

CONSCIENTIOUS OBJ ECTIONS
Stirring Up Trouble About Language, Technology, and Education

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Social Science as Moral Theology

I begin with this essay because it provides the reader with the frame within which I compose my conscientious objections. I see myself as a storyteller (non-fiction division), make no claims to being a scientist, am not insulted when my essays are called polemical. Indeed, as I hope to make clear, there is a measure of cultural self-delusion in the prevalent belief that psychologists, sociologists, anthropologists, and other moral theologians are doing something different from storytelling. The New York Times could help if it stopped reporting their work on its Science Page. It could help even more if it added a Moral Theology Page to which "social scientists" of every variety (including economists) could regularly contribute.

Without getting misty-eyed about it, I think we can fairly say that universities have a sacred responsibility to define for their society what is worthwhile knowledge. These definitions are most clearly visible in university catalogues, where you will find lists of courses, subjects, and "fields" of study. Taken together, they amount to a certified statement of what the university thinks a serious student ought to think about. In what is omitted

from a catalogue, you may also learn what a serious student need not think about.

This observation is in no way a criticism of universities. You cannot have a university which does not, in some way, organize learning and by so doing attribute relative value to categories of knowledge. The trouble is that sometimes a university can suffer from hardening of the categories. This happens when certified scholars resolve, against all reason, to defend their customary view of knowledge from encroachment by more novel perspectives. Why scholars should fall into this state of mind is, all by itself, a fascinating issue and probably should be included in university catalogues as a course, or a subject, or even a field of study. Abraham Maslow devoted a portion of his career to studying the matter, and in his book *The Psychology of Science* he concluded that science (and by extension, scholarship generally) can be "a safety philosophy, a security system, a complicated way of avoiding anxiety and upsetting problems.... It can become—in the hands of some people, at least—a social institution with primarily defensive, conserving functions, ordering and stabilizing rather than discovering and renewing." Maslow went on to describe in some detail the psychopathology of scholarship which when given its head almost always leads to

an environment of extreme intellectual sterility. Of course it must be said, and Maslow said it, that when it is not psychopathic, the conserving impulse in scholarship is quite important, since it serves to protect an intellectual community from expending energy on trivial and even depraved categories of knowledge such as elocution and astrology.

But these are bad times for scrupulous efforts at gatekeeping, and, happily, many universities are now busily engaged in rewriting their catalogues. A pervasive and lively energy, especially in our great universities, is being directed toward the expansion of categories, models, and theories, toward the development of new subjects. Among the most prominent of these is the subject known variously as "Communication," or "Media Studies," or (as we call it at my university) "Media Ecology." This takes as its domain the study of the cultural consequences of media change: how media affect our forms of social organization, our cognitive habits, and our political ideas. As a young subject, media ecology must address such fundamental questions as how to define "media," where to look for cultural change, and how to link changes in our media environment with changes in our ways of behaving and feeling. But such questions rest on another, larger question which is as yet unanswered—namely, what kind of subject is this to be? Is it a science? Is it a branch of philosophy? Is it a form of social criticism? Where, in short, do we place it in the catalogue?

The usual, indeed the only, answer is that the subject must be a social science. Therefore, in this essay I will address two fundamental questions: What are legitimate forms of research in the social sciences? And, what are the purposes of conducting such research?

I must say at the start that I reject the implications of the phrase "social science." I do not believe psychologists, sociologists, anthropologists, or media ecologists do science, and Michael Oakeshott's distinction between processes and practices is definitive in explaining why. Oakeshott means by processes those events that occur in nature, such as the orbiting of planets or the melting of ice or the production of chlorophyll in a leaf. Such processes have nothing to do with human intelligence, are governed by immutable laws, and are, so to say, determined by the structure of nature. If one were so inclined, one might even say that processes are the creation of God. By practices, on the other hand, Oakeshott means the creations of people—those events that result from human decisions and actions, such as this essay or the formation of a new government or our conversations at dinner or falling in love. These events are a function of human intelligence interacting with environment, and although, to be sure, there is a measure of regularity in human affairs, such affairs are not determined by immutable laws. Now, I have been told by friendly colleagues that this last statement, namely, that human actions are not determined by immutable

and universal laws, cannot be proved, and that to assert it is in the nature of a metaphysical speculation. Fair enough. You may consider it, then, to be part of my metaphysics that I believe in free will and in choice; that human beings are fundamentally different from orbiting planets and melting ice; and that while we are profoundly influenced by our environment, our ideas and behavior are not irrevocably determined by natural laws, immutable or otherwise. In other words, I believe with Oakeshott that there is an irrevocable difference between a blink and a wink. A blink can be classified as a process, meaning it has physiological causes which can be understood and explained within the context of established postulates and theories; but a wink must be classified as a practice, filled with personal and to some extent unknowable meanings and, in any case, quite impossible to explain or predict in terms of causal relations.

As I understand it, science is the quest to find the immutable and universal laws that govern processes, and does so on the assumption that there are cause-and-effect relations among these processes. In this definition, I place myself, even if only beside their feet, with Newton and the last of the great Newtonians, Albert Einstein. It follows that I believe the quest to understand human behavior and feeling can in no sense except the most trivial be called science. The trivial-minded point, of course, is the fact that students of natural law and human behavior both often quantify their observations, and on this common ground may be classified together. A fair analogy would be to argue that since a house painter and an artist both use paint, they are engaged in the same enterprise, and to the same end.

The scientist uses mathematics to assist in uncovering and describing the structure of nature. At best, the sociologist (to take one example) uses quantification merely to give some precision to his ideas. But there is nothing especially scientific in that. All sorts of people count things in order to achieve precision without claiming that they are scientists. Detectives and bail bondsmen count the number of murders committed in their city; judges count the number of divorce actions in their jurisdictions; business executives count the amount of money spent in their stores; and young children like to count their toes and fingers in order not to be vague about how many they have. Information of this kind may sometimes be valuable in helping a person get an idea, or, even more so, in providing support for an idea. Numbers may even be useful in browbeating people into accepting an idea that otherwise has no merit. I have, myself, harbored several such worthless ideas, one of which has recently been supplied with some impressive numbers that not only will permit me to continue to believe this nonsense, but may help me to persuade others to believe it. I refer to my theory that living in California, Florida, and other warm climates tends to shrivel the brain and makes people dumber than those living in colder climates, such as New York,

Pennsylvania, Illinois, and Iowa. Since there is no idea so bad that a social scientist will not find support for it, I was not surprised to come across a study by two doctoral students at Texas Technical University who found that the ten states with the highest average SAT scores all had cold winters. Indeed, every state with an average of 510 or higher on both the verbal and quantitative parts of the SAT had an average high temperature in January of less than 42 degrees Fahrenheit. At the other end, five of the ten states with the lowest SAT scores were warm-weather states. Moreover, temperature had a significant relationship to SAT scores even when the researchers took into account such factors as per-pupil expenditures on schooling. So there!

Just as counting things does not a scientist make, neither does observing things, though it is sometimes said that if one is empirical, one is scientific. To be empirical means to look at things before drawing conclusions. Everyone, therefore, is an empiricist, with the possible exception of paranoid schizophrenics. To be empirical also means to offer evidence that others can see as clearly as you. You may, for example, conclude that I like to write essays, offering as evidence that I have written this one and that there are several others contained in this book. You may also offer as evidence a tape recording, which I will gladly supply, on which I tell you that I like to write essays. Such evidence may be said to be empirical, and your conclusion empirically based. But you are not therefore acting as a scientist. You are acting as a rational person, to which condition many people who are not scientists may make a just claim.

Some time ago, I had a conversation with a young communications professor from a midwestern university who repeatedly claimed to be a member of the community of social scientists. The basis of her claim was that she had conducted what is called a correlational study of television viewing and aggressive behavior in children, the conclusion of which was that some children in the state capital who watch lots of violent programs are also apt to act more aggressively than some of the children who watch fewer violent programs. She could not say—and had no hope of saying—whether they were aggressive because they watched television violence, or watched television violence because they were aggressive. She could also not say—and had no aspiration to say—why it was that some children who watched many violent programs did not act aggressively, or why some of those who didn't watch violent programs did act aggressively. Moreover, she told me that within the past five years there have been more than 2,500 such studies conducted in American universities, with the result that there is no agreement on very much except that watching violent television programs may be a contributing factor in making some children act aggressively, but that in any case it is not entirely clear what constitutes aggressive behavior. In other words,

after 2,500 studies, we have a statement that is somewhat less meaningful than my saying that Ronald Reagan's telegenic charm may have been a contributing factor to his being elected President.

Confronted by such a desiccated view of science, I naturally asked what her definition of science was. She replied that it required one to be empirical, to measure things, to make one's methods and conclusions public, and to test one's assertions. Because this definition would not distinguish the act of science from the normal working of a sane mind engaged in problem-solving, it did not take me long to get her to acknowledge that such actions, while necessary in science, were hardly sufficient, and I was able to reduce her to saying, "Well, what difference does it make what you call it?" Now, this is not normally the way one ought to treat a young professor, but I did so because I believe it is important to distinguish science from non-science.

There are three reasons why. First, it is always worthwhile to insist that people explain the words they have chosen to describe what they are doing, so that their purposes may be evaluated. Second, many people who use the word "science" do so in the hope that its prestige will attach to their work. Americans are peculiarly afflicted with science-adoration, which is why we must endure such abominations as the oxymorons Christian Science, Creation Science, Scientology, Policy Science, Decision Science, and Administrative Science, as well as Behavioral and Social Science. And third, when the study of human conduct is classified as science, there is a tendency to limit the kinds of inquiries that may be made: counters and "empiricists"—that is, pseudo-scientists—are apt to deprive others of the right to proceed in alternative ways, for example, by denying them tenure. The result is, of course, that they impoverish all of us and make it difficult for people with ideas to be heard. I want to give one more example of social science to make clear why I would not call it science at all. A piece of work that is greatly admired as social science, at least from a technical if not an ethical point of view, is the set of experiments (so called) supervised by Stanley Milgram, the account of which was published under the title *Obedience to Authority*. In this notorious study, Milgram sought to entice people to give electric shocks to "innocent victims" who were in fact conspirators in the experiment and did not actually receive the shocks. Nonetheless, most of Milgram's subjects believed that the victims were receiving the shocks, and many of them, under pressure, gave shocks that, were they real, might have killed the victim. Milgram took great care in designing the environment in which all this took place, and his book is filled with statistics that indicate how many did or did not do what the experimenters told them to do. As I recall, somewhere in the neighborhood of 65 percent of his subjects were rather more compliant than

would have been good for the health of their victims. Milgram drew the following conclusion from his research: In the face of what they construe to be legitimate authority, most people will do what they are told. Or, to put it another way, the social context in which people find themselves will be a controlling factor in how they behave.

Now, in the first place, this conclusion is merely a commonplace of human experience, known by just about everyone from Maimonides to my Aunt Molly. The exceptions seem to be American psychiatrists. Before he conducted his experiment, Milgram sent to a large group of psychiatrists a questionnaire in which he solicited their opinions as to how many subjects would be likely to continue giving electric shocks when ordered to do so. The psychiatrists thought the number would be very much smaller than it actually was, basing their estimates on their knowledge of human behavior. Which explains to my complete satisfaction why their estimates were so wrong. I do not mean to imply that real scientists never produce commonplaces, but only that it is rare, and never a cause for excitement. On the other hand, commonplace conclusions are almost always a characteristic of academic pseudo-science.

In the second place, Milgram's study is not empirical in the strict sense, since it is not based on observations of people in natural life situations. I assume that no one is especially interested in how people behave in a laboratory at Yale or any other university; what matters is how people behave in situations where their behavior makes a difference to their lives. But any conclusions that can be drawn from Milgram's study must specify that they apply only to people in laboratories under the conditions Milgram arranged. And even if we assume a correspondence between laboratory behavior and more lifelike situations, no predictions can be made about what lifelike situations these might be. Neither can any serious claim be made that there is a causal relationship between the acceptance of legitimate authority and doing what you are told. In fact, Milgram himself shows us that there is not, since 35 percent of his subjects told the "authority figure" to bug off.

Moreover, Milgram had no idea why some people did and some people did not tell him to bug off. For myself, I feel quite sure that if each of Milgram's subjects had been required to read Hannah Arendt's *Eichmann in Jerusalem* before showing up at the laboratory, Milgram's numbers would have been quite different.

But let us suppose that I am wrong about that, and let us further suppose that Milgram had found that 100 percent of his subjects did what they were told, with or without Hannah Arendt. And now let us suppose that I tell you a story of a group of people who in some real situation refused to comply with the orders of a legitimate authority. Would you say to me that this cannot be so, because Milgram's study proves otherwise? Or would you say that this overturns Milgram's

work? I think you would say neither, because Milgram's experiment does not confirm or falsify any theory that might be said to postulate a law of human nature. His study, which incidentally I find both fascinating and terrifying, is not science. It is something else entirely.

Which leads me to say, at last, what sort of work I think Milgram was engaged in—and what sort of work all of us who study human behavior and situations are engaged in. I will start by making reference to a famous correspondence between Sigmund Freud and Albert Einstein. Freud once sent a copy of one of his books to Einstein, asking for his evaluation of it. Einstein replied that he thought the book exemplary but was not qualified to judge its scientific merit. To which Freud replied somewhat testily that if Einstein could say nothing of its scientific merit, he could not imagine how the book could be judged exemplary; it is science or it is nothing. Well, of course, Freud was wrong. His work is exemplary—indeed, monumental—but scarcely anyone believes today that Freud was doing science, any more than educated people believe that Marx was doing science, or Max Weber or Lewis Mumford or Bruno Bettelheim or Carl Jung or Margaret Mead or Arnold Toynbee. What these people were doing—and Stanley Milgram was doing—is weaving narratives about human behavior. Their work is a form of storytelling, not unlike conventional imaginative literature although different from it in several important ways.

I call the work these people do storytelling because this suggests that an author has given a unique interpretation to a set of human events, that he has supported his interpretation with examples in various forms, and that his interpretation cannot be proved or disproved but draws its appeal from the power of its language, the depth of its explanations, the relevance of its examples, and the credibility of its theme. And that all of this has an identifiable moral purpose. The words "true" and "false" do not apply here in the sense that they are used in mathematics or science. For there is nothing universally and irrevocably true or false about these interpretations. There are no critical tests to confirm or falsify them. There are no postulates in which they are embedded. They are bound by time, by situation, and above all by the cultural prejudices of the researcher. Quite like a piece of fiction.

A novelist—for example, D. H. Lawrence—tells a story about the sexual life of a woman, Lady Chatterley, and from it we may learn things about the secrets of some people, and wonder if Lady Chatterley's secrets are not more common than we had thought. Lawrence did not claim to be a scientist, but he looked carefully and deeply at the people he knew and concluded that there is more hypocrisy in heaven and earth than is dreamt of in some of our philosophies. Now, Alfred Kinsey was also interested in the sexual lives of women, and so he and his assistants interviewed thousands of them in an effort to find out what they believed their sexual conduct was like. Each

woman told her story, although it was a story carefully structured by Kinsey's questions. Some of them told everything they were permitted to tell, some only a little, and some probably lied. But when all their tales were put together, a collective story emerged about a certain time and place. It was a story more abstract than D. H. Lawrence's, largely told in the language of statistics and, of course, without much psychological insight. But it was a story nonetheless. One might call it a tribal tale of one thousand and one nights, told by a thousand and one women, and its theme was not much different from Lawrence's—namely, that the sexual life of some women is a lot stranger and more active than some other stories, particularly Freud's, had led us to believe.

I do not say that there is no difference between Lawrence and Kinsey. Lawrence unfolds his story in a language structure called a narrative. Kinsey's language structure is called exposition. These forms are certainly different, although not so much as you might suppose. It has been remarked about the brothers Henry and William James that Henry was the novelist who wrote like a psychologist, and William the psychologist who wrote like a novelist. Certainly, in my meaning of the word "story," exposition is as capable of unfolding one as is narrative. Of course, Lawrence's story is controlled entirely by the limits of his own imagination, and he is not obliged to consult any social facts other than those he believed he knew. Lawrence's story is pure personal perception. And that is why we call it fiction. Kinsey's story comes from the mouths of others, and he is limited by what they said when he asked his questions. Kinsey's story, therefore, we may call a documentary. But like all stories, it is infused with moral prejudice and sociological theory. It is Kinsey who makes up the questions, and chooses who will be interviewed, the circumstances of the interview, and how the answers will be interpreted. All of this gives shape and point to his story. Indeed, we may assume that Kinsey, like Lawrence, knew from the outset what the theme of his story would be. Otherwise, he probably wouldn't have cared to tell it.

Both the novelist and the social researcher construct their stories by the use of archetypes and metaphors. Cervantes, for example, gave us the enduring archetype of the incurable dreamer and idealist in Don Quixote. The social historian Marx gave us the archetype of the ruthless and fat, though nameless, capitalist. Flaubert gave us the repressed bourgeois romantic in Emma Bovary. And Margaret Mead gave us the carefree, guiltless Samoan adolescent. Kafka gave us the alienated urbanite driven to self-loathing. And Max Weber gave us hardworking men driven by a mythology he called the Protestant Ethic. Dostoevsky gave us the egomaniac redeemed by love and religious fervor. And B. F. Skinner gives us the automaton redeemed by a benign technology. I think it justifiable to say that in the nineteenth century, novelists provided us with most of the powerful metaphors and images of our culture. In the twentieth century, such metaphors and

images have largely come from the pens of social historians and researchers. Think of John Dewey, William James, Erik Erikson, Alfred Kinsey, Thorstein Veblen, Margaret Mead, Lewis Mumford, B. F. Skinner, Carl Rogers, Marshall McLuhan, Noam Chomsky, even Stanley Milgram, and you must acknowledge that our ideas of what we are like and what kind of country we live in come from their stories to a far greater extent than from the stories of our most renowned novelists. I do not mean, incidentally, that I think the metaphors of social research are created in the same way as those of novels and plays. The writer of fiction creates metaphors by an elaborate and concrete detailing of the actions and feelings of particular human beings. Sociology is background; individual psychology is the focus. The researcher tends to do it the other way around. His focus is on a wider field, and the individual life is seen in silhouette, by inference and suggestion. Also, the novelists proceed by showing. The researchers, using abstract social facts, proceed by reason, by logic, by argument. That is why fiction is apt to be more entertaining. Whereas Oscar Wilde or Evelyn Waugh shows us the idle and conspicuously consuming rich,

Thorstein Veblen argues them into existence. In the character of Sammy Glick, Budd Schulberg showed us the narcissist whose origins Christopher Lasch has recently tried to explain through sociological analysis. So there are differences among storytellers, and most of the time our novelists are more pleasurable to read. But the stories told by our social researchers are at least as compelling and, in our own times, apparently more credible.

What I am driving at is this: Once we rid ourselves of the false notion that we are scientists, and accept the idea that we are among our culture's most important tellers of psychological and social tales, the answers to the two questions I began with are obvious. As to what are legitimate forms of research into human communication, we may answer by permitting ourselves the greatest possible latitude. Historical speculation, philosophical argument, literary criticism, case histories, biography, semantic and semiotic analysis, ethnography—all these and more ought to be admissible as ways of telling our stories, and the less concern about method, the better. One becomes fastidious about method only when one has no story to tell. The best people in our field have, with few exceptions, been almost indifferent to the question of method. Who can characterize Harold Innis's method? Or Susanne Langer's? Or Eric Havelock's? Or McLuhan's? Or Mumford's? Or Jacques Ellul's? They used whatever social or historical theories and facts seemed relevant; they put forward their arguments by using the instruments of reason, logic, intuition, conjecture. Even Erving Goffman, who seems more technical than most, hasn't much of a method; what he has is a metaphor: that life is a stage and we are all players on it. Of course, we

can also count things, if we wish, and do correlational studies. But if we do, we ought to make it clear what social theory is serving as the frame for our story. George Gerbner does such studies, but only because he wants to tell a story of a people slowly, perhaps inexorably, being overcome by feelings of powerlessness. Stanley Milgram did such studies because he wanted to demonstrate that a commonplace of human experience—what we know about ourselves—can be more terrifying than what we don't know. Alfred Kinsey did such studies because he believed that official morality is, and probably always has been, rubbish.

And so, the answer to the first question is that by resisting the attractions of pseudo-science, and embracing the role of creators and narrators of social myth, media ecologists can enrich our field of study immeasurably. Of course, this cannot be done without risk. It means that most of us will generate piles of junk—unconvincing stories without credible documentation, sound logic, or persuasive argument. After all, how many Lewis Mumfords or Walter Ongs or Lynn Whites or Jacques Elluls are there? But then, how many Franz Kafkas, D. H. Lawrences, or James Joyces are there? It is a risk that must be borne. The alternative is to remain a shriveled pseudo-science, useless for everything except the assemblyline production of Ph.D.s.

As for my second question—What is the purpose of such research?—the answer is not, obviously, to contribute to our field, but to contribute to human understanding and decency. For the most part, novelists do not write to enrich the field of novel-writing. The good ones write because they are angry or curious or cynical or enchanted. The *Scarlet Letter* was not written by a man who wanted to improve the art of the novel, but by a man who wanted to improve the art of living together. Similarly, *The Myth of the Machine*, *Understanding Media*, *The Technological Society*, *Computer Power and Human Reason*, *Stigma*, *Anger*, *Public Opinion*, and, if you will pardon an attempt to guilt myself by association, *Amusing Ourselves to Death*—all these books were written by men and women who were concerned not to improve scholarship but to improve social life. Thus, the purpose of doing this kind of work is essentially didactic and moralistic. These men and women tell their stories for the same reason the Buddha, Confucius, Hillel, and Jesus told their stories. To put it plainly, the so-called social sciences are subdivisions of moral theology. It is true, of course, that social researchers rarely base their claims to knowledge on the indisputability of sacred texts, and even less so on revelation. But you must not be dazzled or deluded by differences in method between preachers and scholars. Without meaning to be blasphemous, I would say that Jesus was as keen a sociologist as Veblen. Indeed, Jesus' remark about rich men, camels, and the eye of a needle is as good a summary of Veblen's

Theory of the Leisure Class as it is possible to make. As social researchers, Jesus and Veblen differed in that Veblen was more garrulous.

Like moral theology, social research never discovers anything. It only rediscovers what people once were told and need to be told again. If, indeed, the price of civilization is repressed sexuality, it was not Sigmund Freud who discovered it. If the consciousness of people is formed by their material circumstances, it was not Marx who discovered it. If the medium is the message, it was not McLuhan who discovered it.

The purpose of social research is to rediscover the truths of social life; to comment on and criticize the moral behavior of people; and finally, to put forward metaphors, images, and ideas that can help people live with some measure of understanding and dignity. Specifically, the purpose of media ecology is to tell stories about the consequences of technology; to tell how media environments create contexts that may change the way we think or organize our social life, or make us better or worse, or smarter or dumber, or freer or more enslaved. I feel sure the reader will pardon a touch of bias when I say that the stories media ecologists have to tell are rather more important than those of other academic story tellers—because the power of communication technology to give shape to people's lives is not a matter that comes easily to the forefront of people's consciousness, though we live in an age when our lives—whether we like it or not—have been submitted to the demanding sovereignty of new media. And so we are obliged, in the interest of a humane survival, to tell tales about what sort of paradise may be gained, and what sort lost. We will not have been the first to tell such tales. But unless our stories ring true, we may be the last.