

Feminism and Science: Against the Marxian Apology,
the Freudian Recidivism, and Scientific and Political
Protection Societies

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1. This is a revised and expanded version of a talk delivered by Naomi Weisstein at the Barnard College Women's Center Conference on the Scholar and the Feminist, May, 1974. The first draft was entirely Weisstein's as was much of the subsequent work. But many of the ideas had been previously thought out in common, and the transition from first to final draft reflects a fully collaborative effort, both in thought and in writing. We speak as "we" throughout the paper except for a few autobiographical statements, where "I" is Weisstein. We are grateful to Vivian Rothstein for critical comments.

These days, science has a bad reputation on the left. This is partly for good reasons, but mainly for bad reasons. ^{TV}We want to speak up for science. Science-- and the scientific way of thinking -- can be extremely subversive, challenging to the standing order. The scientific way of thinking offers a way of challenging authority and thus a way of protecting ourselves against, freeing ourselves from, unjust authority and the chains of oppressing dogma. It is our hope that a better understanding of what science is about will help us to free ourselves from two such dogmas currently in vogue and/or in process of revival on the left. These dogmas are Marxism and Freudianism, and they are here subjected to the tests of evidence.

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What made me want to become a scientist? The trouble began with Microbe Hunters, an exceedingly subversive book about the early bacteriologists. (De Kruif, 1926). I remember reading about Leeuwenhoek's discovery of organisms too small to see with the naked eye. When he told the Royal Society about this, most of them thought he was crazy. And he told them he wasn't. It was simple: the "wretched beasties" were there, he insisted-- one could see them for oneself through the lenses he had made so carefully. It was very important to me that he could reply to them that he had his evidence; evidence was a very powerful thing.

My pantheon is populated with the crazies and the embattled in science, the ones who had their evidence and who trusted their evidence and the theories that predicted and were supported by that evidence more than they trusted the

powerful people, the royal societies, the established authorities who told them they were wrong. Babbage, who designed a real computer in the nineteenth century, and was ridiculed for it, and Rosalind Franklin, whose research told her what DNA was made out of, and who refused to be talked out of what she knew by the ridicule and frenzied theorizing of Watson and Crick (Watson, 1968). She had her evidence.²

Well, somewhere along the line, I decided: me too. I was going to be a scientist. Simple, right? Anybody can be president, so anybody can be a scientist. Simple and stupid; how could I have been so naive? Most children knew better. You could call those early days Weisstein in Wonderland. In fact, you could divide my life so far into three parts: Weisstein in Wonderland, Weisstein in Blunderland, and Weisstein in Plunderland. Weisstein in Wonderland is going to be a scientist. A scientist, wow! Weisstein in Blunderland has already started out on the trail: she's at Harvard (...Harvard!) where they tell her women don't belong in psychology, they aren't smart enough, and where they tell her, no, she can't have the equipment necessary to do her dissertation research because she might break the equipment. (This was certainly true; they broke it every week; I did not expect to be different). And so Weisstein in Blunderland goes some place else where they will lend her the equipment, and she finishes collecting her data and gets her Ph.D. in two and a half years and moves out of Blunderland straight into Plunderland, the profession. Well, not exactly, and not that easily: that was in 1964, when jobs were so numerous two-year olds were turning down offers from Yale. But for some incomprehensible reason they didn't want Harvard Ph.D's who were at the top of their class, at least not this one. "Who did your research for you?" they asked me on a job interview at the University of Chicago. "You certainly couldn't have thought it up yourself". (This last from a Professor there who thought of himself as a radical). Weisstein couldn't

get any of the dozen or so jobs she was considered for; when she finally got a lectureship, they wouldn't let her apply for grant money so that she could start doing research, start being a scientist. (I sat in, so to speak, in the Dean's office, until he let me apply. Describing my presence, he told a colleague, "This is Naomi Weisstein. She hates men.")

And finally, because of a number of things, because, in part, of feminism, Weisstein returns now and then to Wonderland. I remember one experiment I was working on, when I finally was able to get some money for research, where I was so curious about the outcome that I started worrying that I might not live to see it. I started worrying that, before I would be able to see how the data turned out, I would be run over by a truck or hit on the head by a safe falling out of a window, like in the comic books: Whiz! Oof! Blam! Aargh!

Science Affirmed

Because Wonderland continues to exist, we want to start this paper with an affirmation. We think that our endless curiosity, what appears to be our need to understand our world in a better and more complete way than it has been understood before lies at the essence of our humanity --- or at least it is a trait we have which seems to give us some glimpse of what we might be and what we might do in a better world. In our vision of a just and generous society, along with love and joy and honor and justice and dignity, science will be there, Leeuwenhoek with his lenses and Franklin with her x-rays. They will be there.

We also think the search for knowledge is deeply radical. It is radical, it is revolutionary to question the content of beliefs held so firmly and sometimes for so long that they seem to have been given by God. The search for knowledge is an attempt to roll back the existing layers of social fat built up around what appears to be true; it is an all-out attack on the status quo.

We want to affirm the enormous power of science as a tool in the search for knowledge. The scientific methodology involves a simple but incredibly

useful imperative: check your theories against the evidence. In astrology, if the facts don't fit the theory, you revise the facts; in science--- when it's working the way it should --- theories and systems of explanation are revised to fit the facts. (McCain and Segal, 1973).

It is this reliance on evidence and proof which has given science its tremendous power. Scientific method has allowed us to create real wonders, wonders that pass a very concrete test: they actually work. This has allowed us to put away the betel nuts and the tarot cards, the tea leaves and the ritual chants. Without evidence and proof we are dogmatists; we are astrologers; we are cultists. Without evidence and proof, we are ignorant and helpless, and ultimately we are tyrants, or victims of tyrants.

Science Indicted

As noted, science has gotten an awfully bad name, especially on the left. Some of this bad reputation is justified, and some of it isn't. Let's look at the charges, and see what about these charges is correct, and what about these charges is incorrect.

The charges are the following:

1. Scientists are war criminals
2. Bourgeois science discovers only bourgeois truths
3. Scientists are pig professionals.

Scientists as war criminals

There is a fair amount of truth in this indictment. Anyone observing science in the modern era would have to be blind to ignore the atrocities to which scientists have given their expertise. This charge requires very little of our time, not because it is unimportant, but because the evidence is so overwhelming: Dr. Strangelove, from V-2 to ICBM, the ovens and experiments of Auschwitz, Hiroshima, smart bombs, defoliants, napalm.

If this ~~was~~ all there was to science, there would be no reason to keep it around.

Bourgeois Science Discovers only Bourgeois Truths

But science has gotten a bad name not only because it connotes the dependence of scientists on the military and industrial giants they serve. It also has a bad name specifically among Marxists, for what is believed to be a deeper affliction than the flaws noted by radicals in general. Essentially, and ~~were~~ admittedly summarizing some rather complex notions in simple fashion, here is what Marxists have been saying about science (Gorz, 1974; Shroyer, 1970). Since, under capitalism, science serves the bourgeoisie, it follows that the content of science itself will be in some sense bourgeois: bourgeois science is trapped in bourgeois categories. This isn't lethal to the natural sciences, according to Marxists, but it prevents the social sciences from acquiring accurate knowledge. The idea of evidence, the ideas of 'objectivity', the idea of scientific method, the concepts of prediction and control become a mask when applied to the social sciences, a buttress for bourgeois ideology, and a tool by which the bourgeoisie maintains power.

For instance, Shroyer (1970) remarks that "Established social science ... has allowed itself to be conceived as having the same...interest as the strict [I assume he means physical] sciences. Insofar as technical control [Shroyer here means prediction and control in the broadest sense] is the guiding interest of social science it...overtly legitimates class or elite exploitation."

Posing an alternative to bourgeois (and orthodox Marxian) views of science, Shroyer remarks that

"...our conception stresses that men (sic) are active in the constitution of their own world and of their own 'nature'. "

He then asserts that

"This process of self-formation cannot be conceived within a theory which assumes that knowledge represents 'structures' and is neutral in regard to its 'object'"; (p. 72).

In other words, you can't get there from a bourgeois social science.

But it is exactly this conception of humans as active in the "constitution of their world and of their own 'nature'" which bourgeois science has come to on its own, by following the laws of what Shroyer calls the "strict sciences". There were a number of theories which psychologists held a while ago, theories which could be roughly characterized as behaviorist, which seemed to merit the Marxist critique. At one time, in psychology, humans were seen as passive and rather stupid beings who did not have anything in their heads except what was impressed on them from the environment, and who did nothing actively to shape their environment. But even though this is what many psychologists might have started out with (and there were conflicting traditions all along the way), the evidence has led us to a much different account. Many psychologists these days even talk about a "paradigm shift" which has brought modern psychology to view humans as constructing their world, intelligent beings who actively shape their environment. Bourgeois science here had laws of its own, laws of evidence and theory which led to conclusions ideologically quite distinct from the bourgeois model.

The change in conceptions of mind occurred because the old categories simply wouldn't do; they weren't working; they didn't make sense when psychologists considered what developing knowledge in many branches of science told us humans were capable of. But it is not simply a matter of following the data where it leads. It does matter what questions you ask, and although it may be less likely, it is certainly not impossible for bourgeois scientists to question their science and their categories. An even more striking example of bourgeois science conflicting with the preferred bourgeois ideology comes from social psychology. Bourgeois social psychology, working in the context of a political system which would much rather have people blaming themselves for their own oppression, has come up with a highly subversive body of theory and evidence which shows that what people will and will not do, can and cannot do, will and will not feel, is overwhelmingly influenced by the ongoing social situation. Furthermore, changes

that people themselves can bring about in the ongoing social situation will then change their own behavior (~~Some of this is summarized~~ in Weisstein, [1968, 1971]). Ongoing is a crucial term here. For these bourgeois social scientists with their stress on social conditions now have come up with an idea which is extremely radical: the implication of what they're saying is that in order to bring about change we don't have to wait for future generations which can grow up in a society better than our own; we can just change what's here, now, and we ourselves will change. Marxists who follow Freud with his emphasis on childhood personality formation should know that they are working in a much more conservative tradition than that of contemporary bourgeois social psychology.

Let us consider another interesting example, this time of research specifically intended to serve the interests of the state, in which the results nonetheless appear to come into conflict with existing bourgeois values and constitute an advance in knowledge. In The Crowd (1895), Gustave Le Bon saw crowds as mentally inferior, barbarians, acting by instincts, credulous,

"... among the special characteristics of crowds there are several-- such as impulsiveness, irritability, incapacity to reason, the absence of judgement and of the critical spirit, the exaggeration of the sentiments... which are almost always observed in beings belonging to inferior forms of evolution-- in women, savages, and children..." (p.35-36).

In his Group Psychology (1921)¹⁹⁶⁰ Freud began with a chapter on "Le Bon's deservedly famous work." (p.6) "We have made use of Le Bon's description by way of introduction, because it fits in so well with our own psychology in the emphasis which it lays upon unconscious mental life." (p.19) This complex of ideas, which we may call LeBonian, came under fire in the 'thirties and 'forties as bourgeois social psychologists detected a more rational side to human behavior. But it was still widespread enough in the early 1960's to come into conflict with American planning for nuclear war, especially the construction of shelters: if people were so

irrational, so much the captives of their unconscious, so prone to panic, then shelters would be useless. It is of some importance to note that this essentially Le Bonian idea was, at the time, part of the left's arsenal for use against the construction of shelters.

Clearly, the state would do well to figure out what people would really do after the bomb fell. A collection of studies on Panic Behavior, (Schultz, 1964) led to the conclusion that, "There is every reason to believe that, rather than panic, the dominant reaction of the American people will continue to be an energetic, adaptive response to whatever threats and dangers they face." (p.123) Of special interest in the consideration of the relationship of science to social system is the question of the auspices under which some of the findings were obtained. The book's bibliography was an elaboration of a bibliography on The Problem of Panic published earlier by the Federal Civil Defense Administration, one of the studies was reprinted by permission of the Department of Defense and another had been done under a contract with the Army Chemical Center.

This work, much of it done under the most reactionary of auspices (although it should be noted that many of the studies reprinted in the collection had been done earlier under independent auspices) found thoughtful, rational behavior where Le Bonians had seen only "panic". Now this bourgeois social psychology offers a somewhat inspiring view of human capacity, which probably is a valid view: the newer social psychology stands up to test, furnishes a basis for prediction, and, to give one example, offers an explanation which fits with many more of the specifics of the riots of the sixties than old Le Bonian views.

So if the Devil's research sometimes brings us closer to a valid picture of human nature than do ideas reigning on the left, it is certainly time that we stopped mouthing platitudes about the limitations of bourgeois science. There is, nonetheless, as always, an out for sophisticated Marxists, namely that the bourgeoisie needs to know what people are really like, and in this instance, it simply

influenced scholarship to move in directions useful to it. Precisely. But if bourgeois science can break out of bourgeois categories, then perhaps the most parsimonious theory to explain this would be that bourgeois science is not limited by bourgeois categories.

(There is another possibility, of course, which is that what we think of as results which break out of bourgeois categories merely show our ignorance, since they are actually confirming a Higher Bourgeois Category. One can indeed go on in this way, flying in ever decreasing concentric circles until etc., but is the goal really worth the effort? If the bourgeoisie itself should adopt socialism tomorrow, run up the red flag, dress in Mao jackets, ban unions, do all the other things that are called revolution, and then order the expropriation of their plants and their own execution, we presume that there would be some Marxist around to fit all of this after the fact into a Higher Platitude, such as: The bourgeoisie knew it was doomed and, showing the initiative for which their entire history as an emerging, then maturing, then dying class had prepared them, seized the time).

If the bourgeoisie wants to keep control, knowing the truth about "human nature" (either under capitalism or under a different system which might conceivably threaten capitalism) could help it to do so. Finally, then, since it is to the advantage of people in power to know anything they can which will be of use to them, and since it is not always clear beforehand what will be of use and what won't, it is ridiculous to speak of the bourgeois limitations of bourgeois social science. ³ Perhaps some of the confusion ~~is~~ ^{is} really from the fact that there is a good deal of social science which serves an ideological and propagandistic function. But there is also a fair amount of evidence that this part is intended for public consumption only, and that parts of the ruling class operate on a much more sophisticated basis. The people in those think tanks may be knaves, but they're not all fools.

Scientists are Pig Professionals

The third charge -- pig professionalism, the man's science, competition, elitism, careerism --- requires a more extended consideration. This charge is both too soft -- the truth is somewhat worse than the left imagines--and its conclusion is too simple-minded. A critique which ends by telling us to get out of the institution does not begin to tangle with the hard issues; unless the left does not believe that knowledge is worth searching for, it must go beyond simply equating that search with careers, ambition, and corruption. It must face the fact that it is impossible for most people in most scientific fields to do their work outside of existing institutions, and it must figure out how science might be set up so as to advance knowledge and then attempt to transform the institutions in accord with that radical vision.

Let me begin with an anecdote. During the days of the mixed left, there was an organization of left students and faculty called the New University Conference (1968-1972). There were a lot of bright white males in that organization, who would have risen at very least to the safe middle of their profession if the movement hadn't come along. I remember one of them very clearly. I had first met him in '62, before the movement transformed him. Back then I remember him telling me what he was doing his research on, and my responding in my innocence (this was Weisstein just out of Wonderland, with a couple of spangles still sticking to her shirt): why are you doing that? We both know it's trivial. He agreed: he did think it was trivial. He said: You're right, but you gotta earn your bread and butter some way or other. And so, when this same bright white male grew hair and changed his life and joined the movement, and told me that if I didn't get out of the university I was selling out, I understood exactly what he meant. He meant, get out of the corruption, which was the only thing he ever knew. Because he never did know what it meant seriously to seek out the answers. He never did experience the endless curiosity, the ecstasy of discovery. Of if he did, he didn't remember. All he knew was the game: the competition, the meaningless publications, the courting of power and

influence. And when he decided that this was disgusting, he never realized that what was most disgusting about it was its betrayal of what was so precious, so incredibly meaningful, so exciting. No matter how badly scholarship has been distorted by the demands of careers and institutions, by powerful men preserving their fiefs and dukedoms, there is something else to scholarship which is more precious than I can name. It is worth our life's struggle, just as feminism is worth our life's struggle.

The harsher charge that the scientific profession has betrayed science is largely correct. It is a rigidly hierarchical profession which believes profoundly that only white males can do science, and then only some white males (Holton, 1970)⁴; a profession which makes its expertise as unavailable as possible to its own practitioners and then believes in the near-divinity of the select who happen to see their way through the clouds of obscurity in which it shrouds itself; a profession which, today, much as in Leeuwenhoek's time, appears to reward trivia and punish truth, existing

"... for the purpose of getting money through knowledge or for gaining the respect of the world by showing people how learned you are and these things have nothing to do with discovering the things that are buried from our eyes" (Leeuwenhoek, quoted in De Kruif, [1926], p.24).

But even in Leeuwenhoek's day, much as he hated it, he needed the profession. Whereas an initial discovery might be made outside the profession, validation, communication, and criticism of that discovery requires the profession: Leeuwenhoek's hundreds of letters to the royal society make it perfectly clear that he, too, was working in the context of a profession despite his marginality to its rewards and respect.

i. The necessity for professional activity

For most sciences
there is no way you can do science outside the profession, and it is a romantic notion that the new left and some feminists have entertained that you can. Why is this? There are two kinds of reasons, one of which involves coming to terms with things as they are, the other of which involves conditions inherent

in scientific activity which, although presently vicious, are not necessarily permanently so. As a radical, a feminist, and a scientist, I want both to engage in science and to change the structure of the scientific profession. If I devoted all of my energies to trying to change the scientific profession, I would no longer be able to engage in science. This means that, at least in part, I must come to terms with existing realities. A major existing reality is the fact that scientific research involves the use of expensive equipment. You cannot figure out how human beings perceive by reading everything in the public library or in the British Museum. You must formulate your ideas, test them through experiment, reformulate them, refine them, test them ^{again}. You must present a stimulus lasting one thousandth of a second - precisely two tenths of a second after another stimulus, at precisely ten foot-Lamberts of luminance. For a while, I tried to get by on a flashlight and Wonder Woman reflexes. But it won't work. You need optics, electronic equipment, and finally, you need a computer.

But suppose my best friends all got together and gave me my optics and my computer, and I began to do experiments. This does not mean that I can figure out how perception works outside of the institution of science. Once I have the equipment and I've gotten the data and attempted to figure out what it means, I must present it for scrutiny to a community of trained scholars, many of them with conflicting ideas about perception. Were it not for the conditions under which that debate is held, and I'll discuss this further in a moment, I would have no quarrel with this procedure. Science, and scholarship in general requires this procedure: presentation of hypotheses, debate, disproof, development of new hypotheses. I have to struggle against my own set of immovable beliefs as well as those of others; I can't do this by myself. My best friends will nod and smile and hold my hand, and I'll love them for it, but it won't help. I need tough, informed, serious adversaries.

What's wrong with this procedure is not the debate itself, but the conditions under which it is presently held. It is part of the ugliness and macho

character of science that more often than not, these debates are held not on neutral turf, where the goal for all participants is that truth emerge, but in Roman Coliseums, where the crowds are out for blood, the lions haven't eaten for days, and you weren't issued a sword because of the shortage.

Why is science this way? Why is it an event held in a roman coliseum? In part, it is due to the governing ideology of the participants, which sets the tone of the debate. The ideology is called "excellence" and it has replaced the divine right of kings. In order to do science, you have got to be excellent. If you are not excellent, you don't deserve to do science. Academic departments devote a good deal of the year to finding people who are really excellent, to replace the overburdened assistant professors they just "let go" (fired) who were only excellent. Were academic hiring a matter of deciding who's more Jewish than somebody else, it would be much simpler and the criteria much clearer than it is when a department tries to determine who's an excellent scientist.

Who is an excellent scientist? Let's leave the Romans and their crumbling empire, and cut to the modern era. A shoot-out is in progress. And who is excellent? The man who survives the shoot-out is excellent. (Since women aren't allowed to be armed, the participants are generally men). How does he survive the shoot-out? Oh, he is quick; this stud is quick. His words and equations flow along, his answers are immediate. He is beautiful, known to associate with eminent, excellent, and therefore beautiful people who speak well of him. He's got thirty-six inches, thirty-six inches of publications in his vita. (By God! Does he turn them out in his sleep?) This beautiful young stud came here out of Harvard, by way of Stanford: Oh, see where he has been, and who he therefore is!

He is alert. He multiplies in his head. He has never been known to falter. But wait: a question has been asked. And he has paused; this is not good. He must not pause. The crowd begins to murmur; fully one-tenth of a second has passed and he has not answered. The suspense is unbearable. And then -- Good news! He has begun to sing "Melancholy Baby" in middle English. This is correct

This is the right answer. He has survived; he is victorious. The crowd is amazed. They begin to weep and kiss his feet. Here is a man who is truly excellent.

In other words, "excellence" is a set of mannerisms, external appearances, past itineraries, flashy skills, all having no necessary relevance to imagination, originality, or even to intelligence. (It is just conceivable that the traits normally seen as signs of "excellence" are in fact inversely correlated with excellence). Although it would be difficult to devise tests of genuine excellence, that is not the issue here. The ideology of excellence in fact buttresses the focus on the irrelevant and sets the tone of the scientific debate; ^{it makes doing science extremely unpleasant} and it harms science as a whole by excluding numbers of people who have real contributions to make, and who want to make these contributions.

The ideology of "excellence" excludes individuals; there are also interests at stake which would exclude any individual, no matter how "excellent" if that individual expounded deviant ideas. As will be made clear in the body of this paper, the relationship between ideology and interest is a very complex one. So, without saying anything much about which comes first, ideology or interest (in some ways, it can be shown that for science the ideology of "excellence" conflicts with interests) let me say something about the interests in this debate.

Scientists have interests at stake, interests in both the narrow, material sense and in a less tangible but equally real psychic sense. In that material realm -- let's call it "base"--the people with the established ideas, or the ideas that got them to fame and power, cannot give them up easily. It will mean public embarrassment, a slippage of power. Their security and survival are genuinely at stake if they are wrong too often; and once may be too often.⁵

But even if we succeeded in taking science out of the coliseum, or the Corral or the securities exchange, there may always be a struggle. There appears to be a resistance to new ideas above and beyond the fact that most people have something at stake in the old ideas. Perhaps there is an inertia to a body of belief; it is

hard to move that body, to upset coherent schemes about the nature of things. This has plagued the whole history of science, and it continues to plague it. It may just be true that the search for a deeper, better, and more complete understanding of our world will always be a struggle, in science and out of it. In the best of societies, conflict ~~should~~ remain; our task is to design a society in which that conflict will be resolved in ways which are as fair and humane as possible. We must devise a fair and humane institutions within which the scientific struggle can occur.

ii. Science as a protection society

If the description of the institution of science we've given is correct, we might think of this institution as it is today as a protection society. The people in charge want to stay in charge and they want to keep their ideas dominant. To further this end, they reward those who expand their ideas and expand their empires. You can rise in the ranks by doing, in effect, other people's work, with appropriate reference and deference to it -- you become cap, consiglio, lieutenant, by expanding endlessly on Bledsoe's paradigm (whatever Bledsoe's paradigm happens to be that year or that month). Your rewards are in the tangible form of jobs, publications, research money; and in the intangible form of access to the informal networks of communication, the news, to the comment and criticism, and to what's going on.

In those inner reaches, you get to see rough drafts three years before others who have to wait for publication lags; you get to discuss the direction that your area is taking, its flow, you get to talk somewhat more freely about your work (the crowds at the ~~coral~~ have moved to the old chuckwagon, and joking is now permissible), and you get to talk about the work of others, you get the informed comments and the expert advice and the tough, serious criticism. And those inner reaches, ^{for instance} might add, are not just metaphorical and intangible. There actually exist, at least in psychology, two secret societies of which we are aware where fifty or so of the "really excellent" young scientists get together to make themselves better

scientists.

So ~~they~~ acquire protection for ~~their~~ work: ~~they~~ know ~~they~~ can survive, at least for a while; ~~they~~ buy some time. But ~~they~~ pay for this protection: ~~they~~ pay in terms of the meaning and importance of ~~their~~ work, in terms of its fundamental implications. It is not part of the deal seriously to challenge existing systems of explanation. And ~~they~~ pay insofar as ~~they~~ are obliged to take on the mannerisms which constitute "excellence", which means that ~~they~~ must conceal any hint of weakness or deviance, or sympathy for weakness or deviance, which is to say ~~they~~ must conceal ~~their~~ humanity. The deal is to accept the whole show.

For radical, feminist scientists, this deal is intolerable. And in this situation, so are our lives intolerable. We cannot bear science as it is, we cannot bear the trivialization and the dehumanization, but we will not stop being scientists. Having defined science as the center of our lives, we will not get out. So we stay, and try to fight, and hope that the rest of the left of which we are a part will support our struggles. But they do not.

The left as a protection society

The left does not help us fight because its answer is that all we have to do is leave the profession and we will have science and truth. But this is mindless and romantic. In the first place (to repeat) as scientists we need the profession. In the second place, since all truth-seeking occurs in some social context, the institutions or social contexts which we set up in its place may be no better. In fact, they may be worse. Were we to leave science or even to look outside of science for support, what kinds of institutions would we find on the left. Would they be better than those we had rejected? No. The chances for truth outside, in the left, are even more limited than they are inside, in science, all the horrors on the inside notwithstanding. There simply aren't enough skills, money, dissemination of information, and, --we want to stress this -- civil liberties on the left for scholarship to have much of a chance. The left retards our search for truth in ways

are the obstacles set up by

which are every bit as destructive and self-serving as the institution of science. Radicalism, too, is a protection society: it gives you a new context within which you may do scholarship, and it extracts heavy dues for this support.

What are these dues? First, let's divide the left into the subparts pertinent to this discussion: feminism (and the new left) and Marxism (and the old left). Consider feminism first, and consider, first, how being a feminist changes one's life for the better.

Feminism

i. The good fight.

Just as you can't do science outside the profession, we as women could not do science without feminism. Feminism unhinged that solid steel door to the male sanctuary of science. Feminism said: we are no longer going to be outcasts in this place. We are coming in. And feminism has been struggling to make this so: to make it possible for women to get hired at places where previously they would not have been hired; to make it possible for women to do their research who would not otherwise have been able to do so; and, maybe, hopefully, to create an atmosphere in which women come to believe that they can do science, that they can be scholars, that they can deal with the world and uncover its wonders. (I might add that feminism also made this possible vis-a-vis the left. Some memories just don't fade: I will always remember the ridicule, scorn, abuse, and indifference of males on the left to my ideas, and to my odd persistence in pursuing these ideas. I will always remember their ~~failure to comprehend~~ why I was concerned that I could not get a job, and later, that I had lost my job. As a woman, my ideas and my life simply didn't count in the heavy business of changing society.)

Secondly, feminism (and the new left critique, in general) protects us from some of the alienating and inhumane pressures within our professions by giving us an analysis which allows us to deal with them. We are not so easily pulled into that trap of blaming ourselves for the inadequacies and injustices of a system

which is almost always beyond our control. We understand the social pathology of the professions: the dues, the gunfights, the bloodthirsty crowds. And ^{profession's} as often as we can, we fight against the general social pathology. If we don't get guilt-tripped into thinking that our position as "intellectual" is a position of "privilege"⁶ we can try to humanize and radicalize our profession and our work conditions.

And finally, it is feminism (and again, the left tradition) which gives us our sustaining vision of fairness, justice and truth. This is what enables us to fight; it tells us that things do not have to be the way they are; it speaks to a better and more humane world.

So feminism protects us. But we pay out some dues, too. ~~We don't~~ mean the dues that we pay out for being radical and feminist; these are worth it. Rather, ~~we~~ mean the dues we pay to feminism.

ii. Dues.

There's a rather deep anti-intellectual streak running through the political movements of the sixties and the seventies. My political sisters for the most part did not understand my need to do science nor did they think it was important. In fact, it made me suspect. If I were into auto mechanics, well, yeah. Maybe even a little electronics. But vision research? Research on how the brain works? What for? It was really, as we all used to say, a heavy thing. I remember spending enormous amounts of time in my various collectives helping my friends through their attempts to smash monogamy, their becoming gay and then their return to bisexuality, their struggles around their personal life. After all, "the personal was the political." And I remember stumbling my way through the emotional stalinism of these years, tolerating their characterization of my life: reactionary. I wasn't smashing monogamy, I was in ~~one~~ of those disgusting, dependent, monogamous, heterosexual marriages (ugh!); I wasn't growing. And then I remember telling them about some of the struggles in my professional life: how, for instance, a colleague who had agreed to share a computer with me refused to do so, and how this was extremely painful, because it set my work back at least a year. There was no understanding

of my pain over this: the personal was political, but the struggle to do one's research somehow was not.

So while my feminism supported me in my professional struggles, many feminists opposed me or had a negative impact on my struggles. My feminism, and the growth of the feminist movement helped me get my toe in the door of the male scientific establishment: and then feminists told me to get the hell out.

They told me to get out because they didn't understand why I was there; and they told me to get out because they considered me tainted for being there. This is not only anti-intellectualism; it is also an imperative to exemplary behavior. My political sisters told me I was "hung up" on professionalism; I had no business being in a pig institution (the university) and in a male-dominated one (maybe I should have gone to a convent?). I got money from the government for research. I did not confront sexism each and every time I saw it, on the beaches, in the trenches,... I hired technicians who knew electronics, and most of the time, they were not female. (That was a later development; earlier, I had hired my feminist friends at a time when we thought that if you believed enough in your sisters, they could learn anything in two months). In short, I was not pure. I was assenting to the sexism of the institution; I was compromising.

As I said before, it is obvious that I couldn't do science outside the profession. I'm really not sure why the feminism I knew was asking me to, in effect, abandon science. And we don't want to digress too far, but we'd like to point out several reasons that we think it may be important for us to understand.

First, the movements of the '60's and the '70's never successfully shed their inheritance of the fine old American tradition of shoot first, ask questions later, or don't think, do. We know that this is part of the concept of what it means to be a real man, but somehow, many feminists took over this anti-intellectualism. It made you tough. And tough was what we needed if we were going to make a revolution. Don't sit around like buncha intellectuals; put your balls/tits on the line.

Second, a lot of feminists who came from the mixed left had a healthy distrust of the debates that took place among male heavies. They were cockfights, fast-gun gymnastics, the shoot-outs of science transferred to the left's OK ~~corral~~, and it was hard to see the content through the dust kicked up. But more generally, women coming into the movement, whether from the new left or not, had a distrust of intellectual debate, because males had arrogated that territory to themselves.

Because of this, we adopted an atheoretical approach, which in some ways made an exemplary moral stance our only badge of authenticity. That is, without level-headed analysis, and without some general, enunciated politics which included a view of what it means to be in the movement, to be a feminist, for the long haul, for life, feminism demanded continued, daily, ritualized, ever more extreme proof and re-proof that our commitment was real.

This seems to be changing now. Many of us have come to recognize the need for theory, analysis, thought. And with this recognition has come a measure of pluralism: the imperative to be "totally political", to devote all one's waking hours to politics has been replaced by a saner view which tries to figure out how participants in a movement can survive over the many years it is going to take us to change the way things are. Many of us, in brief, have come to realize that short-run politics burn out fast; we need to think of ways to create an enduring, long-term feminist movement, which makes room for many women doing many different things. And we need to create the political theory and analysis which directs and sustains this long-term view.

Marxism, the old left, and the new "new left".

i. Benefits. One model for a long-term politics is, reasonably enough, the old left. Here are traditions which have lasted; traditions which we may inspect and perhaps incorporate: patience, endurance, maturity, the long haul. Turning to the old left as a model may provide us with a sense of sustaining process; it may be a corrective to short-term politics in that it tells us we can

survive in some way or other as a movement, and as participants in a movement, without the kind of total exemplary moral stance which is impossible to sustain.

ii. Dues

Turning from the old left in general to ^{particular: Marxist} Marxism in political analysis and theory may actually foreclose our creation of an enduring feminist left. For, if feminism may be characterized in some ways as a movement in search of a theory, Marxism is certainly, at this stage in American history, a theory in search of a movement. Since it is, as we will argue below, a theory which is largely incorrect, which was irrelevant to the needs and demands of the movement constituencies of the 'sixties and is irrelevant to the needs and demands of feminism today, we must question why we are considering it at all. We must question whether we want to take our broadly-based popular feminist movement and try to understand it by cramming it into the confines of Marxian theory, analysis, and tradition: we must question whether by doing this we will deepen and strengthen our feminist movement, make it truly "revolutionary" and ~~capable of~~ bringing about total political, cultural, economic societal transformation, or whether the opposite will occur: while Marxian theory develops yet another corpus of "brilliant" analysis, our popular movement will shrink and disappear.

Our questioning of Marx should not be taken to imply that we are in no way saying that we should cater to conservative tendencies in our movement. Far from it. Nor am I saying that we should justify whatever is in the air, whether it be killer-dyke or open marriage. We must be analytic, self-conscious, critical; we must develop thought-out, careful, radical political analysis and theory: we must speak the truth, whether or not the truth is popular. What we are questioning is whether Marxian theory is the truth.

But, while Marxian theory is, as we will argue below, deficient in some fairly critical ways, and largely useless to feminism, it is the only theory (with a very few exceptions)⁷ which qualifies you as an intellectual on the left, and, increasingly, as a feminist intellectual. In this respect, then, the left funct-

ions every bit as much as a protection society as does science, with two exceptions.

First, the left doesn't have the money and the power scientists do. Second, it doesn't have to spend time arguing away evidence, since it believes it can ignore evidence. But the left functions as a protection society just the same. While you may be deviant from mainstream society because you are a feminist or because you are a leftist, the left itself has ways of distributing power, authority, and legitimacy: allowing your ideas to be published, to be taken seriously, to be treated as worthy of attention in the ongoing dialogue. You are given legitimacy if you stay within the established dogma, and you are ostracized, ridiculed, not taken seriously, ignored or denounced, if you don't.

As stated above, the established dogma is this: you have got to be a Marxist. And lately, you might do well to be a Freudian, too. Without adherence, lip service, obeisance to Marx / Freud, your theories are treated as invalid, your brain as mush, and your motives are suspect.

Dogma versus knowledge: necessity for detailed critique of Marx and Freud

Now if you have got to be a Marxist or a Freudian because these theories are correct, then that's not dogma. It's only if you have got to be a Marxist or a Freudian despite evidence against these theories that ^{they} become dogma, and the necessity to adhere to them radical "protection".

^{We were} When I was discussing science, we could make a formal critique without talking about the substance of any particular scientific struggle. We could talk about how easily dogma is accepted by scientists who ought to know better; how hard it is to fight the reigning fashions, whether or not they are correct. Most would agree. (We might even make me a slogan to take back to my science: We Must Combat Dogma Wherever it Appears, on the Beaches, in the Trenches....). But now, it seems, we're discussing the very body of tradition and insurgent activity that enables us to combat dogma, and we're calling this dogma, too. So we have to go beyond a formal critique, a critique of the institution, and deal with content. We have to show where

We think Marxian and Freudian theory are in fact dogma, not truth.

Here's the plan: We will first discuss both theories together, because there is a basic assumption common to both which we think is erroneous and outdated. Then we will discuss in further detail how this assumption applies first to Marx and then to Freud, and how it is wrong in both cases.

Joint Critique of Marx and Freud

The assumption common to both: predicting superstructure from base

There is a basic tenet in Marxian and Freudian theory which is common to both, extremely appealing as theory, and probably wrong.

The assumption is simply this: there exists a base, a small set of conditions which are necessary in order that a system (society or an individual) survive; and then there is a superstructure, which can be derived from a knowledge of the base. With Marx, you start with the productive forces and the conditions surrounding production, and you arrive at consciousness and culture. With Freud, you start with the pleasure principle, the raging instinctual demands and you arrive at the rest of what people do -- work, talk, stutter, develop hives, and so on.

Now this is, on its face, what one wants out of theory: coherence, cogence, a parsimonious way to describe and predict an otherwise chaotic array of unrelated events. But while this assumption is tight, and seems to make sense, it is open to serious question.

Now, how do we go about determining whether or not a particular assumption is right or wrong? We have talked previously about evidence as a tool in confirmation or disconfirmation. There is another way to say this: theories have to predict to be useful and valid. We don't mean this in a simple-minded way: we realize that in dealing with human behavior and society, validation of theory sometimes includes a good deal of after-the-fact evidence because that's what's lying around. In history, say, we can't do the same kind of controlled experimentation that we can do in psychology. But a predictive element must be there. If the theory is only good

after the fact, then it is no good at all.⁸

It turns out that whenever you look in detail at a behavioral theory which claims to be able to predict superstructure from base, you find that it can't; either it becomes circular, or it can't predict at all.

A look at a behavioral theory which attempts to predict superstructure from base.

Let us first give you an example from motivation theory in psychology, since both the assumptions and the evidence there are very clear.

In motivation theory, the same base-superstructure formulation was applied to animals. First, the question was posed: Why do animals do what they do? Animals do a bewildering number of things, not all of which make immediate (or even delayed) sense. The answer was formulated: animals do what they do in order to survive, and anything which does not look like it's done in order to survive nonetheless came about originally as a sort of byproduct of some kind of survival action. In other words, when we watch dogs tossing balls in the air and cats playing with half-dead mice before eating them, we can understand this behavior as "superstructure" -- conduct derived from base, which is in this case survival. This "superstructure" was forced into relation to base by the contention that these odd activities arise not as activities in themselves, but as necessary appendages to other activities that these animals undertook in order to survive.

Let us put it more directly in the terms the motivation theorists used. What do animals need in order to go on living? (besides the obvious minima for all living things: respect, a color teevee, and a little place in the country). In order to go on living, animals need food, water, sleep, oxygen, sex, perhaps, if the species is to survive. Suppose one assumes that all things an animal originally does stems from the need to ~~take~~ care of these things. Those wishing to be more general can posit that the way these primary drives make themselves felt is through a state of tension in an animal, and that all behavior can be thought of as tending towards "tension reduction". Freud's "pleasure principle", and his tying of all behavior to instinctual

energies is just an old-time way of putting the same thing.

Now, whatever animals learn, no matter how unrelated it looks to primary drive, can ultimately be traced back (if you try hard enough) to a history of what occurred while animals were on their way towards satisfying their primary drives. The other behavior stayed, so the theory argues, because through some mechanism or other it got confused with, or tied to, primary drive. It stayed even though it was not directly a function of survival behavior.

But it turns out that this formulation is completely inadequate to deal with what animals do. Robert White has argued this brilliantly in his classic paper, "Motivation reconsidered: the concept of competence" (1959). Monkeys will subject themselves to an extremely painful electric shock just to get to the other side of their cage where a window enables them to look out at what is going on in the laboratory, or where there is a mechanical puzzle that they can play with. In order to explore new areas of a cage, rats will run over an electrified grid which they have learned produces a painful electric shock. (Dashiell, 1925; Nissen, 1930). Pigeons and rats, having learned to obtain food by pressing a bar which causes a food pellet to be delivered, will continue doing this, even when their cages are provided with a full cup of food (Neuringer, 1969). Animals are active and curious as hell. To tie curiosity, work for the sake of work, search for novelty, exploration, to survival needs is actually to blow out the basic theory entirely. To argue that ^{of} all these contribute to a larger survival competence is to argue that anything an animal does is survival behavior, that curiosity never killed a single cat. But then the theory becomes completely circular, in that you can't predict beforehand what crazy thing an animal will choose to regard as necessary to its survival.

What this meant for psychology was that we had to revise our notions of why animals did things. Not all that they did related to primary drives, and either we had to expand the notion of what is necessary for survival until it included everything, like eating too many salted peanuts and sky diving and going on hunger

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strikes -- which both dogs and people do -- in which case, it loses all predictive power, or we had to admit that we were cut loose from a tight, coherent explanation of behavior. It meant that we were going to have to drift for a while on the observation that some of the things animals like to do are not necessary for survival: they like to play, and they are curious, and they like to solve problems and look at new things. Indeed Freud himself had to formulate a concept of "neutralized" energy to account for humanS' remarkable (if you start from Freud's theory) capacities to sustain interest in things not directly related to sex and aggression. But once you "neutralize" energy, there goes your theory, again. It becomes circular and unable to predict.

"It is worth noting that Freud's procedure in making the assumption of neutralized instinctual energy is similar to the one followed by orthodox behaviorists in connection with primary drives. These theorists started from the assumption that all behavior was powered by a limited number of organic drives and then, in order to protect this assumption, they developed further hypotheses, such as secondary reinforcement, to account for motivated behavior that bore no obvious relation to primary goals." (White, 1959, p. 310).

Now the point we want to make here is that psychologists not only had to admit that there existed a "superstructure" for animals -- that animals did things which were irrelevant or sometimes counter to survival -- but they also had to admit that the "superstructure" for animals had laws of its own. So we go from a nice tight theory with good potential predictability -- the notion that behavior is "powered" (to use White's term) by primary drives -- into a loose and vague area where we're just not sure why an animal is doing what it's doing. But unless we consider the range of what animals do, their "superstructure", we will not ultimately understand them.

It's still true, of course, that animals do things in order to survive, just as it's true that society must produce in order to survive. And it's true that the conditions of survival influence the behavior of animals, and the conditions of production influence the kind of society we live in. One can't drink poisoned water without being poisoned; power cannot be equally distributed under capitalism.

Base is a necessary condition for superstructure; however, it is not a sufficient

condition.

Another way to look at it is this. Events in the base are highly predictive of activities concerning the base. You know what's going to happen to an animal when it's been lethally poisoned. But events in the base are not highly predictive of activities which don't concern the base: you don't know what an animal will do when it hasn't been poisoned, or when the level of poison is sufficiently low so that the animal has a number of years to assimilate it. The critical point is that the conditions of survival do not uniquely determine much of an animal's behavior.

To complete the parallel: just as behavior cannot be predicted from primary drive, either directly, or indirectly, it may be that culture, social structure, and consciousness -- superstructure, for Marx -- cannot be predicted from base, from the things society does in order to keep going. It may be that superstructure is tied to base in such an indirect manner that studying base can predict only what will happen in base; not what will happen to culture. And finally, it may be that in order to change the base, you have to change the superstructure, not the other way around.

But a parallel is just that -- a parallel. It may serve as a heuristic device which enables us to rethink theory in a different domain, but it certainly doesn't disprove theory in a different domain. So whereas, with Freud, the motivation theory critique is more than a parallel since his theory of primary drives is the same as classical motivation theory and can be criticized on precisely the same grounds, with Marx, we have to turn our attention specifically to whether or not the same criticism applies in the domain of society and social change.

The Marxian Apology

Predicting superstructure from base: Marx in particular

i. Proletarian mission: the original prediction

Marx related consciousness -- an event in the superstructure -- to

base -- the organization of production. He predicted that a particular consciousness would be generated as a consequence of a particular dynamic in the base. Under capitalism, the industrial proletariat, because of their particular relationship to production, because of the social organization of production, and because of what would be their increasing immiseration, would develop the consciousness that a revolution is necessary or desirable. To put it in somewhat looser form, they would develop the consciousness that things are intolerably wrong and must be changed.

One has to look through some pretty weird lenses to find revolutionary consciousness among the proletariat in advanced industrial societies such as our own. Indeed, orthodox Marxists will tell me without flinching that the working class in our country is reactionary (at present, of course) or that the problem with which we all have to deal is the problem of why the working classes in advanced industrial societies are not revolutionary. (Just what kind of consciousness the industrial proletariat does have in any particular country, and why, is an enormously interesting question, involving a great deal of superstructural theory; but this is a tangent, and we won't go into it here).

ii. The Marxian apology: the concept of proletarian mission seen as an instance of a generalized method of predicting superstructure from base (or, if it worked so well in predicting the consciousness of the proletariat, maybe it will work as well for the rest of us).

The term "proletarian mission" may be used to refer to the prediction that the industrial working class will develop revolutionary consciousness as a necessary consequence of their relations to production, to the base. If Marxian theory is to be criticized, is it enough to point out that the proletariat have not yet fulfilled their mission? No way, says your really hip heavy Marxist. If Marx was wrong about his specific prediction, that does not vitiate the usefulness of class analysis. In fact, to point to the weakness of the concept of proletarian

mission simply shows one's ignorance: "... the [concept] serves as the simple handle by which the most unknowledgeable pedants find it possible to 'grasp Marxism According to the Marxian apologists, and 'scientifically' discard it". (Berland, 1966p.) Marx may have been wrong about a specific prediction, but if we ever are to be right, we will have to use the general method, the class analysis. It may not insure correct predictions (vide Marx); but using anything else insures incorrect ones.

It seems to us that at some point there is an irreducible minimum in Marxian theory, a point at which one either stays with the theory or discards it. This irreducible minimum involves the concepts of base and superstructure and of class analysis. C. Wright Mills offers a brief summary and paraphrase of some of these essential elements.

" 1. The economic basis of a society determines its social structure as a whole, as well as the psychology of the people within it. Political, religious, and legal institutions as well as the ideas, the images, the ideologies by means of which men understand the world in which they live, their place within it, and themselves -- all these are reflections of the economic basis of society.

This proposition rests upon the master distinction within Marx's materialist model of society: the economic base (variously referred to as the mode of economic production, the substructure, the economic foundation) is distinguished from the rest of the society (called the superstructure or institutional and ideological forms). " (1962, p. 82).

Mills' comments further define what constitutes the irreducible minimum in Marxian theory:

"There is a tendency among some marxists to attempt to 'defend' Marx's economic determinism by qualifying it. They do this in the manner of Engels' later remarks (made in letters) about the interplay of various factors, or by opposing to it a vague sociological pluralism, by which everything interacts with everything and no causal sequence is ever quite determinable. Neither line of argument, even when put in the abstruse terms of 'dialectical materialism' seems very convincing or helpful. Moreover, to dilute the theory in these ways is to transform it from a definite theory which may or may not be adequate, into equivocation, a mere indication of a problem.

Marx stated clearly the doctrine of economic determinism. It is reflected in his choice of vocabulary; it is assumed by, and fits into, his work as a whole... We may of course assume with Engels that he allows a degree of free-play among the several factors that interact, and also that he provides a flexible time-schedule in which economic causes do their work. But in the end -- and usually the end is not so very far off -- economic causes are 'the basic', the ultimate, the general, the innovative causes of historical change". (1962, p. 92--93).

A word about "neo-Marxists". At some point, they must decide whether they are or they are not Marxists. Mills calls the distinction between base and superstructure Marx's "master distinction", and we agree: without this-- and the resultant belief in the primacy of class analysis-- you're not really a Marxist. Our feeling is that underneath every neo-Marxist we've ever seen there lurked a plain old-fashioned Marxist, a person who would start sneezing and twitching and showing other signs of withdrawal were not a class analysis at hand. Class analysis is the key to understanding society, no matter how masked and jazzed up is this article of faith. Base predicts superstructure no matter how many qualifications have to be added to the original sentence. We challenge neo-Marxists who might contend that we are beating a straw man to argue their position for, say, ten minutes without letting the codpiece slip, that is, without exposing the traditional Marxist essentials. Any attempt to use Marxian theory, either by explaining what it actually meant (e.g., Berland, 1966), or by explaining how our present culture and consciousness arises as a consequence of the relations of production in advanced capitalist society (e.g. Zaretsky, 1973) retains (as it should if it is really Marxian) this inescapable tenet: base determines superstructure.

The autonomy of superstructure

2. Predicting the superstructure from the economic base

But the proposition does not hold. Mills offers as an example of attempts by those whom he calls "sophisticated Marxists" to salvage the theory:

(1) It is true, admits the sophisticated Marxist, that wageworkers in advanced capitalist societies are not revolutionary; they are not even as yet a class-conscious proletariat. (2) But, he argues, that is because of the intensive capitalist propaganda, the misleaders of labor who dominate the trade unions, the "labor aristocracy" that is bought off by the imperialist powers, the traitors who run the social democratic labor parties. (p. 97).

Mills, comments on this:

Note to reader:

Ignore Underling

The admissions of fact (statement 1) seem to disprove the basic theory, the proletarianization of the workers, but are they supplementary explanations (2) contained within the theory, or do they constitute new theories? The explanations suggest the decisive, and possibly autonomous role of the cultural apparatus as part of the superstructure in the formation and persistence of political ideologies. (p. 97).

Mills discusses different kinds of disconfirming evidence for this proposition:

[Note to reader:
ignore underlining]

In various capitalist societies, political policies have greatly modified the economic base—and the social effects of economics upon all strata of the population. The welfare state is not "determined" by the mode of economic production, although of course it is made possible by economic developments. What is politically possible within present-day capitalist economies undoubtedly is wider than Marx's doctrine would lead us to expect. Just how wide it is, we cannot predict, but there is nothing inherent in the capitalist economic system that prohibits political history-making, including reform and deliberate change of the economy itself. (p. 125)

and he thus comes to some very strong conclusions about the autonomy of superstructure:

Neither "consciousness" nor "existence" altogether determines the other, ...
But "intervening variables" are also at work: the means of mass communication, the machinery of amusement, the cultural apparatus—in brief, features of the ideological superstructure. Such variables mediate the relations of "existence" and "consciousness"; they affect each of these and they affect their interplay. They can play and often do play an autonomous role in the development of class consciousness or the lack of it. Existence itself is subject to the definitions of reality carried by the cultural apparatus. (p. 113).

ii. Predicting consciousness from structural factors other than the economic base.

Consciousness appears to be autonomous not only ^{in relation to} the economic base, but ^{also in relation to} other non-cultural or structural factors as well. Although the target seems to be structuralist theories rather than Marxism, Alejandro Portes (1971) has presented strong evidence for the critical role of consciousness as opposed to structural factors in producing radicalism, in his study of the lower class in Santiago Chile in 1968--69.⁹ Portes begins by testing this hypothesis: "the higher the socioeconomic status of an individual, the weaker his attraction toward radicalism of the left".^(p.31) He finds no significant connection between radicalism and such variables as occupational status, personal income, family income, or education.^(p.31) Perhaps, Portes continues, the "crucial stratification factor" is not "static economic position"/but rather "degree and direction of movement"^(p.33)

(p. 33)
 hierarchy"/— in other words, social mobility. But the downwardly mobile turn out to be "even less likely to embrace leftist radicalism" than a comparison of their past and present status would predict. (p. 34) Well, perhaps this gives support to those who, "following a more devious theoretical path, have predicted the opposite: a positive association between upward mobility-- as a factor producing relative deprivation and leftist extremism" (p. 33) No; this one doesn't work either. (p. 34) Portes next tries out the theory that migration into Santiago is connected with radicalism, that it is recent migration "which forms the crucial determinant", and the contradictory hypothesis of "increasing relative deprivation and, hence, receptivity to leftist radicalism among older migrants" (p. 35). He concludes: "no support is found in these data for hypotheses linking migration or different periods of urban residence by migrants with leftist radicalism" (p. 35). Moving on to "social-- psychological approaches" (p. 35), Portes similarly finds no significant correlation between radicalism and "differences in expectation of goal-fulfillment" (p. 36), nor, finally, between radicalism and "frustration with a lower-class situation" (p. 37).

Where, then, does radicalism come from? It is, says Portes, "a complex attitudinal orientation characterized, not only by intense emotion, but also by a definite cognitive approach to social phenomena" (p. 39). Radicalism is not a "direct, automatic consequence of a frustrating situation without mediation of intervening cognitive variables" (p. 39). "The systematic views of society embodied in leftist radical ideologies are neither self-evident nor automatically elicited by the emotional prodding of social and economic deprivation" (p. 39). It is not, then, the social structure itself, nor attitudes following from one's position in the social structure, which cause radicalism, but rather something quite different: the realization that the existing social structure is to blame -- in other words, "consciousness" (p. 39).

iii. Some striking instances of the failure of base to determine superstructure.

If base doesn't determine superstructure, and if underlying social structure does not determine consciousness, then we must radically rethink our ideas about agencies of change and about what makes change happen. The sixties came as a surprise to all of us, including Marxist theoreticians. The black and student movements, left nationalism, third-world struggles, feminism -- none of these were predicted. Attempts were made only after the fact to account for the rise of these movements in Marxian terms. So students were resisting "proletarianization" because they still thought they could escape it; blacks were not integrated into the system of capitalist production the way whites were; and (once heavy Marxists realized they could not talk the women's movement out of existence by pointing out our trivial relation to society, or at least to what makes society change), the relation of women to the base was once again rediscovered in the political economy of housework and the reproductive, sexual and socialization functions that we served.

(Whereas some of the post hoc analyses sounded pretty convincing, they were never convincing in the case of women. For it is precisely our relationship to the base -- to the way reproductive, socialization, sexual and maintenance functions are organized--which makes us isolated and privatized, and thus according to Marxian theory or any distant relative of Marxian theory, unable to act as a united social force against our oppression, or even to realize our oppression. We would be, in this view, something like the peasants that Marx talked about (Marx, 1852, 1959). One of the last groups to understand what was happening to them.

But since we have realized our oppression, we must go back and yet again rework Marx. [Or perhaps Marxists will claim that no significant number of women have come to some consciousness about their inferior status in 1974. We'll match the proportion of women in 1974 who know something about their oppression against any other group Marxists choose to invoke.]

The main Marxist input to all of this -- the new left, feminism -- was the imperative: Make Contact With the Working Class! And among our most clear

The day our friends indeed made contact with the and painful recollections of the student movement was Workingclass: They Became Working Class. They dropped their t's and substituted d's; they talked funny, they walked funny, they rent their bourgeois raiment (levis) and abandoned their bourgeois ways. They got married, had babies, threw out their dope, opened up the beer, cut their hair and started payments on the color teevee. It was a sight to see.

And today, in the women's movement this farce is repeating itself as farce, in the ~~breast~~-beating it requires of us about how we're "only" middle-class (or sometimes in our insane vanguardism when we can say you're only middle-class). It persists in our drive to relate to what the Marxists tell us would be a "real" movement -- that of working-class women. Now this really isn't Marxism, it's just that a lot of people think it's Marxism; it's actually somebody's idea of a joke. There's a body of Marxian theory which at least justifies or attempts to justify proletarian mission, even if it can't make it happen; there is no way to get from that theory to one in which those marginal to production (whatever their fathers, husbands, or ex-husbands do) can fulfill that some function merely because "their men" are involved in industrial production. Indeed, ~~the latter is a~~ profoundly sexist formulation -- not because it assumes that women's status can be determined from the status of "their" men (which is in many ways the case in our sexist society", but because it implies that the oppression of women is so mild that we are more likely to catch consciousness by insertion from men rather than from our own suffering.

Some women do have a direct relation to industrial production. How many? According to one source [U.S. Department of Labor, 1972,] of the 40% of the women who work in the U.S., 15.3% are part of the industrial labor force. This comes out to 6.1% of the women in this country, a huge majority. But we're Marxists. A figure like that is not going to stop us. Let's try it another way. How much of the total work force in this country consists of women in the industrial sector? Industrial workers are 35% of the total labor force;

17 % of industrial workers are women; so 5.8% of the work force in this country consists of women in the industrial sector. Another huge majority. Any way you look at it, proletarian mission for the women's movement is guilt politics and ritual destruction. It's nonsense.)

In the case of the student movement, it may have been precisely because we were told to go working class, and thereby abandon our own constituencies, that we lost what we had, and that it didn't develop into something bigger. That is, the student movement had political consciousness, radical criticism of our society, a developing politics, and a developing vision. If we had kept at it, the result might have been to bring about similarly developing consciousness in other groups. Other groups may have said: "Hey. Lookit. Why aren't we doing that too? There's a bunch of people who are finally saying 'there is some shit we will not eat' Why don't we say it, too? (Information from a recent poll by Daniel Yankelovitch indicates that something very much like this has taken place by showing very forcefully the adoption by working-class youth in the '70's of a complex of anti-authoritarian attitudes developed by the student movement in the '60's . Attitudes are not, of course, synonymous with action. When we look at Yankelovitch's data we recognize the tragic fact that the student movement, which felt it necessary to destroy itself as punishment for its bourgeois origins, had it continued to act -- call its actions an ideology, "bourgeois", or what you will -- might have presented a model which would have suggested to other non-"bourgeois" groups that turning anti-authoritarian attitudes into a political movement was a viable and attractive alternative.) Certainly soldiers "fragging" their officers in Vietnam and going on search-and-avoid missions seems to have something to do with the notion of resistance to unjust authority and order which the student movement demonstrated. And the notion of resistance to authority itself appears to have been touched off by the black movement in the south. Indeed, one might entertain the hypo-

thesis that the early civil rights movement was the catalyst for all the later movements of the sixties: SNCC workers, with their discipline and disobedience to unjust authority, their community and participatory democracy, their strategy of collective moral witness, provided an example which the remainder of the developing movement imitated. In other words, the movements of the 'sixties may be seen as an instance of awareness through example, an event whose "base" is to be found in the superstructure. While we're talking about awareness through example a further perhaps heretical speculation: should we not be giving more attention than we have to the unintended effects of the "Kennedy spirit"? It is only a speculation, but it is reasonable to speculate that the growth of the various movements in the 'sixties was at least partially the result of a cultural mood defined by our adversaries. That is, although it comes bitterly off the tongue, Kennedy's rhetoric and activist pose in some sense set the tone for a whole new political mood.

To carry this through a bit further, one might suggest that if our women's movement keeps on going, gaining strength and challenging inequities of power, articulating our vision of a better world in such a way that it ~~shows~~ the present world ~~to be intolerable~~; developing our politics so that this vision spreads and becomes real in small and large ways, then our example, our revolutionary consciousness, will spread so far and wide that it may even touch the industrial proletariat. (We are not, of course, suggesting that consciousness through example is all it takes. Of course you need organization, structures, and all the flesh that makes for a political movement. Our scenario is not a mere replica of the street Marxist vision of those final days, this time with the women in the lead: all the children are let loose in the streets, the mothers refuse to mother, the teachers refuse to teach, the garbage piles up in the kitchen, the waitresses refuse to wait, the cooks refuse to cook, the telephone operators quit, the key punchers don't punch, the secretaries refuse to type, the nurses call in sick, the lettuce rots in the supermarket, the army comes in to buy the rest of the groceries and take care of the kids -- then, suddenly [loud chords from the

industrial sector] -- the plants shut down, the truckers refuse to drive, the coal stays in the ground and the oil isn't pumped; the furnaces cool and crack, and the power generators die, the patriarchy in shreds and capitalism-- *finally!* -- crawling around on its knees, wishing for the good old days of that harmless creampuff, Marxian theory. In all honesty, we don't think it'll happen this way, but our scenario does show that if you have a taste for these things, it's fairly easy to construct a model in which any group can be designated as the crucial agency of change). But in case the notion of women's movement politics spreading a revolutionary example is at all ludicrous -- I mean, the slightest bit ludicrous--you would do well to re-examine your own sexism.¹⁰ What we are suggesting here, tentatively, of course, is a dynamic in the superstructure: consciousness through example. We will have more to say about this particular dynamic later on, when we talk about social psychology and the obedience experiments of Stanley Milgram.

In summary: what we are saying is that Marxian theory, whether street or salon, hip or heavy, simple or convolved, is inadequate as a theory of society and as a theory of social change. It tells us, in one form or other, that we will be able to understand social change, social movement, society and consciousness only by understanding the base. And we are saying that this is not so. The superstructure has laws of its own, laws which can't be predicted from the base. We are saying that it is time we studied these laws. It is time we realized the importance of culture in creating and maintaining political consciousness, and it is time we started investigating the ways in which culture and consciousness work.

The Freudian Recidivism

Marxist Anti-Intellectualism: the path to Freud is paved with discarded evidence.

Recently, Marxists (Zaretsky, 1973; Mitchell, 1974) have begun to study one aspect of consciousness -- the "personal life" or "subjective experience," or individual psychology. Here, rather than drawing upon the enormous amount of knowledge that has accumulated in the last twenty years about mind, consciousness, and social behavior, these Marxist scholars have turned to Freud. Now this is

simply incredible. Why remain in self-imposed nineteenth-century ignorance about issues which are so vital to our understanding of social change, of society, of self, when the facts are finally coming in?

We think a key to the Freudian recidivism is the business we discussed earlier about dismissing bourgeois science as being incapable of saying anything accurate about human behavior. Along with this dismissal goes a profound ignorance of what science is about; evidence is scorned, and truth becomes whatever theory appeals to you. Thus when Marxists finally want to talk about the self rather than social forces, they tunnel back to good old Freud, the good old "revolutionary" of his time. If he was good enough for the nineteenth century, well, then, he certainly must be good enough for the twentieth, too.

And he certainly is good enough if evidence is not among your criteria for validity. Aquinas and Aristotle are also good enough (Ptolemy would be even better, but he didn't say very much about human behavior), but Freud will do, and anyway, he's kinkier.

The scorn of evidence takes on a note of alarming -- almost hippy -- militancy in ^{Juliet} Mitchell (1971, 1974^a). In Woman's Estate (1971) for instance, commenting on a classic experiment¹¹ which showed that if subjects are told a man wrote something they think it's excellent, and if they are told that a woman wrote the same thing, they think it's poor to indifferent, Mitchell says it's "fun" to know this,

"But don't we know it already? They are illustrations of our theory". (p. 165).

The answer is no, we don't know it already. It is evidence like this that allows us to make theories; and the more specific and detailed the evidence, the better our theories. It is precisely because we don't know it already that we assent to oppression: In the experiment, women subjects evaluated the stories the same way men did. We didn't know it already, and they

didn't know it already. We have to know everything we can about consciousness and the process of coming to consciousness: this experiment tells us something about social forces which coerce us and also about something of our view of ourselves which is self-evident only in retrospect. If we want to change a bad situation, we have to know as precisely as we can what that situation is, what is bad about it, and what causes the bad. Each of these three questions has an enormous number of potential and conflicting answers. We don't know the right answers until we formulate the questions in rigorous form, construct hypotheses (answers) and test these hypotheses in rigorous fashion. Now if you can pin down precisely what it is out there, if you can show the ways in which society denigrates us while at the same time showing that the denigration has nothing to do with us deserving it, then we have made progress. It is at this point -- when we begin to accumulate the evidence that our "inferiority" is society's hatred of us, not our own behavior -- that we can begin to come into consciousness, into politics. For Mitchell to dismiss this as "fun" indicates an alarming lack of understanding about what it means to "know" something.

Mitchell's dismissal of evidence as a criterion for the validity of Freudian theory is asserted again and again:

"...the absurdities of empirical refutation [of Freud's theory] (p.167). [She describes serious research as] vulgar empiricism ... post-Freudian empiricism ... has trapped most of Freud's tentative analysis of sexual differences into a crude and offensive rigidity". (1971, p. 167).

Dismissal of evidence allows her to deal with Eysenck's (1952) study, which showed the spontaneous recovery rates are well above the rates for recovery if you are in psychoanalysis, in the following fashion:

"Eysenck's claim that 44% of psychoanalytic patients improved, 64% of drug, shock-treated, etc. improved, and 72% of those untreated improved... was made in 1952 -- it has not improved with age and reiteration." (1974a, p.340).

Since 1952, a considerable amount of evidence has been accumulating,

a good deal of it confirming Eysenck's original finding that psychoanalysis doesn't help. We will consider some of this below; here we would just like to point out that one recent exhaustive survey (Bergin, 1971) shows deterioration in some patients as compared to untreated controls. Since, as Chesler (1972) and Tennov (1974) have shown, the majority of mental patients are women, Mitchell's cavalier dismissal of evidence, her bizarre assumption that the truth of a proposition is evaluated by its age and the number of times it has been repeated in this case amounts to a justification of continued atrocities against that very group whose advocate she claims to be.

Finally, Mitchell dismisses ^{Weinstein's} earlier discussion (1968, 1971) of the kind of personality theory which has been developed in the clinical tradition (in which ~~it is shown~~ that not only Freudian theory but most personality theory developed in such a tradition has no empirical foundation and no predictive or even descriptive validity) in the following manner:

"perhaps I like least of all ["Naomi Weinstein's"] [sic] experimental-psychological refutation of Freud" (1974a, 1974b, p. 300).

This dismissal of so much data would be shocking in one who keeps talking about science ("...scientific socialism" (p.92) ...[Marx's] great scientific discovery of the nineteenth century (p. 167)...[Freud's]"revolutionary"."new science;"a science of the mind" (p.167, 1971), if it were not so clear that Mitchell knows nothing about science and is simply using the term to sanctify the ideas she likes. The only refutation she offers of ^{Weinstein's} arguments is that they "suppose that all sciences are tested in the same way-- the way of the natural sciences. But on the contrary, a new science explores a new terrain, and has new, appropriate methods of proof..." (p. 166, 1971).

This is a striking statement, which could be used to call anything science. No science dismisses evidence, no matter how new, how revolutionary that science is, and the evidence against Freud is simply overwhelming.

About "vulgar empiricism": True enough, there has indeed been an empiricism which was simply a meaningless compendium of facts. But if Mitchell knows of an actual living scientist who still believes this is science, a Max

Weber Protestant Ethic award to her for her diligent labor, and, to the scientist, a Linus Pauling Vitamin C award for longevity. Facts without theories are useless and every scientist knows this (McCain and Segal, 1973). But ~~theories~~ without facts are equally useless, and few Marxists know this. This is no science without empiricism. If Mitchell thinks all empiricism simply "vulgar empiricism", tough. No matter what the science and what the proof, it is the evidence that counts.

In any case, what these "new, appropriate methods of proof" are, for Freud, or for Mitchell, we are never told. All we are told is that Freud's theory was revolutionary. Fine. ~~We're~~ glad to hear it. So is ~~our~~ theory that objects tend to fly up in the air unless held down. But don't call either theory science unless we have some proof for it. However, it is very important for Mitchell to call Freud's theory science, because that makes it Okay.

"In studying women we cannot neglect the methods of a science of mind ... That Freud, personally, had a reactionary ideological attitude to women in no way affects his science -- it wouldn't be a science if it did." (1971, p. 167).

What a way for a Marxist to talk! What is this strange and wondrous new science, a science that doesn't need evidence, a science that, alone among the behavioral sciences, is not tainted by the ideology of the man who propounded it? Only Freud climbed out of the categories ~~chaining~~ the rest of us; only Freud's science stands above his ideology while all the rest of us scientists are hopelessly in thrall to ours. (In fact, it can be shown, as discussed above, that science can divorce itself from ideology, but only through evidence -- through the very thing that Mitchell chooses to ridicule).

It is important that we call Mitchell's cavalier attitude towards evidence what it is: anti-scientific, anti-intellectual, anti-rational. She is not alone in such behavior -- it is common in contemporary Marxist work -- and we do not mean to single her out for special criticism. These kinds of attitudes had much to do with the self-destruction of some of the movements of the 'sixties. In the absence of a rationalist, experimental spirit, there is no standard by

which to evaluate conflicting data other than the assertion of some kind of authority. So the movement's anti-rationalism led to its authoritarianism.¹² A critique of the movement as anti-rational came often from Marxists. But when we look closely at Mitchell's scorn of evidence and experimentation as criteria for evaluating hypotheses, what we find is a Higher Anti-Rationalism. Mitchell is by no means solely responsible for these attitudes. She is working within a Marxist tradition populated by great numbers of people who have been quick to condemn new left anti-rationalism but whose work displays similarly a priori qualities. The words are classy, the analysis is fancy, but it comes down to the same thing: follow me, I've got the answer, and my new science can't be tested by any standards but my own. Timothy Leary's answer to people who ended up in mental hospitals after taking LSD was that they were on their way there anyway. Leary told me, when he was still a professor at Harvard in 1961, that my bad trip was due not to LSD, but to the fact that I had questioned its effects beforehand, and to my "unwillingness to relinquish control" -- which unwillingness can now be seen as a prepolitical feminist defense against the viciously sexist outfit which Leary and Alpert (now Baba Ramdass) ran. Leary's circular system allowed for no way of testing his claims. A decade of druggies tyrannized themselves and others in similar ways. And now we have heavy Marxists climbing on the anti-rationalist bandwagon, telling us about their revolutionary new science just ^{as} Leary told me about his, and singing, along with the old radio comedian, "Don't ask questions, just have fun".¹³ Sadly, it isn't fun.

Consciousness: The Modern Sciences of Mind and Behavior

i. Mind.

Mitchell faults "Weinstein" for failing to realize that Freud had created a new science-- a science of mind. Golly! The problem with this is that mind is one of the most vital and exciting areas in modern science. Brain scientists, neurophysiologists, vision researchers, cyberneticists, electrical engineers, people working in what is called "artificial intelligence", linguists, mathematicians: we're all trying to find out how the mind works. The science of mind has advanced

to the point where we can show how certain nerve cells in the brain will respond

to a particular thing in the environment -- a monkey nerve cell responding to a monkey hand, for instance -- [Gross, Rocha-Miranda, and Bender, 1972]] ^{or a cat nerve cell responding to a bar}

of a particular size and shape (Hubel and Wiesel, 1962) -- But we can show even more -- we can say something substantial in an area drenched with humanist mystification -- symbolism -- by demonstrating that ^{there are} cells in the brain ^{which fire to fill in}

an object only a part of which is actually presented to the eye. (Weisstein, 1970; 1973). We can go beyond the Freudian mystification of the unconscious, and identify those areas of the brain which are responsible for the initial filing and storage of memory. If we want a dream to be remembered, we can arrange that (Rechtschaffen,). We know that animals create and construct their perception of their world -- something as simple as looking at a pattern is an active, intelligent, symbolic, constructive process. And we have ways of measuring how this process occurs, and what it entails. It's a little hard to be impressed by Freud's hydraulic monster of a model of how the mind works when a science developed since Mitchell's birth of which she seems utterly unaware tells us so much more. If you ignore the modern sciences of mind you won't know how the mind works, and if you have contempt for evidence, you never will.

ii. Behavior

But, perhaps even more important, vital as are the emerging sciences of mind, they do not seem as directly relevant to the question of why we do what we do, why we feel what we feel, and how we will change as does the impressive evidence from social psychology which shows that, to a much larger extent than we thought, we behave the way people ^{expect us to} behave, we even feel what we are expected to feel and think what people expect us to think. I have summarized these data elsewhere (Weisstein, 1968, 1971) and cannot review them again now; but I do want briefly to describe again the Milgram obedience studies and point out their continuing relevance for the left. (Milgram 1965a, 1965b)

(1965a, 1965b)

In Milgram's experiments, a subject is told that he is administering a learning experiment, and that he is to deal out shocks each time the other 'subject' (in reality, a confederate of the experimenter) answers incorrectly. The equipment appears to provide graduated shocks ranging upwards from 15 volts through 450 volts; for each of four consecutive voltages there are verbal descriptions such as 'mild shock', 'danger, severe shock', and, finally, for the 435 and 450 volt switches, a red XXX marked over the switches. Each time the stooge answers incorrectly, the subject is supposed to increase the voltage. As the voltage increases, the stooge begins to cry in pain; he demands that the experiment stop; finally, he refuses to answer at all. When he stops responding, the experimenter instructs the subject to continue increasing the voltage; for each shock administered the stooge shrieks in agony. Under these conditions, about 62½% of the subjects administered shock that they believed to be possibly lethal.

No tested individual differences between subjects predicted how many would continue to obey, and which would break off the experiment. When forty psychiatrists predicted how many of a group of 100 subjects would go on to give the lethal shock, their predictions were orders of magnitude below the actual percentages; most expected only one-tenth of one per cent of the subjects to obey to the end.

But even though psychiatrists have no idea how people will behave in this situation, and even though individual differences do not predict which subjects will obey and which will not, it is easy to predict when subjects will be obedient and when they will be defiant. All the experimenter has to do is change the social situation. In a variant of Milgram's experiment, two stooges were present in addition to the 'victim'; these worked along with the subject in administering electric shocks. When these two stooges refused to go on with the experiment, only ten per cent of the subjects continued to the maximum voltage. This is critical for personality theory. It says that behavior is predicted from the social situation, not from the individual history.

And it is critical for the left. In some ways, the left may play the same role as those two stooges. If the left refuses to go on with things as they are, the influence this might have on other people may be simply staggering. It may not; we don't know. But we will never find out if we don't start looking at evidence like this and taking it further. We are not saying that we know very much yet about human behavior, about mind, ^{in this sense} because we don't. But if the left respected science, we could find out. We could experiment. If we want to know how people come to political consciousness, what makes them act politically, what makes any of us act in ways we would wish to act, with dignity, honor, courage, joy, humanity, what ^{allow} some of us ^{to} change and not others, what will ^{allow} all of us ^{to} change, how we can construct the social conditions under which power is equalized-- if we want to know any of these things we have to experiment, test, judge, reject, confirm. We have to be scientists. To exhume Freud, whose theories have been proven useless, when the work is laid out in front of us the way it is, seems to us to reflect an astonishing and dangerous ignorance, and a deep anti-scientific and anti-intellectual bias.

The Content of Freudianism

What's so bad about this guy, anyway; what, after all, is the evidence against him? It's not just that he's vicious towards women; more important, he's wrong, not just about women, but about humans in general. This has been discussed

in detail elsewhere (Weissstein, 1968, 1971); here, we want to make just a few comments pertinent to the discussion of base and superstructure, and to the discussion of evidence and proof.

i. Predicting superstructure from base: Freud's "insights"

The motivation theory critique applies as much to Freud as to any other theory which begins with a small set of primary drives and tries to predict all other behavior from this set. In particular, Freud posited sexual and aggressive energy as the sinister forces behind what humans do. We have already seen how this is inadequate to explain behavior as "simple" as that of rats exploring new territory-- as a theory of human behavior it becomes patently ridiculous.

In particular, with Freud's ~~pleasure~~ principle, either we ~~assume~~ that people do things (which are ultimately not motivated by pleasure at all), in which case we are not Freudian theorists, or we assume that pleasure is derived from the things that people do that cause them pain. To assume the latter, is, of course, every bit as circular as to assume that any other primary drive causes all behavior. In the Freudian system, we cannot predict beforehand the odd, painful things that people will do; we can only relate them after the fact to what initially gave them pleasure. It's a completely closed, ad hoc system.

But it is a very cruel system of explanation as well. We are ultimately blaming the victim. When I was training to become a clinical psychologist in 1961 (this was before I found out that there was a new science of mind, and clinical psychology wasn't it) the amount of hostility and blame generated towards patients was shocking. "Understanding" brought contempt. And we could understand everything; we were very knowing. All the pain we saw, all the psychic agony and terror, all the suffering and human misery were seen through the Freudian lens as devices which these people had constructed to express their sexuality or hostility. In the Freudian tradition, behavior the victim hates but cannot change is "really" behavior the victim enjoys. This "new science of mind" can be pretty vicious.

ii. A theory which can neither be proved nor disproved is no theory at all.

If one accepts Freud's ground rules, it is impossible to prove or disprove his theory. (In this respect, Mitchell is right when she speaks of the "absurdities of empirical refutation"; she might have put it better had she said that a theory which allows no empirical refutation is by definition absurd). In its simplest terms, Freud's theory of the unconscious is of the form:

P is true

The negation of P is also true

Therefore, anything I say goes.

If you are male, and you are not now and have never been sexually attracted to your mother because you've repressed this painful desire; if you want to er--ah--hmm, "sleep" with her that's because your unconscious has let the desire out. No evidence matters: it is the all-knowing therapist who has discovered what it is in your unconscious, and it's there, no matter what you think about it.

iii. Freud's theory can be disproved.

On the other hand, if we escape from the circular Freudian trap, we can use scientific method to test even this gibberish. For instance, do the 'insights' derived from this closed system help change behavior, as Freud claimed they did? The answer, shown in earlier studies summarized in Weisstein (1968, 1971) and confirmed by more recent work, is no (Bergin, 1971). Of the studies reviewed in Bergin's exhaustive survey of the outcome-of-therapy literature (studies which investigate whether or not therapy helped), for psychoanalytic therapies, Bergin counts ten such studies as indicating that the therapy has some beneficial effects, and nine as indicating no beneficial effects. (This updates Weisstein, 1968, 1971). Although Bergin takes this as 'modest positive' evidence for the beneficial effects of therapy, it seems more reasonable to conclude that therapy has little beneficial effect at all. Indeed, it might make more sense to suspect adverse effects. Consider the following factors: In the studies cited, the criteria for what constitutes beneficial effects are usually quite vague, both patients and therapists have an

interest in beneficial effects, and therapists themselves are usually the judges in these studies of what constitutes the success of their work. This latter factor would seem especially to pollute the data: asking people, in effect, whether they are committing fraud has never produced a high proportion of affirmative answers. This is borne out by internal evidence in these studies. Bergin says:

"When studies involving a report by one therapist of his own case outcomes are compared with studies involving groups of therapists, the individual therapists clearly win out: 55% vs. 39% positive outcomes " (1971, p.238).

One would assume that even groups of therapists would tend to evaluate beneficial effects at a somewhat higher rate than disinterested observers; yet when one compares this 39% positive outcome figure with a median rate of spontaneous recovery of 35%, the positive effects of therapy simply wash out. (Eysenck's 1952 study showed a much higher spontaneous recovery rate, 72%; the new figure is calculated from fourteen additional studies).

In addition, as noted above, it is a reasonable hypothesis that patients want therapy to help. So even if the outcomes were judged by the patients themselves (which some of them are) one would expect a positive bias. In three studies where one group is in therapy, and a control group has contact with the therapist although no specifically therapeutic procedures are used, the rates of recovery are equal. So therapy may help not because it is therapy but because it is one of the few situations in which a barbaric society provides individuals with simple human contact. This suggestion seems even more plausible when one considers the results of a study conducted for the Joint Commission on Mental Illness and Health (Gurin, et. al. 1960) where it was found that, of those people with personal problems, most of them did not go to mental-health professionals, and those who did were not as satisfied with the help they received as those who went elsewhere, that is, to Doctors, ministers, lawyers, and non-psychological agencies. At least 60% (in some cases more) of those seeking help from these diverse non-psychological sources reported that they had been helped; but of those who went to psychiatrists, only

46% gave positive reports. Of those who went to psychological agencies, 39% reported that they had been helped; the figure for marriage counselors was 25%. (This last is the lowest, and most interesting figure: one might speculate that the criteria here involve much more concrete kinds of things like whether or not a couple stays together; the fact that a set of concrete criteria exist in this case may account for the low rate.) Admitting the dangers involved in having such interested parties judge success, these figures at least give us some basis for comparison between contact with mental-health professionals and others. Apparently, therapists don't rank particularly high in the kind of human contact they dispense.

In outcome-of-therapy studies, it is
/ - not always clear whether patients in the non-beneficial effects category did not improve or whether they got worse. Many of the studies have only two categories: "improved" and "not improved or worse" (Bergin, 1971, p.248). In addition, many outcome-of-therapy studies show a greater variation in the resultant effects for those who receive treatment than for control groups which do not receive treatment. This means that the patients lucky enough to be receiving therapy are deterioration as a result of the therapy they receive. Some of Bergin's evidence indicating serious deterioration comes from a mental-health professional who had left the institution where the deterioration took place, and of this evidence Bergin notes "for the time being, the author and clinic must remain anonymous" (1971, p. 250). Indeed, of deterioration evidence in general Bergin says:

" In recent years I have received numerous communications from both therapists and patients who have provided rich detail regarding the process of therapist-caused deterioration. I have found some of these examples most disturbing, perhaps because I have been too naive regarding the way life really is. Apparently there are many areas of error and malpractice that are regularly covered up by practitioners in every field. It seems to be an all too common procedure to ignore these incidents, no matter how serious the consequences may be for the patients involved. Indeed, I hope that one of our suicide centers might do a careful study of the possibility of therapist-precipitated suicides. In general, deterioration of various kinds is much too common to be ignored". (1971, p.250).

This is pretty strong stuff. The data the public has access to appear

to come from a tight little club; only now and then does a *renegade* emerge to suggest how much more horrible things really are.

But if you're a Freudian, empirical refutation does not endanger your faith, for Freud was, after all, developing a "new science of mind" which can lead to a "revolutionary" understanding of oneself. But again, we must stress that any understanding, revolutionary or garden-variety, religious or secular, must be tested against evidence in order to be considered valid. Possession by the Devil Himself is as good an explanation as possession by a repressed unconscious if neither understanding allows us to do anything about our possession. And, to repeat, in any case, here is where Freud did make a prediction: if the unconscious were to become conscious, we would relinquish our neurotic symptoms. The prediction hasn't been borne out. So these "insights" derived from Freud don't lead anywhere; one must question whether they are insights at all.

iv. What's your disease? Every psychiatrist has a different answer. Your own psychiatrist fails to recognize you on Tuesday because you're wearing a wig; he concludes you're a new patient with a new disease and catches himself at the last moment when he recognizes your gold inlays and remembers what he thought you had on Monday. So be your own psychiatrist.

A bare minimum for any science, old or new, is consistency in description and some kind of consensus among observers. But there is no agreement on the clinical categories (derived in large part from Freud) into which people fit, and often individual psychiatrists display what might be generously thought of as schizophrenic conduct by diagnosing the same person displaying the same symptoms in different ways on different days.

Judges do no better than chance in identifying

which of a whole battery of clinical test results are the products of homosexuals and which are the products of heterosexuals (Hooker, 1957), and which, of a battery of clinical test results and interviews (where questions are asked such as "Do you have delusions?" (Little & Schneidman, 1959) are products of psychotics, neurotics, psychosomatics, or normals. Lest this summary escape your notice, let me stress the implications of these findings. The ability of judges, chosen for their clinical expertise, to distinguish male heterosexuals from male homosexuals on the basis of three widely used clinical projective tests—the Rorschach, the TAT, and the MAP—was *no better than chance*. The reason this is such devastating news, of course, is that sexuality is supposed to be of fundamental importance in the deep dynamic of personality; if what is considered gross sexual deviance cannot be caught, then what are psychologists talking about when they, for example, claim that at the basis of paranoid psychosis is "latent homosexual panic"? They can't even identify what homosexual anything is, let alone "latent homosexual panic".* More frightening, expert clinicians cannot be consistent on what diagnostic category to assign to a person, again on the basis of both tests and interviews; a number of normals in the Little & Schneidman study were described as psychotic, in such categories as "schizophrenic with homosexual tendencies" or "schizoid character with depressive trends". But most disheartening, when the judges were asked to rejudge the test protocols some weeks later, their diagnoses of the same subjects on the basis of the same protocol differed markedly from their initial judgments. It is obvious that even simple descriptive conventions in clinical psychology cannot be consistently applied; if clinicians were as faulty in recognizing food from non-food, they'd poison themselves and starve to death.

(a favorite category of Freud's).

I call it an apple; you call it an orange; my learned Freudian friend over there calls it a cockroach.

The Freudian Revision Industry

Just as with Marx, it won't do simply to point out all the shocking inadequacies in Freud's theory, its logical flaws, its inability to predict, and its disconfirmation through the only kind of evidence which is permissible as a test of the theory, and through the inability of practitioners to agree simply on its descriptive categories. The theory hangs on: fans argue that it is the method, or the insights which were provided or the observations that Freud made which are of such value. Or that he could walk on the water, or part the Red Sea. But the method is useless, the insights cannot be validated, and the observations cannot be replicated. If none of this weight of evidence is sufficient to overturn the theory, then perhaps it is time that we stopped carping about the evidence and give Freud his due: He is a great religious leader, right up there with the Maharajii, who can only envy Freud for the devoutness of his followers.

Let us point out the futility and circularity of the Freudian revision industry with ~~one further~~ example. Consider that old peanut, chenis-envy. I

Er, chestnut, penis-envy. (This is the critical juncture in our paper. For those in the Freudian tradition, this slip of the tongue clearly reveals us for what we are: a couple of uptight neurotics, busily repressing what's in our unconscious by intellectualizing. Our paper can now be dismissed on these grounds). Now, suppose we took a survey of all female children, and suppose it were to turn out that most little girls think penises are indescribably ugly, and that they're glad they don't have such horrible little things hanging down between their legs. The Freudians would laugh. They would point to our "absurd empiricism", and they would tell us that of course that's what little girls would say, just because they want penises so badly.

On the other hand, they might be more sophisticated than this, and they might try to convince us that Freud never really meant penis envy per se; he meant jealousy over unequal power. But the problem here is that such a reformulation shatters the base, the primary drive. The base is sexuality, libido, id. If you don't hang on to this, you are not a Freudian. So why use his terminology? You would be better off with your own.

Rock-bound belief systems

Now at this point, your street Marxist and your salon Marxist, your plain Freudian and your neo-feminist-Freudian, your simple-minded pedant and your pedant with one of those minds, the kind that drive you crazy with its unnecessary complexity, all of these might want to ask the same two questions: how come, if these theories are so inadequate, how come they have hung on for so long? Isn't this some kind of proof of their validity?

Let's answer the second question first. No, it's no proof at all of their validity, unless you also believe in the validity of walking on the water and of the power that ghosts have to come back and haunt the wicked. These ideas have hung on longer than Freud or Marx. (Ergo, they are more valid?)

But we think the first question is a really fascinating and serious one, and there may be a variety of answers to it.

First, we think we hold on to theories long after they are proven useless not because we are irrational, but because we are quite rational.

We want to have a coherent explanation for the way things are; we strive after understanding. If we haven't been trained to avoid the pitfalls of "understanding" without evidence, that is, if we haven't been trained to respect evidence and scientific method, then it is easy to see why these theories would live on and on. Post hoc analyses are very seductive; it takes constant vigilance to realize when you're into one.

The second reason brings us back to the concept of protection societies. People begin to have investments in the theories they propound; there's a left intellectual industry around, a market for one's ideas, points for having the right ideas, "standing". Earlier, when we were discussing science, we noted that even there, where evidence is supposed to be held in highest esteem, it is frequently ignored, and this retards learning. In Marxist/Freudian thought, evidence is disparaged. This doesn't simply retard learning; it insures ignorance.

So Marxism and Freudianism become ideologies which are used to buttress the standing order of left intellectual thought. Now it is not enough to explain why this happens by referring to the "base" that is, by referring only to the people who have vested interests: who have already committed themselves to these ideologies; who are careerists on the left; who are hiding their lack of thoughtfulness. A more serious question (and a superstructural question incidentally) is this: why do most of us, who would seem to have very little to lose by discarding these ideologies, hang on to them?

In general, we believe many things which are not true; a passionate attachment to fiction extends not just through Marx and Freud, but, as mentioned above, to a whole range of societal belief we would be better off without, such as the belief in our own inferiority.

This brings us back to the beginning of this paper. Scientific method is full of radical potential. Beliefs are held firmly for a variety of reasons, some internal, some having to do with power, one's own and others. One has to question the content of beliefs, struggle against the status quo of existing belief, challenge received authority, confront present authority. It isn't easy. One might even characterize the search for knowledge as one of the few genuinely permanent revolutions.

Summary: The relationship between being a feminist, a radical
and a scientist

Of course, you have to pay dues to the radical protection society only if you toil in the intellectual vineyards of the movement, only if you mess with theories about humans, and you want radicals to listen to what you say. You don't have to adhere to radical dogma if you work outside, in the professions, or if your theories don't directly connect, as in science. So why try to bring science and the left together? Because, when they are working as they should, they have much in common, and when they are not working as they should, they can help each other to do so.

What promise does such a linking hold, first, for science? The contemporary "scientific enterprise" might be radically redirected in accord with a more humane vision, a vision emerging from a sobered and strengthened left:

~~unf. the fundamental~~ questions asked, more honest and less brutally macho modes of conduct among colleagues practiced, new priorities for research allowed. Scientists could be free to be scientists.

Both science and the left share opposition to received authority. And just as the left, which challenges unjust authority, can help to humanize science, a science which challenges untested authority can help to humanize the left. Indeed, if the left had an ideology of experimentation, it would not be the denunciation carnival and dogmatic jungle it is today. Conflicting strategies would be seen

in an experimental spirit; criteria for what does and does not work would be developed so that knowledge about what to do could be accumulated and each new generation of leftists would not have to repeat the mistakes of preceding generations, either as history or as farce. Leftists, if they were better, like scientists, if they were better, would then see their ideas as tentative proposals to be tested and revised, as ideas to be changed and yielded graciously in accord not with authority, power, and, finally, terror, but in accord with mutually agreed upon criteria for what constitutes validity, in a spirit of common exploration.

This speaks to the cruel and debilitating sectarianism which currently marks the left. But it also speaks to more enduring and important concerns which will outlive the current contests and confusion. As we said at the beginning, there is a sense in which to be a good scientist you must be a radical, and to be a radical you must be a good scientist. This is so because radicals more than any other group need the truth. We need the truth because we need to change society, because it upsets the standing order to search for truth, because the search helps us to define a better society and because in our vision of that better society, we must include the truth-seekers --- the visionaries -- to give us some glimpse of what we might do and what we might be in a better and more humane world.

* * *

By doing radical scholarship, we feminists have little to lose and everything to gain. We have little to lose because we have been so marginal to the protection societies for so long. We have so much to gain because we have the truth to gain. For so long we have not been allowed to be scholars, scientists, movers, makers of change. We have been excluded from active participation in the affairs of this world. The grand theories of change did not include our participation; and the grand theorists hardly bothered to look at us, or, when they did,

they looked at us through the most vicious and hostile lenses. Well, we're here now, and we're looking. Let's make this looking fundamental, deep, true. Let's make our feminism such that the price demanded of our scholarship is the commitment to search for fundamental truths. Let us make our vision of what we could be and what we could do part of our ongoing feminism; let us include in our vision and our labor the search for fundamental truths, a search which treasures evidence as it overturns the old order of established beliefs and established authorities.

Notes

2. Watson's book, The Double Helix (1968) is a breathtakingly vicious account by an unrepentant, indeed, exultant, cowboy (see discussion of shoot-outs, below) of almost everything that is wrong with modern science. It is especially interesting to feminists because of Watson's barbaric treatment of x-ray crystallographer Rosalind Franklin.

3. This inability to decide in advance what will work and what won't is analogous to the halting problem in artificial intelligence (Minsky, 1967) which states that, given the simple rules of mathematics, there is no effective procedure for deciding in advance what is and what is not a solvable problem. Similarly, there is no way to decide in advance how we will get to the solution to any particular problem, nor, for that matter, what problems will lead to what solutions. This has implications, on the left, for the idea of "relevance". Sometimes, "relevance" has opened a fruitful path for new research; but it has also functioned as a device which the left has used to punish intellectuals, and, unfortunately, many such guilt-ridden intellectuals have willingly administered the punishment to themselves. Lemisch addressed this problem in 1970 (see also Lemisch, 1968):

"Relevance puts a heavier stress on the putting to work and application of existing knowledge than on the breaking of old categories of thought and the construction of new ones through the subjection of existing ideas to fundamental criticism.... the commandment to be relevant can seduce us into confirming what we already believe... "

And, speaking to the tyrannization of research by existing theory in a specific area, "the history of women might fit into a class analysis, but the requirement that it must might distort the truth, impede the liberation of women, and the development of theory not only about sex

and what is called intelligence."

Although this critique was aimed in large part at the activist new left, it also described Marxists' "tendency to see history's value primarily in terms of its utility in the construction of a theory of advanced industrial society" as

"a more hip form of instrumentalism which can be as limiting and distorting in its way as the relevance demanded by activists. If our aim is to find out how things work, to sharpen our history, our research must break out of the theoretical boxes to which we too often restrict it. The commandment that research be relevant either to current action or current theory, and the accompanying scorn of research not instrumental to these ends, stymies and inhibits our scholarship."

Today, we feel more strongly than ever the a priori quality of Marxist scholarship, and as time passes it becomes harder to distinguish the hard-core relevance of the new left from the Higher Relevance of the Marxists. Both are anti-intellectual in their exclusive instrumentalism; both bestow moral credit for the confirmation of their categories; and both are trapped in those categories and unlikely to move beyond them, regardless of the strength of the evidence which contradicts them. When all of this is combined with the venom of sectarianism, the fanaticism of correct-line-ism, and the left's utter failure to understand genuine pluralism and civil liberties, it leads to a somber conclusion: at present, bourgeois scholars are more likely than most left scholars to come to a consciousness that they are wet because it is raining, or that they are cold because they have no clothes on.

4. In the fall of 1970, Daedalus devoted its pages to exploring what Gerald Holton described as "the intellectual biographies of some of the men and some of the conceptions that have transformed science during the last few decades

(1970, p.933)". Commenting on this, he remarks that "... the personal development of diverse scientists ... shows that a set of ingenious social devices exists to seek out special scientific talent and to bring the acolyte quickly to the most fruitful frontiers of research" (1970, p.933). Since all of the scientists in the issue are indeed, as Holton noted, men -- guess what color? -- the ingenious social devices that Holton speaks of have to us some of the zero probability flavor of fixed gambling devices at Las Vegas... no, such devices would probably be too crude to be legal in Nevada. Holton's obtuse exultation in the health of science reminds us of the old elephant joke:

Every day, you get on the bus in the morning, and a man is carefully shredding his copy of the New York Times. As the bus turns from 59th street on to fifth avenue, he flings the shreds out the window. One day you get up the courage to ask him why he does this.

"Keeps the elephants off fifth avenue" he says.

"But there are no elephants on fifth avenue" you say.

"You see?" he says, " It works."

This fellow's logic resembles Holton's. Those "ingenious social devices" are exactly the inequities in our social structure which prevent certain kinds of acolytes/As if the elephants ever had a fighting chance on fifth avenue.

5. The rise and fall of immunologist William T. Summerlin offers a poignant example of the pressures under which scientists work. For some years the work of Summerlin and his mentor, Robert A. Good, was touted as a breakthrough in the field of immunology. During much of this time, the bandwagon effect was such that, as the director of the Neurosurgical Research Laboratory, Yale University School of Medicine, recently put it,

"At a number of national conferences over the past several years,

questions as to the adequacy of the description of the techniques used, as well as of the results obtained, were literally shouted down." (Taub, 1974).

But the time came when the weight of counter-evidence was too great to be ignored: no other laboratory could reproduce the results. Good, Summerlin's boss and president of Sloan-Kettering Institute for Cancer Research in New York, had become skeptical. Good was scheduled to discuss the matter with Summerlin on the morning of March 26th, 1974. On the preceding night, Summerlin had slept on a cot in the laboratory, as was his custom on Monday and Thursday nights. As Science magazine would later report, he "needed the time... to complete grant applications and get other paper work done." (Culliton, 1974 p. 1154). And that night, as on many other nights, Summerlin slept poorly. He got up at four in the morning, shaved, and went to look at his white mice who were to do their stuff for Good: their job was to show patches of black skin. He returned to his lab where, at five A.M., his secretaries -- who had been asked to show up early for the walkdown -- presented him with a surprise breakfast of crepes and champagne. Then, just before seven A.M., Summerlin and the mice got into the elevator on their way to high noon in Good's thirteenth floor office.

"This is when it happened.

Summerlin whipped out his felt tip pen and painted the skin of the two mice on top. During the meeting he showed them to Good as evidence of a successful graft." (Culliton, p. 1155, 1974).

We know these facts because the Sloan-Kettering affair has become a cause celebre, and scientists are piously wondering how it ever could have happened. "Why on earth would any rational man paint a mouse?" asks Science. Yes, indeed, why on earth? Who or what is to blame? Most fingers are pointing at Summerlin.

the impressario
. Some fingers have pointed at Good, who had

promoted Summerlin so uncritically before on the way up, and whose extravagant expectations Summerlin quite reasonably must have felt he had to fulfill.

But is it possible that to explain this incident we might look beyond Summerlin and Good or other individuals and ask whether there's something wrong with the way science itself works? Or, as Science put it,

" Is this bizarre affair representative of science?" Science thinks not: "... It does seem that the Sloan-Kettering affair has more to do with the personalities of the individuals involved than it does with 'science' in the abstract".

So here we have it. An individual mouse-painter, acting alone. Otherwise, all's well in the wonderful world of science. The Commission closes its books. We disagree. It is simply ludicrous to explain such an event by focusing on the personalities while largely ignoring the system of modern science. Of course, that system produces a Summerlin as well as a Good, and it will produce more of them, as long as incentives, rewards, and punishment work as they do in contemporary science.

One
6./Of the many guilt-trips for which the left has been attempting to sell tickets is the alleged "privilege" of scientists and intellectuals (e.g., Gorz, 1974). For a brief period in the late 'sixties, this excursion train made regular runs, packed with guilt-ridden, sobbing left intellectuals. But you can't run a railroad for masochists very long, and the paying clientele has now dwindled to André Gorz and sixteen Weathermen. For the beginnings of an explanation of this little train that couldn't, see the previous footnote for information about William T. Summerlin's "privilege" as exemplified in his working and sleeping habits.

7. The anarchist tradition may be an exception. Although we are in many ways in disagreement with this tradition, there is much in it we respect, as exemplified in such works as Murray Bookchin's "Listen Marxist" (1968) and Louise Crowley's "Lilith Manifesto" (1968), which still stands as one of the most clear, moving, and forceful statements of the anarchist-feminist vision.

Firestone's The Dialectic of Sex (1970) is a pseudo-exception.
genuinely

While there is much in Dialectic that departs from the Marxist tradition, in certain other respects (and admittedly this is an oversimplification which does not do justice to the book's sophistication) what Firestone has done is to retain the basic Marxist scheme and to substitute a new primary contradiction.

It may be for this reason that some Marxists have attributed to her work a centrality in the development of recent American feminism which does not match the facts. (Dunbar's early writing and/the original Cell Sixteen [1969], and Millett's Sexual Politics [1969, 1970] come to mind as far more significant contributions to feminist theory; and they have also had greater impact on the development of the movement.)

8. For some discussion of the possibilities for a scientific history which is somewhat different from what has been called "scientific history", see Lemisch, (1969b, 1970).

9. The following summary is taken from Lemisch (1975).

10. For an attack on the "politics of masculine joy," linking the movement's male chauvinism and its authoritarianism, see Lemisch (1969a). This had earlier been rejected on political grounds by SDS' New Left Notes.

11. Freud revised his notions of which were the basic instincts so frequently that it is difficult, as White says "... to pin down an orthodox doctrine" (1959, p.306). In much of his writing (e.g., [Freud, 1925]), tension reduction

as a whole, it seems fair to say that he assumed that behavior was motivated by tension reduction, a drive which, when satisfied, made itself felt as pleasure.

11. For a summary and discussion of some additional studies along these lines see Koestler, (1970).

References

- Bergin, A.E. "The evaluation of therapeutic outcomes" in Bergin, A. E. and Garfield, S.L. (Eds.) Handbook of Psychotherapy and Behavior Change: An Empirical Analysis. Wiley, New York, 1971.
- Foulkes D. and Rechtschaffen, A. "Presleep Determinants of Dream Content: Effects of Two Films." Perceptual and Motor Skills, 19, 983--1005, 1964.
- Berland, O. "Radical Chains: The Marxian Concept of Proletarian Mission." Studies on the Left, 6, 27 --51, 1966.
- [Bookchin, M.] Listen, Marxist. Anarchos, New York , 1969
- Chesler, P. Women and Madness . Doubleday and Company, New York, 1972.
- [Crowley, L.] Lilith Manifesto (mimeographed: Seattle, 1968).
- Culliton, B.J. "The Sloan-Kettering Affair (II): An Uneasy Resolution". Science, 184, 1154 --1157, 1974.
- Dashiell, J.F. "A Quantitative Demonstration of Animal Drive." Journal of Comparative Psychology, 5, 205--208, 1925.
- De Kruif, P. Microbe Hunters , Harcourt, Brace, & World, New York, 1926.
- Dunbar, R. "Female Liberation as the basis for Social Revolution". No More Fun and Games. 2, 103--115, 1969.
- Eysenck, J. J. "The Effects of Psychotherapy: An Evaluation". Journal of Consulting Psychology, 16, 319--324, 1952.
- Firestone, S. The Dialectic of Sex: The Case for Feminist Revolution. William Morrow and Company, New York, 1970.
- Freud, S. "Instincts and their Vicissitudes". Collected Papers. Vol. 4. London: Hogarth Press and Institute of Psychoanalysis, 30--59, 1925.
- Freud, S. Group Psychology and the Analysis of the Ego. Internationaler Psychoanalytischer Verlag, Leipzig, Vienna, and Zurich, 1921. Translated by Strachey, J., Bantam, New York, 1960.
- Gorz, A. "The Scientist as Worker: Speed-Up at the Think-Tank". Liberation, 18, 12--18 , 1974.

- Gross, C.G., Rocha-Miranda, C.E., and Bender, D.B. "Visual Properties of Neurons in Inferotemporal Cortex of the Macaque." Journal of Neurophysiology, 35, 96-111, 1972.
- Gurin, G, Veroff, J. and Feld, S. Americans View Their Mental Health. Basic Books, New York, 1960.
- Holton, G. "Lessons of the Intellectual Biography of Science" Science, 170, 933, 1970.
- Hooker, E. "Male Homosexuality in the Rorschach". Journal of Projective Techniques. 21, 18--31, 1957.
- Hubel, D.H. and Wiesel, T.N. "Receptive Fields and Functional Architecture of Monkey Striate Cortex", Journal of Physiology, 195, 215--243, 1968.
- Koestler, A. "Can Psychiatrists be Trusted?" Encounter, XXXIV 50--53, 1970.
- Le Bon, G. Psychologie des Foules, Paris, 1895. Transl. The Crowd. Viking Press, New York, 1960.
- Lemisch, J. "Who Will Write a Left History of Art While We are All Putting Our Balls on the Line?" (Pamphlet) New England Free Press, Boston, 1968.
- Lemisch, J. "If You Gotta Ask, Man, You'll Never Know" Independent Socialist, September, 1969a.
- Lemisch, J. "Listening to the 'Inarticulate': William Widger's Dream and the Loyalties of American Revolutionary Seamen in British Prisons" Journal of Social History, 3, 1--29, 1969b.
- Lemisch, J. "What's Your Evidence?" Radical Scholarship as Scientific Method and Anti-Authoritarianism, Not "Relevance". New University Conference Papers, 2, N.P. December, 1970.
- Lemisch, J. "The Many Minds of Jack Tar: An Exploration in Cultural History From the Bottom Up" in Young, A.F. (Ed.) The American Revolution: Explorations in the History of American Radicalism. Northern Illinois University Press, DeKalb, 1975.

Little, K.B., and Schneidman, E.S. "Congruences Among Interpretations of Psychological and Anamnetic Data. Psychological Monographs, 73, 1--42, 1959.

McCain, G. and Segal, E.M. The Game of Science, Wadsworth, California, 1969, 1973.

Marx, K. "The Eighteenth Brumaire of Louis Bonaparte." Die Revolution, 1852; Transl. from Foreign Languages Publishing House, Moscow. in Feuer, (Ed.) Marx and Engels: Basic Writings on Politics and Philosophy. Anchor, New York, 1959.

Milgram, S. "Some Conditions of Obedience and Disobedience to Authority", Human Relations, 18, 57--76, 1965a.

Milgram, S. "Liberating Effects of Group Pressure," Journal of Personality and Social Psychology, 1, 127--134, 1965b.

Millett, K. "Sexual Politics: A Manifesto for Revolution." Notes from the Second Year, N.P. 1968.

Millett, K. Sexual Politics: A Surprising Examination of Society's Most Arbitrary Folly. Doubleday and Company, New York, 1970.

Mills, C.W. The Marxists. Dell (Laurel Edition) New York, 1962.

Englewood Cliffs,

Minsky, M. Computation: Finite and Infinite Machines. Prentice-Hall, New Jersey, 1967.

Mitchell, J. Woman's Estate. Random House, 1971. (Our citations are from the Vintage Edition, 1973).

Mitchell, J. Psychoanalysis and Feminism: Freud, Reich, Laing, and Women. Pantheon, New York, 1974a.

Mitchell, J. "Etaoin Shrdlu" in Festschrift fur Gordon Piltdown, New York,

Ponzi Press, 1914, 1974b.

Neuringer, A.J. "Animals Respond for Food in the Presence of Free Food"

Science, 166, 399--401, 1969.

Nissen, H.W. "A Study of Exploratory Behavior in the White Rat By Means of the Obstruction Method." Journal of Genetic Psychology, 37, 361--376, 1930.

Portes, Alejandro, "On the Logic of Post-Factum Explanations: The Hypothesis

of Low-Class Frustration As the Cause of Leftist Radicalism." Social Forces, 50, 26-44, 1971.

Schultz, D.P.(Ed.). Panic Behavior. Random House, New York, 1964.

Taub, A. Letter to the editor. New York Times, May 8th, 1974.

Shroyer, T. "Toward a Critical Theory For Advanced Industrial Society"
Radical America IV, 65--78, 1970.

Tennov, D. Psychotherapy in press, 1975.

United States Department of Labor, Bureau of Labor Statistics, 1972, ^{p.97} in

The World Almanac and Book of Facts, Newspaper Enterprise Association, New York, 1974.

Watson, J.D. The Double Helix. Weidenfeld and Nicolson, London, 1968.

Weisstein, N. "Kinder, Küche, Kirche as Scientific Law: Psychology Constructs the Female". New England Free Press, Boston, 1968. Revised and expanded edition, "Psychology Constructs the Female, or, The Fantasy Life of the Male Psychologist (With Some Attention to the Fantasies of his Friends, the Male Biologist and the Male Anthropologist)". Social Education, 35, 362--373, 1971.

Weisstein, N. "Neural Symbolic Activity: A Psychophysical Measure". Science, 168, 1489-1491, 1970.

Weisstein, N. "Beyond the Yellow Volkswagen Detector and the Grandmother Cell: A General Strategy for the Exploration of Operations in Human Pattern Recognition". in Solso, R. (Ed.) The Loyola Symposium: Contemporary Viewpoints in Cognitive Psychology. Winston and Sons, New York, 1973.

White, R. "Motivation Reconsidered: The Concept of Competence". Psychological Review, 66, 297--333, 1959.

Zaretsky, E. "Capitalism, The Family, and Personal Life". Socialist Revolution, Numbers 14 and 15, (69--125 and 19--70), 1973.