of the new legitimacy being acquired by social scientists) Columbia psychologist J. M. Cattell was clear about the goals for the new knowledge. He wrote:

I see no reason ... why the application of systematized knowledge to the control of human nature may not in the course of the present century accomplish great results commensurate with the nineteenth century applications of physical science to the natural world (quoted by Hale, 1980, p. 106).

The first graduate program in the social sciences was Columbia's School of Political Science, founded in 1880. In the first year of its existence (1903) the American Political Science Association had 214 members; six years later there were 1350 political scientists in America. Some 83 of the 135 articles in the Political Science Quarterly (founded in 1886) were articles of contemporary relevance to problem-solvers (Somit & Tanenhaus, 1967). The world's first sociology department was created at Rockefeller's new University of Chicago in 1892. The American Sociology Society began with 116 members in 1906; it had some 700 by World War I. At the same time, the older disciplines professionalized and re-articulated their professional responsibilities. Thus, the "Statement of Principles" of the progenitor of the American Economic Association (founded in 1884), asserted that "the conflict of labor and capital has brought into prominence a vast number of problems whose solution requires the united efforts, each in its own sphere, of the church, the state, and of science" (quoted from Mitchell, 1969). More generally, as Hofstadter writes:

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There were problems and there were opportunities. But there were, as well, beliefs about science, beliefs being promoted by the voices of industrializing science, especially by philosopher/physicists in Germany and France. This is important and needs some development; but before turning to this, we should notice that it was during this period that accounting took its first steps toward becoming one among the human sciences.

ACCOUNTING BECOMES A HUMAN SCIENCE

The precondition, taken for many of the same reasons that produced the universities and new departments in the social sciences, was the arrival from Britain of the chartered accounting houses which became, in time, today's largest firms (Miller & O'Leary, 1987; Hopwood, 1987). Price Waterhouse, a London firm, sent its first representative to the U.S. in 1890; Peat, Marwick, Mitchell and Co. derives from James Marwick, a Scot who opened an office in Glasgow in 1887 and one in the U.S. in 1896. The American Association of Public Accountants was founded in 1887; New York passed the first CPA law in 1896, requiring licensing after passing an examination. Accountants, of course, had recognized how much their authority would depend on credentials. And like the new breed of social scientists, they too stood firm on scientific objectivity. Thus, a 1908 editorial in the Journal of Accountancy asserted: "The science of accountancy makes inquiry into the laws determining the money results of business operations" (cited by Merino, 1991). In time, it came to be seen that this kind of legitimacy also required a university base. Once in the university, it was easy to deepen and extend the range of expertise of accounting students. In this way, faculties could respond to new demands imposed by changes in society.

If we turn, then, to the very recent past, we can note that in most states there are minimum formal educational requirements which must be satisfied before a person can even sit for the CPA examination. Indeed, not only is there now considerable interest in extending the undergraduate program to five years (vigorously promoted by the AICPA since 1983), but the rapid development of programs in "management science", and of the Ph.D. in accounting, designed surely to legitimate the new practices, strongly suggests that since the 1960s, the practices of accounting have undergone fundamental changes — roughly, from an initial conception as a narrow technical subject to a broad-based, multi-disciplinary subject critical in all economic decision-making. So conceived, accounting education needs to include not merely traditional subjects, but strong doses of economics and behavioral science, and developments in computer and information systems, including expert systems, AI and telecommunications.

I have no doubt that these changes could easily be explained as straightforward responses to changes in global capitalism and to the development of what has been termed an information economy and society (Mintz & Schwartz, 1986; Montagna, 1986; Miller & Rose, 1990). Nor do I doubt that the accountant, once a book-keeper and then a credentialed keeper of the audit, has now become on easily familiar terms a human scientist, perhaps indeed the human scientist most important in the information economy. After all, as "guardian of records and determiner of value" the accountant becomes in this new world the critical player (Foucault, 1977; Baudrillard, 1975). The accountant, always a technician, but now absorbing and assimilating the resources of technocratic sciences, then may rightly claim to be the queen of the (technocratic) human sciences!

THE CURRENT DEBATE IN ACCOUNTING

Viewed in this way we can, perhaps, have a clearer picture of the critical arguments surfacing in accounting, arguments so beautifully being propelled by the contributors to this special issue. In the remainder of my paper, I sketch what seem to me to be the critical strands in this argument, drawing on work by critics from
within. The argument has many dimensions — epistemological, ontological and sociological — and my hope is to show how these are best seen as strands in a fabric, indissolubly linked. I would offer that differences, both between the mainstream and its critics and between critics, are best seen as turning on two sets of assumptions, firstly, assumptions about the philosophy (or theory) of science and secondly, assumptions regarding the nature of society. The differences between the mainstream and its critics define a chasm. The differences between critics, from my point of view, are marginal, but they could be exacerbated. That is, there is wide agreement among critics that accounting, just as institutionalized social science, is a profoundly intrusive technocratic and disciplinary practice. Further, there is considerable agreement that these practices depend upon assumptions regarding “science”, and more generally “knowledge”. The disagreements, while genuine, do not, as I hope to make clear, stand in the way of cooperative and mutually sustaining efforts, both to get a deeper understanding and, then, to transform current practices. I can begin with a brief sketch and history of the mainstream view.

NATURAL SCIENCE, TECHNOLOGY AND SOCIAL SCIENCE

Everyone sees that the mainstream view of social science assumes an empiricist philosophy of science. But it is critical not only to grasp exactly what this means, but to see that this philosophy became hegemonic only in the late nineteenth century. By this time physics, of course, had been well established. But its relation to technological innovation was unclear. Chemistry, but recently come to maturity, changed this. Since mid-century, it had become industrialized and the source of a host of remarkable new technologies, from aniline dyes, to fertilizers, plastics and poison gas. Moreover, while in 1800 electricity was a scientific curiosity, a series of discoveries had made possible the incandescent lamp (marketed in 1879), the first electric power station (1881) and the first electric railway (Landes, 1969). To this point, there had been no generally accepted doctrines about what made a science a science. This was settled by the late nineteenth century physicist/philosophers mentioned earlier: G. R. Kirchhoff, Wilhelm Ostwald, Ernst Mach, Ludwig Boltzmann, Henri Poincaré, Pierre Duhem, and Karl Pearson, Mach’s British trumpeter. Working with materials in older empiricist philosophical traditions, their views had enormous influence. After all, everyone could now see the power of the natural sciences. Surely, if one could clarify what made this possible, social science might do as well? Indeed, we can say that at the time of their articulation as disciplines, the human sciences were substantially constituted in terms of views of natural science which had just then become common currency.

Suppose, however, that these doctrines were false, that they did not give an adequate representation of existing practices in the natural sciences? There were some who thought so, among them Albert Einstein. In his 1933 Oxford Herbert Spencer lecture he gave this advice to students of the sciences: “If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick to one principle: Don't listen to their words, fix your attention on their deeds” (quoted by Holton, 1968, p. 178). But if this is just what the social scientists did not do, then might it be that, at their very inception, the social sciences were ideologically constituted?

As we now know, the doctrines of these founding philosophers of natural science have survived all challenges — until very recently. Impelled by developments in mathematical logic, they became the foundation for twentieth century logical empiricism, from Carnap to Hempel.² Popper's assault did undermine “con-

² It is a mistake to suppose that the logical positivism which came out of Vienna in the 1920s defined contemporary social science. As indicated, the critical assumptions were already very widely promoted beginning in the 1880s. Vienna
firmationism", but contrary to the still widely held view, because he was insufficiently radical, Popper did not offer a viable alternative to the reigning view of science. Indeed, as Hines (1988) writes, "Popper's falsificationism bears no relation to the way that things 'really happen in science'" (p. 659). While I cannot argue this here, if I am correct, almost everything of the received doctrines of mainstream philosophy of science has little to do with the actual practice of physical science. Although there were earlier efforts to show this, surely Kuhn's *Structure of Scientific Revolution* (1962) powerfully propelled this line of criticism, a criticism now very well advanced by a host of studies in the history and sociology of science. Still, it is easy to misconstrue the consequences of this critique, in particular to suppose that scientific practices in all the sciences lack utterly those virtues in terms of which they have secured authority in society. As I suggest, it is quite one thing to reject the empiricist theory of science; quite another to reject science altogether. We can begin by identifying what seem to me to be the most critical assumptions of empiricist philosophy of science, assumptions which, as I have noted, became codified only in the late nineteenth century.

THE CENTRAL ASSUMPTIONS OF THE EMPIRICIST PHILOSOPHY OF SCIENCE

Writing in 1906, Duhem clearly set out the first assumption. He wrote: "A physical theory ... is an abstract system whose aim is to summarize and classify logically a group of experimental laws without claiming to explain these" (Duhem, 1954, p. 7). Why not "explain"? Duhem continued: "To explain ... is to strip reality of the appearances covering it like a veil, in order to see the bare reality itself." But this is metaphysics, not science. Following Hume, and Kant, we can know only the empirical world, the world given in our senses. Thus, "a physical theory is not an explanation. It is a system of mathematical propositions deduced from a small number of mathematical principles, which aim to represent as simply, as completely, and as exactly a set of experimental laws" (p. 19). Mach concurred: "knowing the value of the acceleration of gravity, and Galileo's laws of descent, we possess simple and compendious directions for reproducing in thought all possible motions of a falling body." This "compendious representation of the actual, necessarily involves as a consequence the elimination of all superfluous assumptions which cannot be controlled by experience, and above all, of all assumptions that are metaphysical in Kant's sense" (Mach, 1959). On this view, then, "explanation" was "compendious description", the deduction from "mathematical principles". Indeed, prediction had the same logic; "good predictions" are the test of a "hypothesis" (an experimental law or regularity which relates observables); testing a theory means testing the deductively elaborated consequence of the principles of theory — "predictions" in this sense.

We can illustrate this from current accounting literature. In their presentation of "the capital asset pricing model", Watts & Zimmerman (1986, p. 2) quite correctly note that the positivism was a powerful refinement of earlier empiricisms, made possible by the advances in modern logic. It surely had strong effects on the development of social science after 1930. Indeed, one can still easily find social scientists who sound like Carnap (1928)! Impelled by criticism from within and without, Vienna positivism continued to transform itself, becoming a logical empiricism and then a neo-positivism, best represented by Nagel (1961) and Hempel (1966). This is, I think, the dominant view among social scientists today, even if there are few philosophers of science who would identify themselves with this view. For an extensive review from within, see Suppe (1977). Beautiful short reviews and criticism may be found in Brown (1977) and Chalmers (1982). See also Manicas (1987, Chapter 12).

3 The following are among the most useful: Collins (1975), Harvey (1981), Knorr-Cetina (1981), Latour & Woolgar (1986), Lynch (1985), Pickering (1981, 1984), Pinch (1977, 1985, 1986), Shapin & Schaffer (1985), Travis (1981). The distinction between mainstream — empiricist, positivist, neo-positivist — philosophy of science and alternatives is rough, but still critical. There is a revolution in the philosophy of science in progress. But it is hardly clear how long it will take before practice in social science, especially in America, will be substantially affected.
equation which gives the market value of a firm (as discounted present value of future cash flows) is an appropriate analogue to empirical studies in economics and, better, an example of “the view of theory in science”, citing here Poincaré (1905), Popper (1959) and Hempel (1965).

Notice also that Watts & Zimmerman (like Poincaré, Popper and Hempel, 1965) are not naive positivists in that they see that “in positive or scientific theory there are no brute facts,” that “the interpretation of facts depends on theories” (Watts & Zimmerman, 1986: note 3). Moreover, unlike Poincaré and positivists from Carnap to Hempel, Watts & Zimmerman do not have “a distrust of nonobservable concepts (sic) such as absolute space and time in mechanics”. Thus, “in attempting to explain existing economic institutions, empiricists typically assume that efficient institutions survive” (Watts & Zimmerman, 1986). But, as above, “explain” means “deduce from premises”.

A second and not unrelated assumption of the mainstream theory of science regards causality. It is often said that science searches for causes. But it is almost never noticed that the view of causality which is then assumed derives from Hume, a regularity notion, such that $C$ is the cause of $E$ means “whenever $C, E$”. Indeed, this is perhaps the most characteristic assumption of empiricism, exactly because it is fully consistent with empiricism’s anti-metaphysical animus, because it is a conception of causality representable, without residue, by extensionalist logic and mathematics — the language of dependent and independent variables — and because, therefore, it is the form for laws in the covering law model of explanation in which, as a matter of logic, explanation and prediction are symmetrical.

The assumption is clear in the texts quoted from Mach and Poincaré (above). It is absolutely pervasive in all the social sciences where developments in statistical analysis (beginning with the racist eugenicist, Galton, and his student, Pearson) propelled multivariate analysis, multiple regression, etc.4 These, presumably, allowed researchers unable to experiment to substitute quantitative methods, to identify and weigh factors in complex real world phenomena and, thus, to generate genuine explanatory and predictive laws. A perfect example of this style of research is provided by Kinney (1986) who shows us how in empirical accounting research design we can “disentangle” the “effects of Vs and Zs from the effect of X” (p. 341). That this view systematically confuses the analysis of variance with the analysis of causality goes unnoticed (Manicas, 1989) — so powerful is the ideology of mainstream social science!

But — and this point needs emphasizing — if only because they were already well institutionalized, this philosophy of science had little effect on the practices of the natural sciences, its effects on the social sciences were enormous. Once metaphysics was eschewed, causality mathematized and explanation and prediction were seen as symmetrical, knowledge could become power in a fully technocratic sense: predict in order to control. Given, then, the interests and opportunities presented to the American founders of modern disciplinary social science, the present character of the modern social sciences is hardly surprising.

What then of the second set of assumptions which characterize mainstream social science?

MAINSTREAM ASSUMPTIONS ABOUT SOCIETY

Mainstream social science offers two images of society. The first, methodological individualism,
is anti-metaphysical and owes also to seventeenth
century assumptions about the relations of
individuals. All the current critics of accounting
theory and practice reject methodological
individualism. On this view, society is an
aggregation of fully socialized persons, each
capable of rational goal-setting. For this view,
there is no macro/micro problem since macro-
variables are but the “aggregation effects of
micro-variables”. These writers may use the
language of social structure, but for them, social
structures are not Durkheimian metaphysical
entities. Rather, "structure is repetition; it is the
pattern of the same kind of events happening
over and over again, involving different people
spread across different places" (Collins, 1988,
p. 243). Ultimately, there is no social, and
psychological facts — motivations, interests,
etc. — carry all the explanatory weight. Of
course, people with interests and motives make
choices; but this style of social science fails to
acknowledge not merely that these interests
and motives are socially grounded (and ex-
plainable), but worse, that the choices are
themselves profoundly constrained by existing
social structures (Mills, 1969). The work of
George C. Homans (1987) is perhaps exemplary
in this regard; but one should emphasize the
mathematical versions of this, for example, in
the work of Herbert Simon and, less self-
consciously, in the elegant constructions of
modern micro-economics.

The second image, variants of structural-
functionalism, owes to Parsons' brilliant synthesis.
While usually not noticed, it is the complement
of the first. On this view, society is a system of
social structures which, in order to maintain
itself, must satisfy certain critical functions. Talk
of motivations, interests and the like is perva-
sive in this style of research, but because these
are determined by internalized social norms,
they carry no explanatory weight. Indeed,
agents drop utterly out of the picture. Thus,
Blau defines social structure as:

two kinds of relations among elements in an organized
collectivity: (1) the causal interrelations among vari-
ables that refer to the attributes of the collectivity itself,
not the attributes of the individual members ... (2) the
social interrelations among differentiated positions,
such as the division of labor or hierarchy of authority

If one concentrates on (1), the causal interrela-
tions, then one will likely do quantitative work
with macro-variables, market values, ethnicity,
unemployment, which can then enter into
causal relations in the Humean sense noted
earlier. Thus, as before, one can (optimistically!)
investigate the relation between changes in
accounting procedures and stock market values
(Watts & Zimmerman, 1986). We should
emphasize here that quantitative research can
be extremely valuable descriptively. The sort of
statistical and correlational information pro-
vided, for example, by the Bureau of Labor is
essential. The error is in supposing, as above,
that one has done causal analysis, for example,
that by determining a strong positive correla-
tion between educational attainment and in-
come one has explained something. One needs
theory here, but unfortunately, it is not the
kind of theory which is typically propounded
by structural functionalists.

Alternatively, one can concentrate on (2)
and argue, for example, that “accounting serves
a confidence-building and conflict avoidance’
role in organizations” (Richardson, 1987, sum-
marizing Gambling, 1977). Richardson’s over-
view of structural functionalism is very good,
even if, for me at least, he is far too generous.²

² It is an error of some import to suppose (as does Richardson) that Berger and Luckman’s influential Social Construction
of Reality is any sort of departure from Parsonsian sociology, or worse, that it be considered along with the work of
Garfinkel. Berger and Luckman’s “social constructionism” is but a restatement of Parsons, using the language of Schutz and
dialectics. As Bhaskar (1979) has pointed out, one gets “on the one hand, a voluntaristic idealism with respect to our
understanding of social structure and, on the other, a mechanistic determinism with respect to our understanding of
people” (p. 42). Garfinkel’s work, which called for a radical shifting of ground, was aimed directly at the Parsonsian
mechanistic theory of action. Briefly, social structure was bracketed “in favor studying how the participants create,
assemble, produce and reproduce social structures to which they orient” (Heritage, 1987, p. 231). Although, as Heritage
shows, Garfinkel was persistently misunderstood, this was a major step away from both Parsonsian sociology and
methodological individualism. It is a critical part of Giddens’ synthesis (Giddens, 1984) and of my sketch below.
He does, however, note its fatal flaw: as he writes, “the values which accountants implement are taken for granted. The accountant and accounting system play a technical role in ensuring that values are enacted” (Richardson, 1987, p. 345; see also Arrington & Francis, 1989; and Schweiker, 1991). Put in other terms, in this perspective, accountants lose their agency becoming but bearers of roles within reified structures, a flaw shared by many Marxist approaches as well (Anderson, 1980).

ALTERNATIVES TO MAINSTREAM PHILOSOPHY OF SCIENCE

Critics of the mainstream all share, as I think they should, in rejecting the empiricist philosophy of science which undergirds all work in mainstream social science. But there are problems in what, if anything, is to replace this. There are writers on accounting who might be read as presupposing either that there can be no such thing as a human science, or that, if such is possible, human science is so radically different from natural science that it is misleading to think of it as science (Boland, 1985; Morgan, 1988; Arrington & Francis, 1989). These are writers who have drawn, usefully and rightly, in my view, on writers known to take an anti-naturalistic stance as regards human science and, perhaps more important, an anti-realist stance more generally. These writers include Schutz, Winch, Derrida, Gadamer, Ricoeur, and less clearly perhaps, Foucault. The issue is raised in a footnote in a recent paper by Arrington & Francis (1989). They write that they have “deliberately avoided using Derrida’s oft-quoted/oft-misunderstood dictum ‘there is nothing outside the text’” and offer that “this aphorism does not mean that there is no ‘reality’ other than what is in the text; rather it says that the meaning inscribed within a text is not transcendentally signified outside of the text — there is no metaphysics of presence, no fixed center from which meaning can originate external to the text itself” (p. 53).

This is, I think, correct; but the issue needs some careful gloss. Consider Chua’s formulation. With reference to mainstream accounting thought, she writes: “What is ‘out there’ (object) is presumed to be independent of the knower (subject), and knowledge is achieved when a subject correctly mirrors and ‘discovers’ this objective reality” (Chua, 1986, p. 606). There are, in my view, several things wrong about this, even if I would insist that we need to preserve something of it.

Firstly, knowledge is achieved when subjects “discover” this objective reality. We need to firmly reject the epistemological individualism of standard accounts of science and be clear that knowledge is a social product (Manicas & Rosenberg, 1985, 1988). Second, this “discovery” (appropriately in scare quotes) is an act of social “representation”, not of “mirroring”. Since there could be no way for us to get a God’s-eye, non-representational view of the world, the idea that propositions are true only if they correspond to reality must also be rejected.

On the other hand, our claims to knowledge are about a reality which is “out there”. Consider first natural science: put crudely, we believe that ordinary salt is NaCl because there is a consensus within the scientific community that the something out there is best theorized as a compound of sodium and chlorine with theorized causal properties which explain that, e.g. it will, ceteris paribus, dissolve in water. The something out there cannot determine our theory — its “meaning is not transcendentally signified outside the (theory)’, but the something out there does pose constraints on what our theory can say that something is. This is not an empiricist philosophy of science, but a realist one, one which, I think, better reflects the actual practices of the natural sciences (Bhaskar, 1978; Harré, 1970, 1987).

Note also that it rejects the Humean account of causality. On this view, causes are the powers of “things”. But because things always exist in contingent complexity, the outcome of their being energized will depend upon the contingent collocation which actually exists. The salt of everyday experience does not always
dissolve in water because neither it nor ordinary water is only NaCl or H2O — theoretically defined salt and water. They contain other things. Moreover, the theoretically defined conditions in which pure NaCl must dissolve in pure H2O are not always sufficiently approximated. That is why there is both stability and precariousness in the world. Thus there are patterns, the indispensable generalizations which we all live by, and there is radical contingency, the unexpected and unpredictable.

PHILOSOPHIES OF SOCIAL SCIENCE

What then of social science? It might be supposed that even if this account holds regarding natural science and the natural world, it fails as regards the social, that, to use the same formulation as before, there is no “out there” posing constraints on our representations of the social world. Now part of this is true. Unlike the natural world, the social world does not exist independently of us — the truth of idealism. It is the case that the social world exists only by virtue of human belief and action. But from this it does not follow that there are no social constraints, nor, accordingly, that a proper social science can afford to reject the effort to identify and represent these. Durkheim was correct to see that there were “objective social facts”, that, to use a preferable theoretical formulation, existing social structures both enable and constrain action. He was mistaken in reifying social structure, in making it, in effect, independent of action. As incarnate in the activities of people, social structure is medium and product of action; it is both reproduced and transformed by our activities; and critically, for each of us, it pre-exists in the activities of those who came before us. It is thus that history is crucial and theoretically informed historical narrative indispensable.

The denial of this, the posture of voluntarism in social science, is often hinted at by postmodernist critics of the possibility of a human science. Consistently held, it entails not merely that the social world does not exist independently of us, but that its existence is ephemeral and subjective. On this view, since there is no true or false of the social world, we can represent it as we choose!

THE POSSIBILITY OF A HUMAN SCIENCE

But one can reject empiricism and the naive realism and objectivism which it presupposes and still preserve the possibility of a human science, one which, indeed, entails enormous differences in implications for inquiry. On this view, subjectivism, methodological individualism and Parsonian sociology are each wrong. Chua (1986, p. 620), in quoting Bhaskar, summarizes much of the foregoing:

"Society does not exist independently of human activity (the error of reification). But it is not [solely] the product of it (the error of voluntarism)." Rather, society provides the necessary, the material conditions for the creative subject to act. . . . At the same time, intentional action is a necessary condition for social structures. Society is only present in human action, and human action always expresses and uses some or other social form. Neither can, however, be identified or reduced to the other. Social reality is, thus, both subjectively created and objectively real (Chua, 1986).

IMPLICATIONS FOR INQUIRY

The implications for accounting research and practice and, more generally, for human science are plain enough. As Montagna writes as regards accounting theory, where choices are limited by a host of concrete factors and outcomes are contingent products of these, “uncertainty may be an inherent characteristic of a market environment.” Thus, “rather than emphasize a unitary logic of transactional efficiency, accounting must use causal analysis in a historical setting” (Montagna, 1986, p. 129). This means, as well, that we need to reject the search for laws of the sort characteristically sought in mainstream social science; and, attending this, we need to reject completely the covering law model of explanation. On the present view, explanation requires that we show concretely
how something came to be, including sometimes how actors came to believe what they believe! But, as everyone knows, predictions do often go wrong. The reason is clear enough: because collocations of causes are contingently changing — including here the fact that persons are causal agents! — the future is inherently contingent. After the fact, aided by theory we can offer a reconstruction of what was involved in bringing about just that outcome, including, then, even complicated events such as a stock market crash. Mainstream thinking about science, amazingly, takes the solar system to be the typical case, when manifestly it is nearly unique in being a nearly closed and unchanging system.

Montagna's example presumes that there is a social reality which structures choices by actors and that it can be known. But knowledge of social reality takes the same form as knowledge of nature, except that as Giddens has argued, there is then a double hermeneutic at work. The natural world exists independently of us, but as known, it is a social construction; the social world (which thus includes our experience and knowledge of the natural world) is socially constructed; and thus, so too is our knowledge of it — the "double hermeneutic". That is, since there is no privileged access and we are social beings who, with the materials at hand, negotiate all our understandings, the epistemology of natural science cannot satisfy empiricist demands. It must be hermeneutic. But the objects of inquiry in social science (unlike the objects of inquiry in natural science) are themselves social constructions — the hermeneutical product of lay actors whose conjoint acts constitute the social world. It is this last, of course, which is shared by Schutz, Gadamer, Derrida and Foucault. And it is for just this reason that deconstruction and ideological critique is both possible and liberating. Arrington & Francis quote Ryan: "To affirm the abyss deconstruction opens in the domain of knowledge is politically to affirm the permanent possibility of social change" (p. 16).

HERMENEUTICS AND CRITIQUE

Social reality is both subjectively created and objectively real; but it does not follow from this that social forms constituted by the acts of persons are fully transparent to them. There are both unacknowledged conditions for action and unintended consequences of action, and at the level of subjectivity, both the persistent tendency to reify the historically contingent social forms and the real possibility of forms of self-delusion, repression, and the like. As Giddens (1984) writes, persons do have "practical knowledge", otherwise they could not "carry on". But if the social world were fully transparent, human science would be utterly unnecessary. I thus want to argue that the deconstructive moment is not sufficient and that to take the further step one needs to affirm the possibility of an emancipatory human science. In this sense, then, now following Chua's dichotomy of alternatives to the mainstream paradigms, the critical must be favored over the interpretative. Yet this is not because the interpretative is wrong, but because it is incomplete.

This is not the place to try to provide a defense of this idea, to speak explicitly to the many problems which it raises. The outlines, however, may be sufficiently clear. The epistemology of such a science is not empiricist, but hermeneutic. It acknowledges that knowledge is a social product and that it lacks any sort of secure foundations. This science is also realist in the sense that it presupposes both that there is a knowable external world and that, while the social world is a construction, it is profoundly constrained by a specific history which provides actors with the materials for continued reproduction and transformation. It is, then, precisely, in coming to an understanding of society that we are engaged in critique.

The point is important: were actors to acknowledge "the abyss of deconstruction", and thus the permanent possibility of change, were they to grasp the conditions of their
action, e.g. how the wage-form, in confusing the value of labor with the value of labor power, conceals the logic of the process of capitalist accumulation; were they to have an understanding of the unintended consequences of their actions, for example, the feedback loops which so often self-implicate victims in perpetuating their condition, they would have satisfied the first condition of self-liberation. From this point of view, then, critique is always immanent. It does not involve some transhistorical or transcendental posture. It requires neither teleological history — as in too much Marxism — nor God; it demands only that there be a gap between belief and purposive action, a gap narrowed by having a better understanding of social reality. To be sure, even where social theory does provide assistance in this understanding, it is by itself powerless, exactly because as the reproduction of the social world is a conjoint product, so too is its transformation.

This can be put in other terms: suppose that some belief is false and essential to some practice. Acknowledging this, actors have a reason to transform the practice (Giddens, 1985, p. 340; Bhaskar, 1978, pp. 69–83). Of course, showing that social beliefs are false will not be easy — as critics of mainstream assumptions about accounting know. But changing a practice needs more: one needs to have those others, whose conjoint actions sustain the practice, share in seeing that it is false and be willing to act conjointly to transform the practice. Indeed, since “techniques of accounting and ways of controlling and organizing economic life go hand in hand […] it follows … that for ‘self-liberation’ one not only has to understand these processes but literally to construct alternative ways of organizing production processes” and the like. As Marx so very well knew, this will be neither automatic nor easy!

Nevertheless, a profound pedagogical task is suggested (Lehman, 1988). If accounting is now the queen of technocratic human science, then pedagogues of accounting occupy a privileged place! Viewed in this light, then, we can say that all critique: deconstructionist, rhetorical, Foucauldian, Marxist, just to name a few, is on an equal hermeneutic footing. Each seeks to eliminate false consciousness (or in Derridian terms, “the false grounding of textuality in a metaphysics of presence”); each seeks to undermine the myths which both constitute and legitimate practices; each seeks that understanding which is the first and essential step toward emancipation.

Moreover, there is plenty of non-competing work to be done. Not only can critique take a number of forms, but, as well, we need to acknowledge the inter-connectedness of accounting practices to capitalist practices, domestic and global, and to state and educational practices. As accounting practice is affected by these, it too has effects. Finally, and perhaps most important, we need to acknowledge our agency as reproducers and possible transformers of these practices. Indeed, because of this there is an inescapable moral dimension to all human activity, a dimension not restricted to making choices regarding alternatives presented by the prevailing state of things, but including, perhaps more critically, choices regarding the prevailing state of things themselves.

BIBLIOGRAPHY


