Controversy

Anti-Business?

Steven Lagerfeld's article "An Anti-Business Business Magazine" is erroneously titled. *Business Week* is not anti-business and Mr. Lagerfeld never attempts to make a case that it is. He says that because the magazine has written some things he considers to represent liberal thought the magazine is not extremely conservative. Not being conservative enough for him is not the same as being anti-business.

Business Week makes no claim to being ultraconservative, just as it makes no claim to being ultra-liberal. The magazine's goal is to expose our readers to a wide spectrum of ideas, so they are aware of them, not necessarily because we or they believe them or should believe them. The magazine's objective is to be independent, a position an ardent apostle like Mr. Lagerfeld obviously considers intolerable.

To buttress his case, Mr. Lagerfeld has selected so carefully items to make *Business Week* appear to be of the left. He has ignored anything that would make the magazine appear to be conservative. For example, there is no mention of the supercritical book review of John Kenneth Galbraith's memoirs, in which the magazine's Deputy Editor William Wolman, highly criticized by Mr. Lagerfeld, wrote "...Galbraith also writes off anyone who fundamentally disagrees with him—especially conservative economists—as either idiotic or morally obtuse." And later he added, "... it is the capstone of a career that was part of a liberal era whose central ideas are dead or dying..."

I could find as many quotations from *Business Week* that liberals consider a sign that we are conservative as Mr. Lagerfeld has found to prove the opposite point. In fact, one can selectively choose quotations from the magazine to prove it is conservative, liberal, anti-conservative or anti-liberal. By reading *all* of the magazine an honest person has to conclude it is none of the above.

But his bias leads to such inaccuracy that the final result is dishonest. For example, he criticizes the fact that the publisher of the paperback edition of Lester Thurow's *Zero Sum Society* used a portion of the *Business Week* review as a blurb on the cover. But he ignores, or doesn't know, that the publisher of George Gilder's book *Wealth and Poverty* has used a blurb from the *Business Week* review of that in advertising for the book and credits the *Business Week* review, which was one of the first printed, with having given the book its first big sales surge. The publisher obviously thinks more of the review than Mr. Lagerfeld, showing that Mr. Lagerfeld is looking to see only what he wants to see.

Two other points raised by Mr. Lagerfeld are worthy of comment. *Business Week* has always been opposed to government intervention in the economy, preferring the forces of the market to act, where they can. But the magazine's view of the world is pragmatic, and we do not cling to hopes for free-market solutions when none can be forthcoming. That is why we laid out the causes of the decline of competitiveness of U.S. industry and some possible solutions. Incidentally, one function of a magazine like *Business Week* is to examine issues so that they can be discussed and solutions devised by those in business and government capable of doing so. *Business Week*'s editors concluded that the decline in competitiveness of U.S. industries cannot be solved solely by free-market forces because foreign governments are not playing by those rules and because no one will put the capital into industries such as steel, autos, textiles, and appliances to reverse the decline in competitiveness. To think that any investor, either individual or institutional, will pour capital into Wheeling Steel, or Chrysler, or any of a hundred smaller, less-known companies in basic manufacturing is to be either a fool or a knave.

Business Week was critical of the Kemp-Roth tax proposal—until the Reagan people included explicit spending cuts—then it supported supply-side economics. Incidentally, this view mirrors one held by much of the business community. Mr. Lagerfeld is either unaware, or ignores the fact that *Business Week* is perceived as reflecting a consensus opinion of the business community on almost all issues. I find that impossible to reconcile with your calling it an anti-business magazine.

What Mr. Lagerfeld ends up saying is that if you don't agree 100 percent with me 100 percent of the time, you are clearly a leftist. His and your calling *Business Week* anti-business does not make that true.

I am disappointed that the standards of scholarship of The Heritage Foundation and *The Public Policy* [sic] are so shoddy. I would have hoped that they would be higher.

Lewis H. Young, V.P.
Editor-in-Chief
Business Week

Steven Lagerfeld replies:

Lewis Young defends *Business Week* with the proud assertion that the magazine "makes no claim to being ultraconservative." But whoever had suggested that *Business Week* made—or ought to make—such a claim? No one at all. Mr. Young is merely trying to pin the boo-label "ultraconservative" upon me and *Policy Review*. What Mr. Young is saying is that, if you don't agree 100 percent with me 100 percent of the time, you are clearly an ultraconservative. Which illustrates the standards of *Business Week* far more neatly than anything in my essay.

The magazine's strange political perspective is illustrated yet again when Mr. Young remarks that only "a fool or a knave" would expect private investors to put their money into failing businesses, but neglects to wonder what kind of person would automatically assume that government should undertake what its citizens wisely avoid.

Mr. Young accuses me of "inaccuracy" and "shoddy" scholarship, yet he disputes none of my facts—he merely disagrees with my interpretation. I illustrated my argument with a great number and variety of citations. Indeed, a large part of my essay was devoted to *Business Week*'s special issue on "The Reindustrialization of America," which the magazine itself promoted lavishly through a series of full-page advertisements in national newspapers. I hardly think this represents an unduly selective reading of the magazine.

I was also careful to note on several occasions that there are exceptions to the prevailing leftist tone at *Business Week*, and I specifically observed that the magazine accorded George Gilder's book a lukewarm but respectful hearing. (The book's publisher, incidentally, managed to excerpt exactly one word from the review for the blurb.) However, such instances of fairness are dismayingly rare in *Business Week*'s pages. As for the magazine's review of John Kenneth Galbraith's book, it merely shows that *Business Week* is capable of rejecting one set of discredited liberal ideas for another.

Mr. Young is right about one thing. My article was "erroneously titled" in that, in the original manuscript, a question mark followed the title. I failed to notice its subsequent omission by the editors. I suggested in my essay not that *Business Week* is openly anti-business (it would be out of business if it were), but that it exhibits a set of values and beliefs characteristic of the anti-business stance. Because *Business Week* aspires to educate and speak for the business community, I thought it useful to examine the peculiar attitudes that inform its views. If, as Mr. Young suggests, *Business Week* already reflects a consensus opinion of the business community, God help us.

U.S. Unionism:

The comment ("The Three Faces of Unionism," Fall 1980) by Messrs. Troy, Koeller, and Sheflin (TKS) on our article "The Two Faces of Unionism" (*The Public Interest*, Fall 1979) raises two issues: 1) Did we fail in our analysis to treat adequately the impact of unionism on government's activity in the labor market?; 2) Did we incorrectly read the evidence about union effects on efficiency, income distribution, and social/political outcomes in the United States? In our response we wish to stress that TKS have ignored almost completely the quite extensive body of empirical work on the impact of unionization in the U.S. completed during the past decade (over 75 studies by over 40 different authors), and have based their attack primarily on three earlier articles for the U.S. which shed little empirical light on the issues of concern, and on two pieces on U.K. unionism, which have no obvious relevance for an analysis of unionization in our country.

Like other organizations, such as firms, unions try to encourage the passage of laws which serve their interests. This is no more a "third face" of unionism than it is another "face" of business. It is a *method* by which both labor and management seek to meet their monopoly and collective voice goals. We pointed out in our *Public Interest* article that, in general, organized labor has not been effective in pushing for policies that increase union monopoly power. We said, "Typically, only when unions and management in a particular sector have united in favor of legislation to benefit that sector have unions had much success in gaining support for their 'special interest' legislative proposals." This is certainly the case in the example cited by TKS with respect to the trucking sector—a study, we note, by one of our students. Where unions have been more successful in the political arena is, as we say in our article, in pushing for more general social legislation like the Civil Rights Act of 1964, the Public Accommodation Act of 1964, the Voting Rights Act of 1965, and acts designed to fight poverty.

It is in one way easier and in another way more difficult to respond to the TKS criticism of our reading of the evidence on union effects. It is easier because their discussion is marred by some obvious factual inaccuracies due, as indicated above, to an apparent failure to read much of the last decade's literature on U.S. unionism and a consequent reliance on studies relating to the United Kingdom or on a few selected studies relating to our country from a decade or so ago.

It is more difficult to respond to the TKS critique of our work because of the very large number of errors which they have made. To keep our response near the required length, we will limit the discussion to TKS's criticisms of our conclusions regarding efficiency and income distribution, the issues given the most attention in our research and in our article, and shall further limit ourselves to only their most flagrant errors.

TKS refer to their "evaluation of the empirical data concerning the efficiency effects of unionism," but they do not in fact refer to a single recent empirical study on the U.S. that supports their views. They cite one study from the U.K., Milton and Rose Friedman's views, and Albert Rees' 1963 guesses. In our work we referred to Professor Rees' conjectures as suggestive because he admitted his analysis was *not* based on empirical evidence (due to unavailability), but rather on an assumption about the key parameter determining the amount of societal loss in a standard welfare economic approach: the elasticity of demand for unionized labor. Professor Rees guessed that the elasticity was about 1. TKS, without referring to the extant findings on the topic of concern and without any investigation of their own, arbitrarily double Professor Rees' guess to allow for featherbedding. Does actual evidence suggest that Professor Rees' guess is high or low? Recent estimates by many different economists of the elasticity of demand for labor suggest numbers much below unity, which implies that Professor Rees' figures should be reduced, not raised. Moreover, recent econometric analysis suggests that the demand elasticity for unionized labor is substantially below that for the economy as a whole (as Professor Rees thought it might be), which implies a further reduction in Professor Rees' estimate. Thus, existing facts strongly suggest that the Rees guess overstates the loss of GNP due to union monopoly effects. Scientific evidence is, we hold, better than arbitrary doubling of figures to obtain a notion of the possible union monopoly impact on social welfare. Finally, we should mention, since TKS did not, that Professor Rees recognized that unions can and do in some settings have the positive effect on productivity which we have found to be less the exception than the rule. Albert Rees wrote in the *Journal of Law and Economics* (October 1963): "It should be noted, however, that direct union impact on output is not always restrictive. Under some circumstances, unions have made significant contributions to efforts to raise output or productivity, especially where jobs have been threatened by competition from new products or products produced in other locations."

The remainder of the TKS discussion of productivity, where comprehensible, is replete with errors. They quote a statement by John Pencavel that "...if management were cognisant of the productivity-augmenting effect of unionism, then we should see more cases of firms actively encouraging the unionisation of their workers," which is just plain wrong. As any businessman or woman will tell you, the firm is concerned with profits, not productivity for productivity's sake. Unionized establishments may be more productive than nonunion establishments, but if union wages are higher, profit rates in unionized firms may be lower. As we note in our article, "the bulk of the economic gains that spring from unionism accrue to workers...the rate of return on capital [is often] lower under collective bargaining." This gives one good reason for firms' failure to encourage unionism, despite a positive union productivity effect. TKS's references to productivity in agriculture (which is *lower* than productivity in manufacturing, but has *grown* more rapidly), and to the minimum wage are incomprehensible. Their statement that "most of the gains in the standard of living of American workers, historically speaking, occurred when there were few, if any, unions," is simply erroneous. Productivity and real wages grew—until the 1970s—far more rapidly after World War II when unions were at their greatest strength, than before the war. National Bureau of Economic Research figures show an annual rate of productivity growth of 2.0 percent from 1900 to 1947 compared to 3.4 percent from 1947 to 1966. The recent slackening in the growth of productivity has occurred as unionism has been weakening in the private sector in the U.S., though we do not regard that correlation as causal. These facts in productivity growth should be obvious to anyone who has bothered to check *Historical Statistics*.

If it is possible, the TKS discussion of income distribution shows even less knowledge of recent work. The "thorough" analysis by Harry G. Johnson and Peter Mieskowski is a study *not* based on careful empirical estimates, but on admittedly rough computations. It does not take account even in a crude way of the main point of our analysis: that union-induced standard rates create less income inequality within and between organized companies; and that union-caused reductions in white collar-blue collar differentials also reduce inequality. What we have done is take account of these factors *and* the standard monopoly wage gain considered by Professors Johnson and Mieskowski to get an assessment of the total effect of unionism on inequality. While TKS apparently grant the importance of the two effects that reduce inequality, they prefer to rely on the Johnson-Mieskowski guess that fails to include them in the analysis. We do not understand this at all. Belief that the union effects which increase equality are "yet another example of wage distortion," as TKS state, does not justify ignoring them in an analysis of the union impact on the income distribution.

In recent years there has been a growing body of evidence regarding the monopoly and non-monopoly effects of unions. We suggest that TKS read this literature before making any further criticisms of conclusions based on it. Contrary to the impression given by TKS, work on U.S. trade unionism is not so limited that one needs rely on two studies for the U.K. (whose union structure differs markedly from the American, despite TKS's assertion to the contrary) or on studies a decade or so ago, which were hampered by a lack of data.

Many facts about U.S. unionism exist, but they are inconsistent with TKS's fancies. Most certainly, when TKS become aware of these facts they will recognize the wide divergence. As we stated in our article, ". . . our research on the non-wage effects of trade unions is by no means complete and some results will surely change as more evidence becomes available." We enthusiastically welcome more facts about U.S. unionism, no matter what they reveal, and hope that TKS will turn from fancies to facts in their next examination of what unions do.

Richard B. Freeman and
James L. Medoff
Harvard University and
National Bureau of Economic
Research

Leo Troy, C. Timothy Koeller, and Neil Sheflin reply:

Richard Freeman and James Medoff's (FM's) response to our article "The Three Faces of Unionism," fails, we believe, to address the major issues we raised in relation to their earlier article in *The Public Interest*. Moreover, FM's response contains a number of conceptual and factual errors. More importantly, their "facts" often do not lead to or even support their new view of unionism.

As FM note, we feel that their treatment of the political aspects of unionism is inadequate. In a mixed system such as ours, government actions significantly impact the economy. Unions, with over 20 million potential member-voters, play a rather different role in this system than other economic agents. This political influence is evident in administrative and legislative actions at the state and local as well as at the federal level. Yet FM devoted a fraction of a page at the end of their article to a cursory discussion of unions' influence on the passage of federal legislation. As we suggested, the failure to examine unions' influence on economic affairs through this political channel, the "third face" of unionism, can seriously distort an assessment of organized labor.

Our most important criticism is not, as FM suggest, that they misread the evidence about unions' effects, but rather that they over-read, over-stated, and over-extended it. They reached, explicitly or implicitly, sweeping conclusions from "limited," "tentative," and "partial" evidence. Such conclusions reflect non-economic judgments as much as "fact." Further, they failed to explicitly address what they themselves termed the key question—the relative importance of their "two faces."

The specific points raised in FM's letter are rather easily answered. Their statement that we arbitrarily doubled Albert Rees' estimates of the output loss due to unionism is extraordinary since we indicated our basis for doing so, namely, Rees' statement indicating that the featherbedding losses most likely exceeded the losses from union relative-wage distortions. (Indeed the 0.3 figure they cite is itself a doubling by Rees of his original estimate). Even more remarkable is the change in their evaluation of Rees' study—a study, by the way, originally cited by *them* in support of *their* argument. Characterized as "analysis" in their paper, Rees' conclusions become "conjectures" in their letter.

As to *their* conjecture, that the output loss may be lower than Rees' estimate due to a lower labor demand elasticity than Rees used, FM ignore the possible offset due to increases in the union relative wage advantage, which appears to have grown in the 1970s (see Daniel Mitchell's "Unions, Wages, and Inflation" [Brookings, 1980] and George Johnson's unpublished "Changes Over Time in Union Non-Union Wage Differential in the United States," 1981).

The confused discussion of union productivity contained in their article is amplified in their letter. The relevant issue from a social welfare perspective is not whether "unionism may increase productivity in some settings and decrease it in others," but rather, whether the possible union-induced productivity increases offset the higher union wage, and whether *total* productivity is increased. FM's answer to the first question is apparently "no." For as the awkwardly spliced quotation from their article reads, "...the bulk of the economic gains that spring from unionism accrue to workers... (p. 91) the rate of return on capital [is often] lower under collective bargaining," (p. 92). (The "bulk" which FM refer to is apparently *all* and then some). Even so, FM characterized these "facts" as "limited" and "tentative," with "notable exceptions." FM had little to say of unions' effects on total productivity beyond citing Rees' figures indicating that it indeed must decline.

FM's discussion of income distribution, continued in their letter, virtually ignores the issues of horizontal and vertical equity in labor markets and fails to distinguish between salary *differentials* based on performance and *discrimination*. There seems little social welfare implication in their "apples and oranges" netting of the within-union income differences and union/non-union differentials. As we suggested in our article, the important issue is unions' impact on incentives and efficiency, effects they conceded are probably negative. FM contended, in their article, that "no definitive accounting" of the distributional consequences of the union wage advantage exists. It is extraordinary, therefore, that they attack our reference to the analysis of Johnson and Mieszkowski ("The Effects of Unionization on the Distribution of Income: A General Equilibrium Approach," *QJE*, November 1970) which focused exactly on this issue. FM contend that the study is "not based on careful empirical estimates," yet Johnson and Mieszkowski characterized it as "...an empirical analysis..." and they incorporated the statistical results of seven previous studies.

The remaining minor points made by FM can be dealt with briefly:

1. Our article was not intended as a survey and our citations did not reflect everything we have read in the past ten years. We certainly hope theirs didn't either. (We think that it should not have been necessary for us to write this.)

2. Their criticism of our use of "...earlier articles for the U.S. which shed little empirical light on the issues of concern..." (such as Greg Lewis' book, perhaps) is vacuous. FM felt free—quite correctly, we believe—to cite older studies (such as the 1960 book by Slichter *et al*) in support of their arguments.

3. FM's concern with our references to British industrial relations is peculiar coming from associates of the National Bureau of Economic Research. International studies have long been a hallmark of the Bureau methodology, as has been objective, factual research. Recently, Thomas Kochan recommended that the Department of Labor pay increased attention to comparative international industrial relations. Incidentally, FM found no difficulty in relating the American experience to the Japanese in support of their arguments.

4. Our parenthetical statement that some of the greatest gains in productivity occurred when unions were weak or nonexistent is, of course, supported by the data on non-farm and manufacturing productivity (*Historical Statistics*, series D683, 684). These reveal that the decade of greatest productivity growth occurred during the 1920s compared to the 1950s and 1960s. The figures are 33.9 percent for non-farm, 62.5 percent for manufacturing in the 1920s; 23.5 percent and 30.5 percent for non-farm and manufacturing productivity, respectively during the 1950s; and 33.9 percent for manufacturing during the 1960s.

As for gains in the standard of living, again referring to the manufacturing sector, data on real average hourly wages of production workers between 1900 and 1966 (Historical Statistics, series D802, spliced with Rees' N.B.E.R. series for the period 1900–1919) show that virtually half of the increase in this series occurred before 1947 (144 percent of the 290 percent gain). While this is marginally less than "most" of the gains, and we might have more clearly indicated that we were referring to the manufacturing or non-farm sectors (the sectors of concern in any discussion of unions) our point was that unions are by no means a necessary condition for gains in the standard of living or productivity.

The central issue is not a matter of facts or of fancies, for FM presented little in the way of new facts nor did we deal in fancies. The studies FM cited are limited and subject to varying interpretations (see John Addison's unpublished, "Trade Unions and Restrictive Practices," 1981, for example). Their "facts" hardly impel an objective reader to their "new view of unionism" and in many instances their evidence is contrary to such a view. Moreover, they don't contest many of the conclusions of the "monopoly view" of unionism.

In the final analysis, FM's "new view" of unionism appears to us to derive as much from subjective evaluations and premature conclusions (what one might call "fancy") as from any "facts" they present. And while certainly interesting, their presentation is outside of the realm of positive economics.

Plant Closures

Professor McKenzie ("The Case for Plant Closures," Winter 1981) contends that free mobility of corporations and workers will minimize the general societal costs of unemployment. However, reliance on free mobility alone, denies the needs and rights of individuals—such as children—who are unable to avail themselves of that freedom.

In his argument, Professor McKenzie inappropriately draws an analogy between corporate and individual responsibility to the community. As he well knows, in the "free market," countless individuals make decisions, any one of which has negligible effect on the market. In contrast, a company that closes a plant has a significant impact on the local market. Just as the closing of a plant impacts on a community, so the involuntary loss of work affects an entire family. Therefore, a more appropriate analogy would be between the company's responsibility to the community and the primary wage earner's responsibility to his family. An individual worker may indeed include the risk of ulcers, depression, or suicide in calculating whether to opt for the higher wages available in a company unencumbered by restrictions on its movement of capital. However, the mere fact of involuntary termination probably affects the children detrimentally, independent of provisions made by the worker (L. H. Margolis and D. C. Farran, "Unemployment: The Health and Behavioral Consequences for Children," *The Networker*, Fall 1980). Throughout 1980, approximately 500,000 men with children involuntarily lost their jobs.

To cite just one—but particularly cruel—cost of involuntary work loss, the children of terminated workers are at substantially increased risk of suffering physical abuse at the hands of their parents (R. J. Light, "Abused and Neglected Children in America: A Study in Policy Alternatives," *Harvard Educational Review* 43:556–598, 1973). Clearly, children are not capable of exercising "the key economic liberty of mobility." Are not these children entitled to some protection through attempting to prevent that abuse instead of the equivocally effective treatment which variably follows child abuse? What of the battered children who remain undiscovered and thus bear an invisible cost of parental work loss?

Professor McKenzie is in error in claiming that the cost of restrictions on plant openings are invisible. The children of uneducated, "trapped" workers are at increased risk of abuse and that risk is reduced as those workers ascend out of poverty through employment.

The costs to terminated workers compete with the costs of unemployed workers waiting for new plants. However, the children of neither group are able to participate in the market mechanism Professor McKenzie posits. As a result, our society should strive to decrease both of these costs.

Unfortunately, Professor McKenzie's "case" is narrowly against the National Employment Priorities Act. Surely he realizes that, as with any proposed legislation in a free society, debate and negotiation should and will follow. One would hope that the policy which emerges recognizes all of the hidden costs and attempts to distribute them in an equitable and efficient manner.

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Richard B. McKenzie replies:

Mr. Margolis attempts to make the case for restrictions on plant closings by pointing to the harm done to unemployed workers and their children. He argues that child abuse is increased by unemployment and lost income (and cites studies that back up his contention). He concludes, wrongly, that governmental restrictions on plant closings are a means of reducing the overall incidence of child abuse. A major point of my article is that in a dynamic economy, restrictions can only be fully evaluated by looking at their effects on both plant openings and closings. I stressed that restrictions on plant closings are necessarily restrictions on openings and on the growth in workers' wages and employment opportunities. Hence, to the extent that child abuse (or any other physical or social problem) increases with reduced employment opportunities and wages, restrictions on plant closings encourage child abuse. In short, restrictions on plant closings are a bad idea, even from Mr. Margolis' child-care perspective. At the very least, to make his argument convincing, Mr. Margolis must do more than merely point to all the cases of child abuse that occur when a plant closes down; he must show that the number of cases of child abuse caused by plant closings is greater than the number of cases of child abuse that will occur if plants are kept from opening (and if the growth in jobs and wages is reduced). Frankly, that is a very difficult empirical assignment. Proponents of restrictions have tended to avoid the issue from both a conceptual and empirical point of view. They have tended to present a one-sided argument and to draw one-sided (and erroneous) policy conclusions. If the solvency of banks were evaluated in a similar manner, only the withdrawals would be considered. Clearly, such a narrow focus of attention on one side of banks' balance sheets would lead to the conclusion that all banks will fail and that the entire economy will go down the drain.

ERRATUM

In *Policy Review* #17 a footnote in Kenneth Adelman's article, "Beyond MAD-ness," was inadvertently omitted. Footnote 1 should read: Colin Gray and Keith Payne, "Victory is Possible," *Foreign Policy* No. 39 (Summer 1980) pp. 14–27.

The Myth of Social Conditioning

KENNETH MINOGUE

In the vocabulary of conditioning, responses can be "triggered." This essay on the idea of conditioning is a response, and I can explain precisely what "triggered" it. At a party on Christmas Day at Hampstead in London, I came across a young man wearing around his neck a studded dog's collar. It is necessary to emphasize that I mean a collar appropriate to a dog, and not simply a clerical collar. But trading on that possible confusion, I immediately asked him whether this decoration was a witty way of proclaiming that he intended to become a clergyman. When he denied this, I suggested that possibly it proclaimed that he was a cynic, since the Greek word *kynikos* means a dog. Wrong again. Launched on the game, I remembered Alexander Pope's couplet:

I am his majesty's dog at Kew
Pray tell me, Sir, whose dog are you?

But he managed to exhaust all my ingenuity, and finally explained to me what he meant by the dog's collar. It symbolized the human condition. It was not difficult to work out what he meant, for he was expressing one of the commonest views of what human beings really are.

We imagine (so the opinion runs) that we are superior to animals, yet we are actually nothing more than a set of organismic responses to stimuli like smiles, packaging, gender codes, the connotations of words, images, and symbols of all kinds. Hence it is appropriate that Pavlov, the famous Russian scientist who developed a conditioning theory of learning, should have made his most famous experiments on dogs. For all our illusions of superiority to the animal world, our life is nothing else but a form of higher salivation. Lurking in the background of this young man's position was a revised version of the Socratic paradox: not "I am wise in that I know that I am ignorant," but rather, "I am free, or at least rather superior to the ordinary person, in that I know that I am conditioned."

One does not have to have adventured far into the realm of ideas to recognize in this position a version of one of the central beliefs of the western world in the twentieth century. It is, I think, a fundamentally religious belief, and it may be the only religion still