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COMMUNITY WITHOUT COERCION

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In the West, we are witnessing a remarkable regrouping of political forces. Marxism is seen by more and more people as a pure fantasy, irrelevant to our time and day, although it will, of course, linger on for a while in some educational establishments. But with its decline, we may perhaps be returning to the political problems that preoccupied pre-Marxian thinkers, in particular the old tension between conservatism and liberalism. There is one difference: now, those who call themselves socialists are in fact conservatives, while self-styled conservatives are, at least sometimes, liberals. In this paper, I propose accordingly to examine one or two conservative (socialist) arguments against the market order, not in terms of efficiency, but other values, which, it is alleged, market supporters cannot take into account. I will do this with special reference to Hegel, as he seems to inspire many contemporary non-Marxian critics of capitalism.

HEGEL'S ANALYSIS OF CIVIL SOCIETY

The problem with capitalism, as perceived by Hegelians, is this: if society is to be legitimate, there has to be "universality"; in other words, a sense of citizenship, of people identifying with the state. But in capitalism, or as Hegel called it, civil society,¹ there is only "particularity"; human relationships are based on self-interest, on the mutual fulfilling of needs, not on any common identity. Civil society is a society of strangers. Thus, a sense of loss, or alienation,

is created. Some members of the community do not feel as its members, they experience the community as something external and unintelligible. There is, then, a conflict between what Adam Smith called the "commercial spirit," and ethical community in which man can fulfill his role as man. It is a conflict between civil society and the state that can only be overcome by a Hegelian *Aufhebung* of civil society into the state. Translated into modern terms, this means an interventionist state, correcting the outcomes of the "blind" play of the market forces.

Hegel thought that the unhampered free market had two undesirable social consequences. In the first place, the individual was deprived of the intellectual development that was only possible within a community.² Hegel agreed, then, with Adam Ferguson and Adam Smith, that the division of labor, although on the whole beneficial, had some undesirable social consequences which, in turn, meant that the legitimacy of the liberal order was inherently questionable. Hence, modern Hegelians argue that, despite the affluence of contemporary capitalist states, they are seething with discontent. Space does not permit us to provide an adequate response to their argument. It does not, however, appear as convincing now as 10 or 15 years ago when the "New Left" was in fashion.

Let me, however, note four points. First, the problem seems to be somewhat exaggerated. The intellectual development offered to the common man in precapitalistic society was not very great. Second, if the liberal order lacks legitimacy, why do people everywhere try to move from less to more liberal countries? They go from Mexico to the United States, from East Germany to West Germany, and from China to Hong Kong; not the other way around. Third, the discontented group in our societies does not consist as much of ordinary citizens as intellectuals who cannot easily find a market for their "services." Is not the alienation they describe in such detail their own alienation? Fourth, and this is a point to which I will return later in this paper, civil society may be able to generate the identification, fellow-feeling, and social monitoring that may be necessary for its maintenance. This it may achieve through voluntary associations, different communities, churches, localities, and the like.

The other undesirable consequence of the unhampered market, according to Hegel, was that the individual became prey of blind and uncontrolled market forces in all their unpredictability and uncertainty. Overproduction forced people into poverty, turning them into "a rabble of paupers,"³ creating alienation again. As Hegel said:

This inner dialectic of civil society thus drives it—or at any rate drives a specific civil society—to push beyond its own limits and seek markets, and so its necessary means of subsistence, in other lands

which are either deficient in the goods it has overproduced, or else generally backward in industry, etc.⁴

For Hegel, as Michael Oakeshott has remarked, poverty was “the counterpart of modern wealth rather than a sign of personal inadequacy.”⁵ Hegel was well aware of the fact that poverty had existed before capitalism, and he was familiar with the classical economists’ argument that capitalism created wealth, not poverty. His thesis was rather that in the context of progressive society the existence of poverty was a social problem, whereas in precapitalistic society it might have been an individual problem. Poverty was relative rather than absolute; it was the position that the poor occupied in society. By their membership in a progressive society the poor had come to form certain expectations which were legitimate, Hegel believed, but were not fulfilled.⁶

In this paper, I shall concentrate on this argument. First, there is the idea of poverty as relative deprivation that has to be relieved by the state. Second, we have the notion that socially generated expectations are legitimate and that the state has, likewise, to step in and fulfill them. The “inner dialectic” of civil society consists then, as I understand Hegel and his followers, in its creation of needs that society is not itself able to satisfy, so that it is pushed beyond its own limits. The liberal state—the state as confined to civil society—is not enough. It is, in the Hegelian scheme, almost a contradiction in terms. Underlying the argument there is a conception of man as a being who can only capture his essence in the state, by which Hegel meant an ethical community, a community of shared ideals and ends. Man is free only insofar as he is a member of such a community, participating in its *Sittlichkeit*. As a citizen of the state, he has duties toward his fellow citizens; but he also has rights against them that transcend the contractual rights of civil society. The welfare state, with its conception of social justice, is therefore rational, indeed inescapable.

Hegel’s arguments have recently been restated by communitarian critics of liberalism. On the right, Roger Scruton, Irving Kristol, and Sir Ian Gilmour accuse Hayek and other liberals of endorsing the uncertainty which can only sever the bonds of loyalty between individual and society.⁷ Distribution of income has to have, Kristol contends for example, a meaningful moral content: otherwise it will always be seen as illegitimate. On the left, Charles Taylor and Raymond Plant argue that liberals have an impoverished notion of human beings, perceiving them as utilitarian calculators and therefore unable to provide a satisfactory theory of their loyalty to society.⁸

THE HISTORICAL ARGUMENT

Let me try to respond to the Hegelian argument on three levels:

historical, philosophical, and economic. On the historical level, liberals can question the claim that pauperization was a consequence of capitalism. In the early 1950s, a meeting of the Mont Pelerin Society was devoted to the treatment of capitalism by historians, some of the papers being published in a book in 1954, *Capitalism and the Historians*.⁹ There, the authors reach the conclusion, on the basis of their analysis of the movements of wages and prices in the 18th and 19th centuries, that there was a "slow and irregular progress of the working class" during this period.¹⁰ This conclusion has since been reinforced by the research of Max Hartwell and others.¹¹

In his introduction to *Capitalism and the Historians*, Hayek tries to explain why the opposite view came to be dominant. In the first place, he contends, there was "evidently an increasing awareness of facts which before had passed unnoticed. The very increase of wealth and well-being which had been achieved raised standards and aspirations."¹² Secondly, and more importantly, the landowning class had a vested interest in depicting the conditions in the industrial areas of the North as darkly as possible, in its political struggle with the capitalist class. Finally, most of the historians who were interested in economic history in the 19th century were sympathetic to socialism or interventionism; they had certain preconceptions and found ample evidence to support them, as all historians do who seek out such evidence.

But Hegelians can point out that this does not dispose of their thesis. They are concerned about *relative*, not absolute poverty, and about the resulting estrangement of the poor from society. They are right. The Hayekian reading of history, if correct, only serves to change some of their preconceptions, to bring some balance into the picture, but it does not show that the Hegelian worry is groundless. Should hard-working, conscientious people risk losing their jobs or at least suffering worse living standards because of a change in fashion of a technical innovation in another country? Are such people not the victims of circumstances, indeed of market forces? And, perhaps more importantly: Should whole communities that have existed for centuries be allowed to go under, lose their identity, their history, the traditions and social values that they have developed?

THE PHILOSOPHICAL ARGUMENT

This brings us to the second response, which is philosophical. It pertains to how people can come to have legitimate expectations. In his treatment of this problem in *Anarchy, State and Utopia*, Robert Nozick asserts that it depends on whether or not the fulfillment of such expectations requires the violation of the rights of other people to choose. If some people's expectations remain unfulfilled simply because other people have chosen things provided by the former, then those people have no justified complaint, their expectations

have not been legitimate. As Nozick says:

Arturo Toscanini, after conducting the New York Philharmonic Orchestra, conducted an orchestra called the Symphony of the Air. That orchestra's continued functioning in a financially lucrative way depended upon his being the conductor. If he retired, the other musicians would have to look for another job, and most of them would probably get a much less desirable one. Since Toscanini's decision as to whether to retire would affect their livelihood significantly, did all of the musicians in that orchestra have a right to a say in that decision?¹³

A possible Hegelian response to this argument is that these considerations may apply to purely contractual relationships, but that many social relationships are not contractual. Moreover, Hegelians may argue that people are interdependent and that the needs of the poor are shaped by society, or in other words partly by those who then refuse to accept the goods of the poor. In that sense, the poor are victimized. Our rejoinder must be this: first, those human relationships which are interesting from a moral point of view are voluntary. It is the joint decision of two individuals whether or not they marry; there is a joint acceptance of you by society and of society by you, otherwise you emigrate or you lose your citizenship. If resources are transferred from Norwegian taxpayers to fisherfolk in the North in order to sustain their community, then the Norwegian taxpayers have been deprived of something without their direct consent. They have lost, while the fisherfolk have gained. Second, even if it is right that people are interdependent in civil society, it does not follow that they are equally interdependent. It is precisely their market value, their price, as agreed in voluntary transactions, which reflects the dependence of others on them. If they carry a price lower than expected, it only shows that society is not as dependent upon them as they had thought. (This is not to say, however, that such people are worthless in the eyes of society, and hence totally rejected by it. Everybody can carry a price in Hegel's "system of needs," but it may be very low. Nozick has an illuminating discussion of this in his chapter in *Anarchy, State, and Utopia* on "Self-esteem and Envy."¹⁴)

It is undoubtedly true that by living in a progressive society people come to have greater needs than in a primitive society. They therefore feel deprived, even if their standard of living is better than in a primitive society. Hegelians are surely right that poverty can sometimes be relative. In modern affluent society, poverty is not as much starving as not being able to keep up with the Joneses. The answer must then be the rather Hegelian one that people must come to understand that they cannot expect the Joneses to slow down; they have to run faster themselves. Or perhaps they should choose

another competition where they will be better than the Joneses. It is a misunderstanding, moreover, that the only contest in modern society is the competition for pecuniary rewards. Modern society is pluralistic, there are many games going on simultaneously. Scholars, scientists, athletes, and artists, although usually welcoming pecuniary rewards, are not pursuing their careers only in order to obtain such rewards.

Again, Hegelians may offer some responses. They may point out that a transfer of resources from the Norwegian taxpayers to the fisherfolk is perhaps not a question of one community losing and another gaining. The Norwegian taxpayers do not constitute a community as such; they do not perceive themselves in any meaningful sense as the community of taxpayers; self-awareness is to some extent, communitarians can argue, a necessary condition of a community. The rejoinder to this argument must, I submit, focus on the relationship between a fisherman and another Norwegian within the Norwegian community. The real and independent community in this example is Norway itself. In it, all citizens are supposed to be equal. Yet, some are subsidized at the expense of others. Is this not a violation of the communitarian principle that there must be some kind of consensus behind political decisions? The whole idea of community seems to lose its attractiveness if the community is not self-sufficient or autonomous in some sense. If a part of the population becomes dependent upon another part of it for its livelihood, it soon loses its independence of mind, its self-esteem, its moral autonomy. Is the spirit of the pauper really worth conserving?

Moreover, the Hegelian argument may, if followed through, have some perverse consequences. If the "legitimate" expectations of communities are dependent, not on their absolute but their relative, standard of living, then it seems that those in the very affluent community in Beverly Hills in California are as justified in claiming subsidies to maintain their (relative) standard of living as the fisherfolk in Norway or the British miners. If they suffer a loss because the demand for their services has fallen relative to the demand for other services, for example because films have been superseded by other forms of entertainment, then they are apparently, on at least some communitarian principles, entitled to have enough resources transferred to them from others to enable them to live their usual lives.¹⁵

In *The Constitution of Liberty*, Hayek focuses on the moral arbitrariness of our membership of a community; we are usually members by chance, not choice. The demand of subsidies to communities, Hayek says,

is in curious conflict with the desire to base distribution on personal merit. There is clearly no merit in being born into a particular community, and no argument of justice can be based on the accident of a

particular individual's being born in one place rather than another. A relatively wealthy community in fact regularly confers advantages on its poorest members unknown to those born in poor communities. . . . There is no obvious reason why the joint effort of the members of any group to ensure the maintenance of law and order and to organize the provision of certain services should give the members a claim to a particular share in the wealth of this group.¹⁶

THE ECONOMIC ARGUMENT

On an economic level, the response to the Hegelian critique is that in all systems, always and anywhere, some expectations will be disappointed. And it is necessary that they are. In all economic systems there has to be a process in which people take on the tasks for which they are deemed qualified. In all systems those who make mistakes have to be made to realize this in themselves; otherwise they will not be able to correct their mistakes. Under socialism or interventionism everybody is supposedly assigned to that station in life where he can best realize his capacities. But the rulers may make mistakes as well as others, and the ruled may want to do something that has not been assigned to them. Under capitalism, on the other hand, nobody is directly assigned to any one station in life; it is left to each individual to decide and then get feedback from society in the form of a market price. If a person is a miner's son in Wales, then he chooses whether or not to become a miner himself in the light of the information available to him. If he is a fisherman in Norway, the same applies. The feedback may be positive; it may also be negative. What is essential, however, is that there should be some feedback, because otherwise individuals obtain no information about their performance.

AN INTERPRETATION OF ECONOMIC HISTORY

The main point is this: if you make a choice, you also have to understand and accept the fact that others make choices. And the real question is the following: which is, on balance, a better alternative in Hegelian terms, that is to say, less likely to create alienation; to have your station in life chosen by others in a direct manner, or to choose it yourself, thereby having to accept the similar choices of others? There is little doubt that the second alternative is less likely to create estrangement.

An aspect of the problem has, however, rather been bypassed than solved by these considerations. The problem is not that some unfulfilled expectations are illegitimate, but that some people will *feel* that their unfulfilled expectations are legitimate and turn against the free market. The problem can be put in different terms. Much more information is available to many people about their

possible losses than their gains in the market game, and hence this game will in their eyes come to lack legitimacy. People who are experiencing a diminished demand for their services know what they are losing, but they do not know what they may be gaining (for example by rapidly adapting). They are not aware of the opportunities provided by the market. The process will appear unintelligible; the market forces will seem external. This can surely explain much of modern economic history. Those who perceive themselves to be on the losing side in the market game, for example farmers, and some big companies, have combined to try to ensure their relative security from competition by legislation or other political means. Then, one intervention has made another necessary, a vicious circle has developed, and an invisible hand has led people to create an ever-increasing state. This process is, in a sense, made intelligible by Hegelian arguments. The demand by interest groups for government intervention has been an inevitable, although perhaps misconceived, reaction to the vicissitudes of market forces, simply because people have a better sense of such vicissitudes than of the benefits conferred upon them by those same market forces. Hegel's inner dialectic of civil society can be interpreted not as an apology for the welfare state but as the dialectic of excessive expectations or, in other words, as an explanation for the transformation of the liberal order into a welfare state.

THE MODERN WELFARE STATE

What is to be done? Hegel's own dilemma was that he wanted at the same time to retain civil society and to reform it. He recognized that on the one hand, the "particularity" of civil society implied freedom, variety, and individuality. On the other hand, he thought that it implied the alienation of those who were deprived by civil society of the fulfillment of needs which civil society had generated in them. This seems to be an argument for the modern welfare state, where market forces are allowed to operate, but where government "corrects" their operation by intervention. And indeed Hegel wrote:

When the masses begin to decline into poverty, (a) the burden of maintaining them at their ordinary standard of living might be directly laid on the wealthier classes, or they might receive the means of livelihood directly from other public sources of wealth (e.g. from the endowments of rich hospitals, monasteries, and other foundations).¹⁷

But Hegel was acutely aware that such a welfare state might in fact create as well as solve problems. It might be true that civil society caused the alienation of those who were not chosen by the market,

but charity, whether voluntary or involuntary, also caused alienation. As Hegel said:

In either case, however, the needy would receive subsistence directly, not by means of their work, and this would violate the principle of civil society and the feeling of individual independence and self-respect in its individual members.

Another solution, almost Keynesian, was the creation of jobs through public works. "As an alternative, they might be given subsistence indirectly through being given work." But there was a problem about that, Hegel thought:

In this event the volume of production would be increased, but the evil consists precisely in an excess of production and in the lack of a proportionate number of consumers who are themselves also producers, and thus it is simply intensified by both of the methods (a) and (b) by which it is sought to alleviate it.¹⁸

Civil society could not ensure the consumption of its production as it tended, according to Hegel, to overproduction. Hegel also mentioned that civil society might tend to extend its boundaries to what is nowadays called the "underdeveloped nations." But such kind of "imperialism" was only, of course, a temporary solution.

It seems, then, that Hegel was unable to come up with a solution to modern poverty, which, in turn, led people not to identify with the community within his own system. But a few comments are in order. In the first place, Hegel's belief that markets do not clear, his denial of Say's Law, is highly controversial.¹⁹ The concept of price is curiously absent from his analysis. Everything in the marketplace is a matter of degree. If people are willing to lower their price, they will be accepted. In other words, there is no such thing as overproduction (or, in this particular context, oversupply of labor). There is only production at a price other people are not willing to pay. There is also occasional discoordination in the economy that is ascribed by the Austrian economists to a lack of information about available opportunities. Even if the price of a good is lowered, potential buyers may not be aware of it. The task of the state should then, if we accept Hegel's premise, be to try to eliminate rigidities in the labor market and other markets and the distortion of information, and this it can only, according to Hayek and other Austrian economists, accomplish by allowing the market forces freely to operate.

In the second place, the money spent by government on public works would alternatively be spent by profit-seeking individuals. Non-Keynesian economic theory, perhaps more widely accepted today than during the last few decades, tells us that such profit-

seeking individuals are more likely to find opportunities for growth and hence for the creation of jobs than government officials. This is not primarily because they have a greater incentive, although that is certainly true, but mainly because they operate under a more efficient feedback system where mistakes are costly and eventually lead to the elimination through bankruptcy of those who persist in making them.

Thirdly, and most importantly, while a permanent rabble of paupers is created by charity, as Hegel saw, those who are rejected by the market are only rejected so long as they try to exact a price for their services deemed unreasonable by the rest of society. As soon as they lower their price, or alternatively improve their services, they are accepted again by the market. On balance, a Hegelian should prefer bankruptcies of a few businessmen, and the temporary hardship of those hit by market forces, to permanent pockets of poverty as in the slums in the Bronx and in some of the Merseyside communities where individuals may lose all sense of responsibility and do nothing but collect their weekly checks from government.²⁰ The important thing here is that the market is an adjustment process: it allows those who make mistakes to correct them; hence, it gradually eliminates alienation. Our conclusion is, then, that Hegel's economics are deeply flawed, at least from a Hayekian point of view and that the poverty problem can be solved within civil society, although a few poor people will always be with us.

THE ROLE OF AUTONOMOUS ASSOCIATES

Let us, however, turn to an interesting idea that Hegel entertained about at least a partial solution to the problem. It was by individual membership in social classes, (or estates, as Hegel called them) and corporations. By such a membership the individual could gain social identity, begin to feel at home in the world. Such classes and corporations, given freedom of entry and exit, may not be very different from the autonomous associations described by Alexis de Tocqueville in *Democracy in America* or the competing utopias described by Nozick in the last part of *Anarchy, State and Utopia*. By such a membership the individual could enjoy security from losses in the market (and, of course, forsake some gains). This Hegelian idea seems to be implemented to some extent in Japan where workers and management in big corporations form what can almost be described as an organic unity. It seems also to be manifest in some workers' cooperatives (like the Israeli kibbutz). Private insurance companies, autonomous associations, and families also fulfill some such functions. (Secret societies, such as the Freemasons, are supposed also to be informal insurance companies of some kind.)

We realize, then, with Tocqueville, that within civil society there may be means of overcoming the possible alienation and insecurity resulting from the workings of civil society. This is well understood by a left-wing Hegelian, Charles Taylor, who writes that Tocqueville "saw the immense importance to a democratic polity of vigorous constituent communities in a decentralized structure of power, while at the same time the pull of equality tended to take modern society towards uniformity, and perhaps also submission under an omnipotent government." Taylor adds that the convergence between Tocqueville and Hegel on this score "is perhaps not all that surprising in two thinkers who were deeply influenced by Montesquieu."²¹

Of course man is not only a *homo economicus*; he is also a *zoon politikon*. People are socially interdependent; they are indebted to one another. Needs, preferences, expectations, and wants are socially generated. But liberals part company with communitarian Hegelians, whether conservatives or socialists, when the latter try to impose communitarian values on individuals who do not want to step out of their social roles, to make an exit from their communities, and who are not harming anyone by doing so. From the communitarian premises it does not follow that government intervention is necessary or that the artificial creation or maintenance of communities which are no longer viable on their own are necessary. To borrow a phrase from Joseph Schumpeter: we do not need communitarianism in an oxygen tent.

It is an open question whether there are any alternatives to the possible alienation in civil society that are not worse than it. It is surely a shortcoming of some of the communitarian theories about alienation and self-expression through participation, that they do not include a viable model of politics. There, I suggest, communitarian conservatives might learn something from the neo-Hobbesian analysis of politics, pursued by the Virginia School (Public Choice) in economics.²² What is emphasized by this school of thought is that man does not change his nature by moving from a market setting to a nonmarket setting. Much follows from this apparently trivial point. It is difficult to see, for example, why we should not expect selfish behavior from bureaucrats, if we expect it from managers of private enterprises. (And if we are allowed to postulate moral constraints in nonmarket settings, why should we not also postulate them in market settings?) Recent experience of public enterprises, labor unions, and the bureaucracy does not suggest that we can be as optimistic about their public-spiritedness as some Hegelian conservatives may be.

Liberals have won the argument from efficiency. Therefore, we have to prepare for another kind of argument: the argument from identity; the argument not about what we *have* but what we *are*. In this paper, I have dealt with one or two such arguments. I am aware that I have barely scratched the surface of deep problems which

troubled thinkers like Adam Smith, Adam Ferguson, and Hegel. I also know that there are many strong arguments, besides those offered here, which classical liberals can employ. But let me by way of summing up say this: Surely we need community. Of course the market has to be grounded in a specific morality, perhaps best approached in the familiar maxim: *Honest vivere, neminem laedere, suum cuique tribuere*, that is, To live honorably, to harm no one, to allow each their own. But our community has to be a community without coercion, as Tocqueville emphasized. Our morality must be voluntarily chosen or accepted by individuals, not imposed on them. My contention is that the liberal order has the means to cope with problems generated by market forces, and that government is not the solution, but the problem.

1. Hegel's concept of civil society is much more complex and comprehensive than I make it out to be here.
2. *Hegel's Philosophy of Right*, trans. T.M. Knox (Oxford: Clarendon Press, 1942), p. 243. Raymond Plant, "Hegel on Identity and Legitimation," in *The State and Civil Society: Studies in Hegel's Political Philosophy*, ed. Z.A. Pelczynski (Cambridge: Cambridge University Press, 1984) points out that these themes are discussed at some length in Hegel's *Jenenser Realphilosophie* (pp. 229-230).
3. *Hegel's Philosophy of Right*, p. 244.
4. *Ibid.*, p. 246.
5. Michael Oakeshott, "The Character of a Modern European State," in *On Human Conduct* (Oxford: Clarendon Press, 1975), p. 305. See also Raymond Plant, "Hegel on Identity and Legitimation," in Pelczynski, *The State and Civil Society*, p. 232. *Society*, p. 232.
6. *Hegel's Philosophy of Right*, p. 230: "But the right actually present in the particular requires . . . that the securing of every single person's livelihood and welfare be treated and actualized as a right, i.e., that a particular welfare as such be so treated." See Z.A. Pelczynski, "The Hegelian Conception of the State," in ed. Z.A. Pelczynski, *Hegel's Political Philosophy: Problems and Perspectives* (Cambridge: Cambridge University Press, 1971), p. 9: "The highest type of freedom—freedom in the ethical sphere—is the guidance of one's actions by the living, actual principles of one's community, clearly understood and deliberately accepted, and in secure confidence that other community members will act in the same way." The problem is, as Hegel saw clearly, that in the marketplace we can never rest in "secure confidence" about other people's behavior.
7. Roger Scruton, *The Meaning of Conservatism* (London: Macmillan, 1984), p. 96: "A citizen's allegiance requires fixed expectations, a settled idea of his own and others' material status, and a sense that he is not the victim of uncontrollable forces that might at any moment plunge him into destitution or raise him to incomprehensible wealth." Also, Sir Ian Gilmour, *Britain Can Work* (Oxford: Martin Robertson, 1982), pp. 224-25: "A free state will not survive unless its people feel loyalty to it. . . . In the Conservative view, therefore, economic liberalism, à la Professor Hayek, because of its starkness and its failure to create a sense of community, is not a safeguard of political freedom but a threat to it." And Irving Kristol, "Capitalism, Socialism, and Nihilism," in *The Portable Conservative Reader*, ed. Russell Kirk (Harmondsworth, Eng.: Penguin Books, 1982), p. 629.
8. Plant, "Hegel on Identity and Legitimation," and Charles Taylor, *Hegel and Modern Society* (Cambridge: Cambridge University Press, 1979), pp. 112-13.

9. Friedrich Hayek, ed., *Capitalism and the Historians* (Chicago: University of Chicago Press, 1954), with an Introduction by Hayek and contributions by T.S. Ashton, Louis Hacker, W.H. Hutt, and Bertrand de Jouvenel.
10. *Ibid.*, p. 14.
11. Max Hartwell, "The Consequences of the Industrial Revolution in England for the Poor," in *The Long Debate on Poverty* (London: Institute of Economic Affairs, 1972).
12. Hayek, *Capitalism and the Historians*, p. 18.
13. Robert Nozick, *Anarchy, State and Utopia* (Oxford: Basil Blackwell, 1974), p. 269.
14. *Ibid.*, pp. 239-46.
15. I am indebted to Stephen Macedo for this example.
16. Friedrich Hayek, *The Constitution of Liberty* (London: Routledge and Kegan Paul, 1960), pp. 100-101.
17. *Hegel's Philosophy of Right*, p. 245.
18. *Ibid.*
19. W.H. Hutt, *The Theory of Idle Resources* (Indianapolis: Liberty Press, 1975). And Hayek pointed out in *The Pure Theory of Capital* (London: Routledge and Kegan Paul, 1941), that Keynes's theory implied the denial of the law of scarcity.
20. See, for example, *The Moral Hazard of Social Benefits*, by Hermione Parker (London: Institute of Economic Affairs, 1982). There have been numerous studies in America of the detrimental effects which welfare benefits have, for example, in breaking up the family (by making it more profitable for teenage girls to be able to register as single mothers than as married). This is what elementary economic analysis would have enabled us to predict.
21. Taylor, *Hegel and Modern Society*, p. 118. Also Z.A. Pelczynski, "Hegel's Political Philosophy: Its Relevance Today," in Pelczynski, *Hegel's Political Philosophy*, pp. 240-241: "Tocqueville was under the strong influence of Montesquieu. But so was Hegel, and this is one reason why he shares with Tocqueville the fundamental belief that the spiritual—that is, moral, intellectual, religious, and cultural—forces operating in a society profoundly affect its political life."
22. Gordon Tullock, *The Vote Motive* (London: Institute of Economic Affairs, 1976), and William Niskanen, *Bureaucracy: Servant or Master?* (London: Institute of Economic Affairs, 1973). The theoretical foundations of this approach are laid in James M. Buchanan and Gordon Tullock, *The Calculus of Consent*, 2d ed. (Ann Arbor, Mich.: University of Michigan Press, 1965).

ECONOMICS AND THE LIMITS OF VALUE-FREE SCIENCE

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In this paper I take issue with the statement that “as a scientist, the social scientist has no basis on which to commend one criterion for ranking, or judging, decision-rules or outcomes over another. Put another way, the social scientist is hopelessly lost as a scientific ranker of outcomes—whatever be his competence as a generator of theories or outcomes.”¹

Economists, moral, legal, and political philosophers, and others have proposed various criteria in order to arrive at some systematic ability to judge the “betterness” of one outcome or procedure over another. Many of these proposals have generated protracted controversies among the social scientists and philosophers. But it has also been said that each of these criteria “suffers from the same defect: the substitution of one criterion for another will enhance the interests of others. From this dilemma there appears to be no escape.”² Now this may well be true: there are and will be conflicts of interests. But why should this be a “dilemma?” Why should it make the social scientist qua scientist helpless in the face of the demand for a scientific ranking? How can the mere fact that someone feels his interests will be damaged if a particular criterion is adopted be decisive evidence for the lack of scientific merit of that criterion?

I shall try to defend the thesis that the scientist as such, and the economist qua scientist, is entitled to make certain value judgments concerning social relations, including systems of property rights defining the distribution of authority regarding the use of scarce

resources. My starting point will be a remark by Jacob Bronowski: "Those who think science is ethically neutral confuse the findings of science, which are, with the activity of science, which is not."³ The distinction between the findings of science and activity of science is of course an important one. But coupled to the very plausible insight that the scientific enterprise, because it is an activity, a search, a process of discovery and justification, cannot be ethically neutral or "value-free," it raises the question of whether 1) the findings of science can be value-free if the activity is not; and 2) the supposed ethics of science does not entitle or even commit the scientist qua scientist to make certain value judgments that are bound to involve him in what Max Weber liked to call "a battle of the gods."

My thesis is an answer to the second question. In defending it, it will be necessary to answer the first question also. Moreover, we need a formulation of the "doctrine of Wertfreiheit" that saves its deontological merits while avoiding its methodological imbroglions. In taking seriously the possibility of a scientific ranking, we should not open the gates for the cheap "Kathederwertungen" that made the insistence on "Wertfreiheit" such an important element in scientific education.⁴ To the extent, however, that methodology is concerned with the link between the activity of science and its functions, the thesis implies that, even if in one sense "facts" and "values" are logically independent, in another sense, it would be quite illogical to believe that a scientist may without inconsistency subscribe to any value-position whatsoever (provided only that it is itself internally consistent).

THE "EXTERNAL" MORALITY OF SCIENCE

In one respect, the fact that the activity of science is not "value-free" is obvious even to the most casual observer. Value judgments influence the choice of problems to be investigated and the choice of the methods to be employed. Animal rights propagandists and those who protest against research on human embryos are too vocal to allow scientists to remain unconscious of the value judgments that guide their daily activities. However, many of these value judgments are "external" to the scientific enterprise: a particular line of research may be denounced as immoral, even criminal, and yet, however grudgingly, be recognized as an impeccable piece of work, judged from a purely "technical" point of view. The fact that some findings were arrived at in an ethically repugnant way need not jeopardize their standing as possibly significant contributions to our knowledge.

Science as an activity is embedded in a social context. We should not take the doctrine of Wertfreiheit to mean that in the search for truth the end justifies the means. On the other hand, it will be clear that the "external" morality of science, the morals of the society

within which science is embedded, cannot and should not serve as a criterion for the scientific ranking of social outcomes—that is to say, not until it has been scientifically validated.⁵ The crucial test here must be the “internal morality of science”—the ethics and politics of “the scientific community,” at least to the extent that it is a vital and essential precondition of science itself.

THE DEONTOLOGICAL REQUIREMENT OF WERTFREIHEIT

I suppose it is safe to say that the doctrine of Wertfreiheit is usually taken to mean that the truth, however unpleasant, should be pursued—or alternatively, that falsehood, illusions, and prejudice, however comforting, should be exposed. Most scientists would probably agree that Wertfreiheit is an ideal that certainly in the study of human affairs—cultural, social, economic, and political phenomena—may be difficult to attain. But even here the most common attitude is that it is quite proper to identify the value judgments (one’s own or those of one’s fellow scientists) that may have played a role in arriving at particular results and to point out that unless the value judgments can somehow be validated, the results that depend on them are to be treated, not as a “finding of science,” but as at best a tentative conclusion, a contribution to an ongoing discussion or a possibly fruitful suggestion for further research.

In this sense, it is rather misleading to single out value judgments pertaining to ethical, political, or cultural values and ideals. The deontological requirement of value-freedom should not be taken to involve the demand that an iron curtain be erected between “science” and “ethics.” Rather, it involves the demand for complete intellectual honesty in making clear just what the status of one’s pronouncements is—“scientific truths,” hypotheses, conclusions derived from such and such premises, interpretations based on this or that evidence, meaning-postulates, etc.—and for the willingness to allow others to challenge these status-claims, whether or not they apply to ethical or political value judgments.⁶

To be sure, scientists should not reject a proposition merely or primarily on the ground that its truth would be very inconvenient or subversive from the point of view of the proponents of some metaphysical, religious, social, political, economic, or racial doctrine.⁷ And it cannot be denied that the temptation to do just that is often very great—especially when the doctrine is itself linked up with the prevailing morality of a (part of) society and so with the “external” morality of science. But then scientists should also not reject a proposition merely on the ground that its truth would be inconvenient for some prevailing scientific orthodoxy. There are fashions in science,

and there is considerable social pressure.⁸ Quite possibly the dramatic effect of insisting on *Wertfreiheit* is greatest when we have in mind the many blatant instances of people trying to turn what looks like science into a prop for some ideological or political cause. But its value may be greatest when it is used to combat the overhasty assignment of truth-values by the members of the scientific community themselves—to prevent a promising or fruitful idea from establishing itself as an unshakeable dogma.

It would perhaps be better to drop the term *Wertfreiheit* altogether, and to speak only of “freedom from prejudice.” Science represents the movement from prejudice to informed, rational judgment. This formulation does not prejudice the question of whether value judgments can or cannot embody scientific knowledge. In addition, it reminds us of the fact that scientific knowledge need not consist only of propositions of the form “We know that it is true that . . .” Such knowledge is the exception rather than the rule. Scientific knowledge consists almost entirely of propositions of the form “It is true that we do not know . . .” and “We know that it is not true . . .”

WERTFREIHEIT AND METHODOLOGY

This interpretation of the *Wertfreiheit* ideal is far removed from the Weberian version, which was based on the thesis that all value judgments are ultimately and irremediably and necessarily irrational, merely subjective prejudices.⁹ It is safe to say, however, that most scientists, while recognizing Weber as a champion of *Wertfreiheit* and remaining firmly (and justifiably) skeptical of the rhetorical argumentation of moralists and politicians, would probably refuse to make the leap into Weberian value-nihilism. And they could refuse for the good scientific reason that it would require some sort of “impossibility theorem” to justify Weber’s move from the undisputed heterogeneity of the problems of ethics and those of, say, physics, or geometry, or economics, to the conclusion that there can be no science of ethics. Weber did not supply an impossibility theorem, nor did anyone else.

Indeed, on the interpretation of *Wertfreiheit* given here, this Weberian leap itself violates the canon of *Wertfreiheit* because it denies any scientist qua scientist the right even to attempt an investigation of the validity of value judgments—and also because it is a prime example of a competent scientist using his reputation as a scientist to lend authority to a thesis that is not “scientific” at all. As Weber himself pointed out, it is not even permissible for a scientist to say that the search for knowledge and truth, the life of reason and decision based on knowledge, is objectively good, or that science is a worthwhile vocation—or even that it may be possible one day to discover the truth or validity of these judgments.¹⁰

Weber's value-skepticism, far from being "healthy," amounts to outright value-nihilism.¹¹ By denying that a scientist could eventually come up with an "objective" or rational justification of the value of science, he effectively destroyed any ground upon which to make a stand against those who claim reason is evil and knowledge to be avoided at all costs; who maintain that there is no virtue in trying to think logically, or in trying to devise critical experiments, or in striving for clarity and intellectual honesty. How can one claim that in order to judge the status of a proposition—to see whether it is certainly or only possibly true, whether it is a conclusion or a presupposition, a serious hypothesis or merely a joke—one should approach the question with the mind and attitude of a scientist, if one has already admitted that one's attachment to scientific method is just a prejudice, an irrational leap of faith, with no possibility of a rational justification?

And where does this leave the findings of science? How can one avoid the slide from healthy fact-skepticism into the abyss of fact-nihilism, if one agrees that it is just as rational to accept the findings of science as the ravings of a madman? If believing in the value of science is irrational, then so is believing in the facts of science.¹²

There can be no facts in a world without values. A "scientific fact" (*factum*) is something we have made in accordance with the art of critical judgment—it is an interpretation that derives its value entirely from the process by which we arrived at it. If science has no more value (speaking "objectively") than the fancy of a court-astrologist or the wit of the columnist of the year, then the facts as presented by the sciences cannot and should not be taken more seriously than the facts as presented by prejudice—certainly not by the Weberian scientist with his "irrational" commitment to intellectual honesty and consistency. Weber, it is true, passionately refused to make "the sacrifice of the intellect," but he had painted himself into a corner where he had to admit that the refusal could only be made passionately. Whatever his deontological intentions, Weber's doctrine of *Wertfreiheit* reaches far into the domain of methodology and turns into darkness and despair.

As Friedrich Kambartel has noted: "Those who accept (Weber's thesis that value judgments ought to be eliminated from the praxis of science because they cannot be justified), cannot even understand how mathematics can be a science."¹³ The findings of mathematics can have no transsubjective validity if the methodical norms that make it possible are denied such validity.

THE IS AND THE OUGHT

Weber may have thought the thesis that one cannot derive an "ought" from an "is" justifies his doctrine.¹⁴ It does not—no more

than the impossibility of deriving anything that might be a theorem of economics from all the theorems and reports in all of the literature on physics or chemistry, justifies the conclusion that there can be no science of economics.

In order to oppose the Weberian doctrine of *Wertfreiheit* we do not have to deny the gap between "is" and "ought"—only that we are exclusively on one side of it, viz., on the side of the "is." There is no *a priori* reason why there could not be normative principles that can be asserted with as much reason as any finding of scientific fact. On the contrary: the reason for accepting a scientific fact or proposition as true (i.e., worthy of belief) or as worthy of further consideration, depends entirely on the conformity of the cognitive practice that produced it with the cognitive ideal and the norms that constitute it. If it were not for the fact that we ought to be reasonable, it would not be unreasonable to deny that anything ought to be believed because it is "a fact."

That we ought to be reasonable is the most fundamental, the most indubitable fact of all—the fact without which nothing else can be a fact. And this fact, let it be noted, is expressed by means of a proposition that is neither a mere formal tautology nor an empirically falsifiable proposition—a characteristic it shares with such other facts as that we are rational beings or that we are purposive agents.¹⁵ There is no way in which *we* could hope to falsify such propositions, although, because we are rational, we can easily imagine another kind of rational entity, say: a god, for whom it would be possible to assert, without contradiction, that we are not rational agents, or that, if we are, we nevertheless ought not to be reasonable. Philosophers have known for a long time that the logical import of a sentence may undergo a radical change if, without modifying its grammatical structure, we change the subject from the third person to the first person. "The liar" is perhaps the most famous instance of this phenomenon.¹⁶ It should not be surprising, then, that a proposition may be a necessary truth *sub specie rationis humanae*, when it is no more than an empirically falsifiable proposition *sub specie aeternitatis*. And surely, while it may make sense for a god to measure the science human beings are capable of with the yardstick of his "scientia divina," a human scientist should not forget that, at least in epistemology and methodology, "man is the measure of all things."

We can assert, bluntly, that we ought not to be reasonable, but if we do we should not add insult to injury by spelling out the "reasons" why we ought to accept that position. We cannot reasonably deny that we ought to be reasonable: anyone who ponders, i.e., seeks an answer to, the question of whether or not we ought to be reasonable, must arrive at the conclusion that we ought.¹⁷ We cannot reasonably deny this fact, which has been known at least since Aristotle, in his *Protrepticus*, argued that the question of whether we ought to philosophize or not, logically permits only

one answer—that we ought to philosophize.¹⁸ (And let us note here that at least one economist, Ludwig von Mises, has claimed that economics, as a science of human action, should rest on the non-tautological, yet meaningful and nonfalsifiable proposition that man is a rational agent—i.e., on a fact of the same epistemological status as the one we are considering here.)¹⁹ Against Weber we must accept that there can be no fact without values and no objective or transsubjective facts without objective or transsubjective values. Science does not require a leap of faith: there can be a science of ethics and therefore also an ethics of science that is quite objective if it conforms to the normative facts as discussed by the science of ethics. Still, ethical judgments are not infallible. Although it is nonsense to say that the findings of a science are “value-free,” it makes perfectly good sense to claim that no prejudice should be allowed to survive in the development of a science of ethics. There is, then, a sense in which the doctrine of *Wertfreiheit* applies to ethics too—and, if I am right, it is the same sense in which it applies to every science. And to say that we ought not to tolerate the survival of prejudice, is but another way of saying that we ought to be reasonable. And if it is the self-imposed mission of science to effect the movement from prejudice to informed, rational judgment, then we need have no qualms about affirming the objective ethical value of the scientific enterprise.

SCIENCE AND THE POLITICS OF DIALOGUE

There can be no more fundamental truth than that we ought to be reasonable. Science is man's attempt to rise to this challenge in the field of judgment. But this means that the scientific undertaking cannot be a solitary enterprise. There is no way an individual can break out of the prison of “the evident,” no way he can even identify, let alone begin to question, his prejudices, unless he has come to understand that what is evident to him may not be evident to another and that his point of view is not the only one. Science is a dialogical undertaking: it requires that we make public what we think and try to refute what we believe we ought not to accept, and try to prove what we believe we ought to believe—it requires that we give our reasons.²⁰ But this is only part of the story. A dialogue is not just a solitary monologue, nor even a monologue delivered in front of an audience. Neither is it a debate: the aim of the participants may be to defeat one another in a debate—this may even be their only motive—but it is not victory or defeat in debates that marks the progress of science. Nor is a dialogue a collective deliberation aiming at a collective decision binding on all, to be accepted by all and questioned by none: truth is not a matter for decision.²¹

We cannot be reasonable unless we are prepared to judge only in

the light of reason—that is unless we are prepared to accept that whatever can be questioned may (and ultimately ought to) be questioned, and that there are no answers anyone ought to accept if he or she cannot understand why it would be unreasonable not to do so. A dialogue is an argumentative, not a persuasive, not a rhetorical exchange: the aim of participation is to understand others in order to make oneself understood in order to allow others the opportunity to indicate just why their understanding of one's point of view does or does not appear to them sufficient reason to share it.²²

No doubt, rational dialogical discussion is rare, even in the history of science. But that does not mean that it is wrong to say that history derives its unity from the fact that the scientific enterprise is an ongoing, open, unprejudiced, nonauthoritarian and nonpersuasive dialogue—no matter how great the interest of particular individuals in winning the debates or in having their views accepted as official or holy writ. Bronowski said it well: “The values of science derive neither from the virtues of its members, nor from the finger-wagging codes of conduct by which every profession reminds itself to be good. They have grown out of the practice of science, because they are the inescapable conditions for its practice.”²³ A not inconsiderable, though not formalized, part of a scientific education consists in learning to respect the ethics of the dialogue—to allow others to question one's most sincere convictions and to refrain from claiming too much for them unless one has answered their questions: to refrain from using rewards or punishments—promises or threats—as means for securing the agreement of others; to refuse to argue against one's better judgment; and to insist that others do likewise. But most of all: to respect the dialogical rights of others—their right to speak or not to speak, to listen or not to listen, to use their own judgment.

That we ought to respect these rights, recognized in the practice of science, follows from the fundamental norm that we ought to be reasonable—that one ought to respect rational nature, both in oneself and in others; that one ought to cultivate one's own reason and ought to allow others to do the same. This requirement of respect for the rational autonomy of every participant turns the dialogue into the primary political institution for preventing prejudice from establishing itself as an impregnable barrier against free and independent thought, and so for making science possible.²⁴

Once we have exorcized the ghost of Weberian value-nihilism with its consequent fact-nihilism, we can see why it is inconsistent for a scientist to claim transsubjective validity for the findings of science while disclaiming any such validity for the ethical and political norms that define the practice of science (and philosophy), which, to quote Kuno Lorenz, “may be understood to be the unfinished attempt, in an open dialogue to strive for an uncoerced consensus in all fields of knowledge,”²⁵ and to realize the goal of mutual

enlightenment. The scientist qua scientist, i.e., regardless of the particular discipline he or she has chosen to work in, is not only entitled, but logically committed, to uphold the ethical and political value judgments that make science possible. In fact, the scientist qua rational being cannot reasonably deny that the question, whether or not one ought to be reasonable, logically permits only one answer, the affirmative one, and so cannot but accept whatever follows from it.

It may seem strange that Plato, the undisputed master of the dialogue as understood here, failed to draw any political conclusions from it. His philosopher-kings did not engage in dialogue, and they had no place in their cities for the institution of the dialogue. Even Plato's second-best solution, as presented in *The Laws*, has no room for the institution of the dialogue: its aim is to arrest evolution (i.e., further decline and corruption) by a strict enforcement of discipline based on traditional, not-to-be-questioned laws. In his attempt to rescue the good city from the effects of sophistry and demagoguery, Plato was willing to sacrifice the Socratic dialogue as a model of human interaction, and to uphold the very nonhuman ideal of non-argumentative knowledge.

There can, however, be a human history of science only where there are no philosopher-kings; only where the principle of philosophy, i.e., the dialogue as a political institution, with its jealous regard for the right of all people to act on their own judgment, prevents all philosophers or scientists from consolidating their eminence or leadership among their followers into a legal authority that cannot tolerate dissent.

SPEECH AND ACTION

If science were a matter of revelation, it would be indifferent between any two political regimes. As it is, it is illogical for scientists not to see that qua scientists they must support that system or regime that best conforms to the dialogical requirements: a scientifically or philosophically defensible political system must be one in which science and philosophy can come into their own—not just as elitist and esoteric pursuits subject to special rules which set them apart from the rest of society, but as ethical ideals that pervade all human activities. In what other sense can “the unity of theory and practice” be a valid idea?

Because the fundamental “ought” cannot be restricted to a particular kind of activity, the requirement of reasonableness applies across-the-board to every human endeavor. It applies to action no less than speech. Human action always rests upon and involves judgment. Scientific or theoretical knowledge is not essentially or qualitatively different from “ordinary” or practical knowledge.

“The intellectual methods of science do not differ in kind from those applied by the common man in his daily mundane reasoning. The scientist . . . merely uses them more skillfully and cautiously.”²⁶ Neither science nor “our daily mundane reasoning” fare well if we do not see the continuity or do not recognize that both equally face the challenge of reasonableness.

If the ethical and political requirements of the dialogue are valid for science, then they are universally valid wherever judgment and decision based on knowledge may be involved. They derive their validity, not from any particularity of the scientific enterprise as such, but from the fundamental fact of our existence as moral (i.e., rational) beings. They cannot plausibly be restricted to the recognized “intellectual pursuits” (recognized, that is, by the self-styled “intellectuals”). The required respect for every person’s rational autonomy is founded in our rationality (proven by our ability to enter into dialogical relationships), not in any professional or class solidarity.

There is, then, a glaring inconsistency in the views of those who defend “free speech” and “the free market in ideas” but attack freedom of action and the free market in goods and services. It is true that this inconsistency has been absorbed into the very fabric of our culture, which, with its long tradition of dualism (mind vs. body, culture vs. economy, supernatural vs. nature) has succeeded very well in making the differential treatment respectable.

The inconsistency of separating speech and action is also masked by the adoption of the definition of democracy as “government by discussion.” But, as mentioned earlier, the goal of a dialogical discussion is not to arrive at a collective decision, binding on all, whether or not all have come to the conclusion that the decision is, all things considered, the right one. In a culture where there is a tradition of discussion, i.e., a dialogical tradition, people can at any time come to it as if to a stream, to refresh their minds and to gain in understanding, and leave to take their chances on their own responsibility, without having to abide by any collective decision or having to ask anybody’s permission. The “free market in ideas” is merely another name for the dialogical process and its underlying structures of rights. But we should not forget that goods and services too incorporate ideas and theories. Production, as Mises liked to say, “is not something physical, material, and external; it is a spiritual and intellectual phenomenon . . . Man produces by dint of his reason . . . : the theories and poems, the cathedrals and the symphonies, the motor-cars and the airplanes.”²⁷ Also: “thinking itself (is) an action, proceeding step by step from the less satisfactory state of insufficient cognizance to the more satisfactory state of better insight.”²⁸

If the ethics and politics of the dialogue are valid for speech, they are also valid for action. Respect for the rational autonomy of an agent is just as much a requirement of reasonableness as respect for the rational autonomy of a speaker.²⁹

ECONOMICS AND THE POLITICS OF DIALOGUE

As a scientist, then, the economist is entitled to urge the adoption of the political requirements of dialogical interaction and to rank social relationships according to that standard. Of course, in a world in which many people coexist, and which, partly because of the fact, exhibits the phenomenon of scarcity, there is no possibility of respecting another unless one can define both oneself and the other, at least in the sense of the ability to determine where the one ends and the other begins.³⁰ In verbal communication the boundary is obvious enough: people are biologically distinct entities. But in other sorts of interaction the situation is different: people use many "things" that are not part of their biological organism, and when they use them they turn them into means for the realization of their purposes—they bestow a meaning on them (grain becomes food, clay becomes building material, and so on.) But many different people could use the same "thing" as means for many different and incompatible purposes. (Does the grain become food for human beings or for someone's collection of exotic birds? Does it become "my food" or "your food"?). In order to respect others as rational agents we must know the distinction between "mine" and "thine."

Now this knowledge is already implicit in the knowledge that the other fellow is another person. One who has turned a mere "thing" into a means has produced (in the fullest sense: created) the means, because where previously there was only a thing, there now is a means, something that actually serves a human purpose. The thing has been transformed into a good by the purposive activity of some person. It is therefore his and not anybody else's and remains his as long as he has not given it away, exchanged it for something else, or abandoned it. It is his in the sense of being an embodiment of his judgment, of his capacities and designs.³¹ If we are to respect the person we ought also to respect what is his, otherwise we would deny him the right to act on his own judgment, and thereby destroy the dialogical relationship.

We can argue that, all things considered, the world would be a better place if some resources were owned (i.e., had been brought into use or otherwise produced) by different persons than the ones who did. But even if this is our conclusion, it would not entitle us to effect the "appropriate" redistribution against the will of the persons concerned. Nor does it entitle us to force them to use such resources in the way those who did not (but should have) acquire(d) ownership would have used them—supposing we somehow knew anything about this. Maybe the world would have been a better place if there were no quantum-physicists, or no Christians, etc., but under the rules of dialogue that conviction does not entitle anybody to take steps to outlaw quantum-physics, or Christianity, and so on. It does not matter whether we base our conclusion on some ethical or

religious doctrine or on consideration of “utility” or “efficiency.” The label we apply to our arguments does not justify an infringement of the basic requirements of reasonableness, which is that we respect the rational autonomy of the other, both as an agent and as a speaker.

I have not the space to do more than just suggest that the dialogical requirements can easily be transformed into a general statement of the principles of private property and uncoerced exchange, i.e., of the freedom of every individual to use his or her own means (but no one else’s) as he or she wills. This, I suggest, is the fundamental political truth.³² Of course, just as in a conversation, mere respect for the rules of the game does not guarantee the quality of lives people lead. It is just as possible to argue persuasively or rhetorically or to talk nonsense without infringing the political structure of the dialogue—which implies the right not to listen or to disengage oneself from any particular conversation—as it is to waste one’s resources, one’s life, without infringing that political structure of rights. But if this is to say that, from the point of view of an individual agent, political virtue (i.e., respect for the rational autonomy of the other) is not enough—because he still needs an ethic to make good use of, to respect, his own rational nature—then we must nevertheless insist that this political virtue may well be the only one that has transsubjective validity. The general requirement of reasonableness tells us that we ought to develop, each one of us, such a personal ethic, but not which one. It is possible that each person has to discover it for himself, and that there is no way to articulate the knowledge involved so as to turn it into an objective theory—a theory that comes with a full declaration of all its conditions of application and requisite background knowledge. The knowledge involved in such a personal ethic may be, in the truest sense of the word, personal knowledge—person-relative (and therefore not absolute or universalizable) and yet objective (because pertaining to the reality of the person and not to his possibly mistaken self-conception).

An immediate consequence of this is that within the framework of the political order based on the principle of rational autonomy, an almost infinite variety of social forms of cooperation and co-existence is possible, each with its own particular challenges and opportunities.³³ Thus we should expect to find that in some communities or societies some property rights are valued less than others and consequently are either not enforced at all or else are enforced in a lackluster manner. Similarly, conventions regarding the exact delimitation of various property rights, or regarding the interpretation of the meaning of various acts and words, may vary considerably from one time or place to another. The nearly universal institution of “marriage” is an example of the immense variety of meanings and conventions that may come into play. The test is political, not ethical in the sense of conformity to anyone’s particular

conception of the good life. It is one thing to say "If I were you," it is another to forget that that is always and necessarily a counterfactual judgment that can never be tested.

The importance of this political dimension—and of keeping it as a regulative principle above any particular doctrine (whether of personal ethics or of economic organization or of social propriety) is obviously methodological. For the requirement of reasonableness is that any doctrine, whatever claims are made for it, be considered, not as a final proposition to be disputed no further, but as a contribution to a public discussion or dialogue. As such it must be capable of being taught and so of being learned. This teaching and learning is to be distinguished from drill and indoctrination. It requires that the doctrine be presented together with methods for testing and evaluating it—which means that it must be presented in a context that leaves open the possibility of comparing it to alternatives (i.e., of experimenting with alternatives) and of rejecting it altogether.³⁴ And where these doctrines pertain to action, to ways of life, such teaching and learning must of necessity take place in an open society where the liberty of all is guaranteed by the universal respect for the autonomy of every individual, not just as a speaker, but also and perhaps primarily as an agent—for it is only in action that a speaker can prove that he means what he says, or that he has accepted what he has been taught.

ECONOMIC POLICY PRESCRIPTIONS

It appears that economists can without the least scientific impropriety advocate policy prescriptions that are intended to move the legal framework of the society in the direction of a greater respect for the rational autonomy of every human participant and the property rights that are implied by it. It follows from this that it is emphatically not the business of an economist to assume that, because the observed behavior of people is consistent with their having the preferences that define, say, a prisoners' dilemma, they are in a prisoners' dilemma type of situation; and to conclude that it is therefore unequivocally a good thing to restrict their (and other people's) property rights so as to prevent them from realizing the "non-cooperative" outcome.³⁵ Their observed behavior may be consistent with their having other preferences than those required for prisoner's dilemma types of situations. It is a methodological mistake³⁶ to make the transition from observed behavior to a well-defined underlying preference-structure when there may be any number of alternatives. Furthermore, it is deontologically improper to make that transition with little more reason than to be able to secure the production of some good (conveniently dubbed "public") without considering its costs (the goods, whether private or public, that would have been realized if the resources needed for their production had not been tied up in one particular imposed project).³⁷

There can be no justification for basing policy prescriptions that affect real men and women—on the conclusions from an argument that assumes all agents to conform to behavioral postulates of some theory that allows us to predict what the agents will do when we know the objective pay offs associated with the alternative actions. There is no point in terming a real action by a real person inefficient because it does not match the predicted action of a theoretical construct in some economist's model of the world.

Of course, an external standard has to be imposed in order to make meaningful judgments of efficiency. From a purely subjective point of view, every action is efficient (from the standpoint of the agent, at the time of action) and inefficient (for there is bound to be someone else for whom that action was not "the best" that could have been taken). Unless we once again go the route of interpersonal utility-comparison, not those performed by the agents themselves (for that would not solve the problem), but such as would be performed by a unanimous body of economists, we must be able to determine which (i.e., whose) subjective point of view shall be taken into account. In other words, we can only say that an action is efficient relative to a given property rights structure (if it does not involve the infringement of any element in that structure). But the normative import of such a statement is obviously nil unless we are in a position to rank such structures according to some objective criterion (which cannot be "efficiency").³⁸ A final remark: the political judgment I have presented does, of course, rule out a number of other political judgments. For example, political socialism does not fit the requirement of reasonableness. But that is not to say that in some circumstances some people might not quite reasonably conclude that the best use they can make of their freedom is to organize a form of association that can only be described as "socialism." Such experiments are quite legitimate as long as they are introduced in the spirit of dialogue and remain throughout "a use of one's rational autonomy," without becoming pretexts for limiting it.³⁹

1. William Stubblebine, "On Property Rights and Institutions," in ed. H.G. Manne, *The Economics of Legal Relationships: Readings in the Theory of Property Rights* (St. Paul, Minn.: West Publishing, 1975), p. 14.
2. *Ibid.* Some of the criteria mentioned are: Pareto-optimality, Kaldor-Hicks-Scitovsky compensations tests, equality of rates of substitution, preservation of competition, majority rule.
3. Jacob Bronowski, *Science and Human Values* (N.Y.: Harper & Row, 1972), p. 63s.
4. Carl Menger, *Problems of Economics and Sociology* (Urbana: University of Illinois, 1963), pp. 235-257; Joseph Schumpeter, *History of Economic Analysis* (London: Oxford University Press, 1954), p. 802; Max Weber, "Der Sinn der 'Wertfreiheit' der soziologischen und ökonomischen Wissenschaften," in Weber, *Gesammelte Aufsätze zur Wissenschaftslehre*, 4th ed. (Tübingen: J.C.B. Mohr, 1974), pp. 489-540.

5. Economists appear to have a hard time avoiding appeals to popular opinion. The following is neither atypical nor dated: "The case for drastic progression in taxation must be rested on the case against inequality—on the ethical or aesthetic judgment that the prevailing distribution of wealth and income reveals a degree (and/or kind) of inequality which is distinctly evil or unlovely." H.C. Simons, *Personal Income Taxation* (Chicago: Chicago University Press, 1938), p. 18. Cf. W.A. Weiskopf, *Alienation and Economics* (N.Y.: Dell, 1971), chap. 3; T.M. Hutchison, 'Positive' Economics and Policy Objectives (Cambridge, Mass.: Harvard University Press, 1964); Gunnar Myrdal, *The Political Element in the Development of Economic Theory* (Cambridge, Mass.: Harvard University Press, 1954); and Myrdal, *Value in Social Theory* (London: Routledge & Kegan Paul, 1958).
6. Myrdal, *The Political Element in the Development of Economic Theory*, p. viii, insists on making value judgments quite explicit. Weber, *Gesammelte Aufsätze zur Wissenschaftslehre*, p. 491, says it is "ein Gebot der Intellektuellen Rechtschaffenheit." See Weber, p. 511, for the conditions under which "Wertungsdiskussionen . . . ihren sehr erheblichen Sinn haben."
7. Weber, *Gesammelte Aufsätze zur Wissenschaftslehre*, p. 603: "Wenn jemand ein brauchbar Lehrer ist, dann ist es seine erste Aufgabe, seine Schuler unbequeme Tatsachen anerkennen zu lehren, solche meine ich, die für seine Parteimeinung unbequem sind" (from "Wissenschaft als Beruf").
8. The descriptive truth of this observation cannot be denied, nor its relevance for the history of science. See Thomas Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970), and the science of science approach generally, but it should not become a pretext—cf. Karl Popper, "The Myth of the Frame-Work," in ed. E. Freeman, *The Abdication of Philosophy: Essays in Honor of P.A. Schilpp* (LaSalle, Ill.: Open Court, 1976).
9. Weber, *Gesammelte Aufsätze zur Wissenschaftslehre*, p. 608: "Überall freilich geht diese Annahme, die ich Ihnen hier vortrage, aus von dem einen Grundsachverhalt: dass das Leben, solange es in sich selbst beruht und aus sich selbst verstanden wird, nur den ewigen Kampf jener Gotter miteinander kennt—unbildlich gesprochen: die Unvereinbarkeit und also die Unaustragbarkeit des Kampfes der letzten überhaupt möglichen Standpunkte zum Leben, die Notwendigkeit also: zwischen ihnen sich zu entscheiden."
10. *Ibid.*, p. 609.
11. L. Strauss, *Natural Right and History* (Chicago: University of Chicago Press, 1965), pp. 35-85.
12. Karl Popper, *The Logic of Scientific Discovery* (London: Hutchison, 1974), pp. 59-77; Ludwig von Mises, *Human Action: A Treatise on Economics* (Chicago: Regnery, 1966), p. 647; Mises, *Epistemological Problems of Economics* (N.Y.: New York University Press, 1981). It is now generally accepted that even simple factual descriptions are thoroughly "theory-laden."
13. F. Kambartel, "Ethik und Mathematik," in eds. F. Kambartel and J. Mittelstrass, *Zum Normativen Fundament der Wissenschaft* (Frankfurt: Athenäum Verlag, 1973), p. 129.
14. For reasons I cannot go into here, I do not accept J.R. Searle's claim to have derived an "ought" from "is." See Searle, *Speech Acts: An Essay in Philosophy of Language* (Cambridge: Cambridge University Press, 1969).
15. Frank Van Dun, *Het Fundamenteel Rechtsbeginsel: Een essay over de grondslagen van het recht* (Antwerpen: Kluwer-Rechtswetenschappen, 1983), pp. 161-175.
16. J. Mackie, *Truth, Probability and Paradox* (London: Oxford University Press, 1973), pp. 237-295; J.M. Boyle, Jr., G. Grisez, and O. Tollefsen, *Free Choice—A Self-Referential Argument* (Notre Dame, Ind.: University of Notre Dame Press, 1976), pp. 281-286.
17. Van Dun, "The Philosophy of Argument and the Logic of Common Morality," in eds. E.M. Barth and J.L. Martens, *Argumentation: Approaches to Theory Formation* (Amsterdam: John Benjamins, 1982), pp. 281-286.
18. V. Rose, ed., *Aristotelis Fragmenta*, frag. 51. Cf. W. Kneale and M. Kneale, *The Development of Logic* (London: Oxford University Press, 1971), p. 96.
19. I mention this here because it seems to me to warrant an attempt to accomplish a

unity of the *humane studies* project on a single epistemological basis.

20. Karl Popper, *The Open Society and Its Enemies* (London: Routledge & Kegan Paul, 1966), p. 217.
21. E. De Strycker, *De kunst van het gesprek. Wat waren de dialogen van Plato?* (Antwerpen: De Nederlandse Boekhandel, 1976); Kambartel, "Ethik und Mathematik"; and Platos's *Gorgias*.
22. Van Dun, *Het Fundamenteel Rechtsbeginsel*, pp. v-viii.
23. Bronowski, *Science and Human Value*, p. 60.
24. G. Calogero, *Filosofia del Dialogo* (Milano: Edizioni di Comunita, 1977); Van Dun, *Het Fundamenteel Rechtsbeginsel*, pp. 417-441.
25. Kuni Lorenz, "La Science pour la science. Bemerkungen zum umstrittenen Autonomie der Wissenschaften," in eds. Kambartel and Mittelstrass, *Zum Normativen Fundament der Wissenschaft*, p. 90.
26. Mises, *Human Action*, p. 58.
27. *Ibid.*, pp. 141ff.
28. *Ibid.*, p. 99.
29. A. Director, "The Parity of the Market Place," in Manne, *The Economics of Legal Relationships*, p. 105.
30. This section summarizes some conclusions from chapter 3 of my book, Van Dun, *Het Fundamenteel Rechtsbeginsel*.
31. Henri LePage has provided a useful overview of some of the relevant literature in *Pourquoi la Propriété* (Paris: Hachette, Pluriel, 1985).
32. It goes without saying that the principle of rational autonomy, i.e., of private property and uncoerced exchange, does not commit one to defend any particular legal title of property. It should not be presupposed nor held to be self-evident that any particular legal system has in fact developed without any violation of that principle.
33. Robert Nozick, *Anarchy, State and Utopia* (New York: Basic Books, 1964), pp. 297-334, and the very relevant qualification in David Norton, *Personal Destinies: A Philosophy of Ethical Individualism* (Princeton: Princeton University Press, 1976), note on pp. 336ff.
34. O. Schwemmer, "Grundlagen einer normativen Ethik," in eds. Kambartel and Mittelstrass, *Zum Normativen Fundament der Wissenschaft*, pp. 160ff; Friedrich Hayek, *The Constitution of Liberty* (London: Routledge & Kegan Paul, 1969), pp. 22-38, 54-70.
35. James Buchanan, "Is Economics a Science of Choice?," in ed. E. Streissler, *Roads to Freedom: Essays in Honor of F.A. Hayek* (London: Routledge & Kegan Paul, 1969), 56-62; and Van Dun, "Collective Action, Human Nature, and the Possibility of Anarchy," pp. 63-76.
36. J.C. Harsanyi, "A General Theory of Rational Behavior in Game Situations," in *Econometrica* 34 (1966), pp. 613 ff.
37. Mises, *Human Action*, pp. 658-660.
38. Cf. the discussions by M. Rizzo, Murray Rothbard, Harold Demsetz, and J.B. Egger in Rizzo, *Time, Uncertainty and Disequilibrium* (Lexington, Mass.: Lexington Books, 1979).
39. I wish to thank professors William Grampp and Leonard Liggio and the Hayek Fund for giving me the opportunity to present this paper to the "History of Economics Society Conference" at George Mason University, Fairfax, Va., in May 1985.

WHAT IS REALLY WRONG WITH MILTON FRIEDMAN'S METHODOLOGY OF ECONOMICS

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INTRODUCTION

Since its appearance in 1953 a rather extensive literature has arisen in response to Milton Friedman's article "The Methodology of Positive Economics." However, to date no consensus has emerged as to what Friedman's methodological views are. And, partly as a result of this, there is little agreement on the merits and defects of Friedman's position. My purpose here is twofold. First, I want to offer an interpretation of Friedman's methodology which, in several important respects, is different than any so far advanced. Secondly, though my sympathies are largely with Friedman's critics rather than his defenders, Friedman's position seldom receives careful, precise statement by the critics. And too often the criticism focuses on minor, peripheral issues. I hope to bring out more adequately than has been done hitherto what is really wrong with Friedman's views on the methodology of economics.

One might legitimately wonder what the point is of yet another contribution to the inconclusive literature generated by Friedman's 1953 article. The answer lies in the fact that Friedman's article, as well as the literature responding to it, attempts to deal with an issue

of the first importance for neoclassical microeconomics, which remains today the dominant approach to microeconomics in a variety of countries, including the United States. The essence of the neoclassical approach is the assumption that agents of interest to economists—households, business firms, government bureaus, and so on—are optimizers. That is, they maximize or minimize something—utility, profit, the bureau's budget, etc.—perhaps subject to constraints. The neoclassical approach manifests itself in virtually all the *specific* theories or models regularly presented in textbook treatments of microeconomic theory. These models include among their assumptions or axioms that economic agents of some type—firms in a perfectly competitive product market, a firm that is a monopsony buyer of labor, and so on—are optimizers. However, going as far back as Thorstein Veblen, a number of economists and noneconomists have criticized the neoclassical models on the basis that their assumptions, and especially the assumptions that agents of various types are optimizers, are *unrealistic*. In his 1953 article Friedman attempted to parry once and for all this type of criticism of neoclassical models.¹ Given the state of the existing literature on Friedman's article, what justifies spending further effort on it is its rather novel attempt to lay to rest the persistent criticism of the neoclassical approach to microeconomics just described.

FRIEDMAN'S METHODOLOGY OF ECONOMICS

Friedman's paper, "The Methodology of Positive Economics," begins by citing an alleged threefold distinction between positive economics, normative economics, and the art of economics. Aside from some brief remarks about normative economics at the outset, Friedman's article is entirely concerned with positive economics. Viewing positive economics as a product rather than a process or activity, we can say that positive economics is supposed to contain only so-called descriptive statements and no value judgments. Among the formulations of positive economics two especially important types are hypotheses and theories. It is with these that Friedman is particularly concerned. He says:

This paper is concerned with certain methodological problems that arise in constructing the "distinct positive science" Keynes called for—in particular, the problem of how to decide whether a suggested hypothesis or theory should be tentatively accepted as part of the "body of systemized knowledge concerning what is."²

Unfortunately, Friedman's use of the term "hypothesis" is ambiguous. Sometimes he uses it to refer to a single general statement such as "A substantial increase in the quantity of money within a relatively short period is accompanied by a substantial rise in prices."³

But at other times he uses "hypothesis" to refer to a *theory*, i.e., a whole set of statements which can be organized into a deductive system.⁴ In this paper I will use "hypothesis" to refer to a single statement, never an entire theory.

Before describing the criteria for the acceptability of economic theories and hypotheses Friedman sets out, it is necessary to discuss his conception of the process of *testing* a scientific theory or hypothesis. Friedman tells us the following about the process of testing:

Empirical evidence is vital at two different, though closely related, stages: in constructing hypotheses and in testing their validity. Full and comprehensive evidence on the phenomena to be generalized or "explained" by a hypothesis, besides its obvious value in suggesting new hypotheses, is needed to assure that a hypothesis explains what it sets out to explain—that its implications for such phenomena are not contradicted in advance by experience that has already been observed. Given that the hypothesis is consistent with the evidence at hand, its further testing involves deducing from it new facts capable of being observed but not previously known and checking these deduced facts against additional empirical evidence.⁵

Let H represent an economic theory or hypothesis. The passage quoted suggests that testing H at time t is deducing from H one or more statements—call them "evidence statements"—about observable phenomena, and then determining the truth-values of these inferred statements.⁶ In Friedman's view the truth-values of the inferred evidence statements are not known or justifiably believed prior to testing at time t. Alternatively, if one or more evidence statements are deduced from H at time t and their truth-values have not been ascertained at or before t, then Friedman counts these evidence statements as *predictions* of H.⁷ And the evidence statements involved in a test of H must in Friedman's view be predictions of H. This seems clearly implied by the last sentence of the quoted passage.

Two further matters concerning Friedman's views of testing deserve comment. If H is tested at time t and the inferred evidence statements all turn out to be true, then the test of H at t is successful; but if one or more of the evidence statements turns out on investigation to be false, the test of H at time t is unsuccessful. In Friedman's view, if H has been tested one or more times at or before t and each test has been successful, then H is *confirmed* at time t by the body of evidence statements involved in the tests.⁸ This is how Friedman uses the notion of confirmation. Note that to say that a hypothesis H is confirmed in this sense by a body of evidence statements E, is *not* to say E affords grounds or reason, though ones that are less than deductively conclusive, for thinking H is true. It is merely to say H is not *refuted* by E. Inductivists hold that the fact that a hypothesis has been tested (and always successfully) provides

good but less-than-conclusive grounds for thinking the hypothesis is true. And they often express this by saying the hypothesis is confirmed by the evidence statements involved in the tests.⁹ Friedman's use of "confirmed" should be kept distinct from this inductivist use. Friedman's notion of confirmation is much closer to Popper's notion or corroboration.¹⁰

Friedman thinks that no matter how many successful tests an economic theory or hypothesis H has had, H could still be false. That is, the fact that H is confirmed at any given time does not logically imply that H is true. Friedman commits himself to this when he says: "Observed facts are necessarily finite in number; possible hypotheses infinite. If there is one hypothesis that is consistent [sic] with the available evidence, there is always an infinite number that are."¹¹ In completely general terms, the claim Friedman is making here is that for any scientific theory or hypothesis T that is confirmed at a given time, the body of evidence statements confirming T is consistent with theories or hypotheses *other* than T, including ones incompatible with T. This of course is the widely accepted principle of the underdetermination of theory by evidence. It may also be expressed like this: for any theory T that is confirmed at any given time, the body of evidence statements confirming T does not logically imply T. Clearly Friedman's acceptance of the underdetermination principle commits him to saying the fact that an economic theory or hypothesis is confirmed does not logically imply that it is true.¹²

We can partially sum up the discussion of the last three paragraphs by saying Friedman's conception of testing is a variant of the hypothetico-deductive method of testing scientific hypotheses and theories. It is a variant I will call "simple hypothetico-deductivism." And the term "simple" is appropriate. For the view merely asserts that testing a hypothesis or theory consists in deducing one or more predictions from it, and then determining whether the predictions are true or false. The test is successful, the theory or hypothesis passes the test, if all the predictions turn out to be true; otherwise the theory fails the test. And this is all there is to testing a theory or hypothesis, whether in economics or any other nonformal science.

With Friedman's conception of testing in hand we can set out the chief epistemic rules for economics he presents.¹³ Two of them are found in the following passage:

As I shall argue at greater length below, the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted; great

confidence is attached to it if it has survived many opportunities for contradiction. Factual evidence can never "prove" a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been "confirmed" by experience.¹⁴

Again let *H* be an economic theory or hypothesis. One epistemic rule for economics Friedman proposes in the quoted passage is this:

(R1) *H* is acceptable at time *t* if *H* is confirmed at *t*.

Recall that for Friedman to say *H* is confirmed is to say *H* has been tested one or more times (in the manner prescribed by simple hypothetico-deductivism), and all the tests have been successful. So, (R1) makes the fact that *H* has been tested, and always successfully, a *sufficient* condition for the acceptability of *H*. Another epistemic rule in the quoted passage is:

(R2) *H* should be rejected at time *t* if (a) *H* has been tested on one or more occasions prior to *t* and on many of those occasions the test has been unsuccessful, or (b) at *t* the percentage of unsuccessful tests *H* has had is greater than the percentage of unsuccessful tests of some existing alternative to *H*.

(R2) makes the satisfaction of condition (a) or (b) a sufficient condition for rejectability. But condition (a) of rule (R2) is vague or imprecise inasmuch as it speaks of *many* of the tests of *H* being unsuccessful. (The word Friedman actually uses in the quoted passage is "frequently"). Out of the total number of tests *H* has had, what specific number must be unsuccessful to enable us to say that many of *H*'s tests have failed? Clearly there is no general answer. What is important about this imprecision of condition (a) of rule (R2) is that it has the result that (R2) does *not* make a *single* unsuccessful test sufficient for rejectability. Suppose at time *t* an economic theory *H* is well confirmed. Economists acting on rule (R1) accept *H*. Imagine that after time *t*, *H* is tested again but the test is unsuccessful. Rule (R2) does not require economists to now reject *H*. Should they continue to accept *H*, they will not violate (R2). For (R2) says many of *H*'s tests must be unsuccessful for *H* to be worthy of rejection. And in the situation at hand most of *H*'s tests have been successful; it is only one test that has failed. (For simplicity's sake I assume in the situation being envisaged that there is no alternative to *H* with a smaller percentage of unsuccessful tests.) In short, according to Friedman's methodology of economics, it is epistemically permissible for economists to continue to accept a theory or hypothesis in the face of a certain amount of adverse empirical evidence.

Since Friedman accepts the principle of the underdetermination of theory by evidence, he must allow that a situation can arise in which economists are confronted with two or more theories inconsistent with one another but equally confirmed. In such a situation Friedman's epistemic rule (R1) would obviously be powerless to enable economists to decide which of the theories to adopt. Friedman is aware of this and supplements rule (R1) with an additional epistemic rule to cover just the sort of situation we are envisaging. He says this: "The choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant criteria are suggested by the criteria 'simplicity' and 'fruitfulness,' themselves notions that defy completely objective specification."¹⁵ Let H_1, H_2, \dots, H_n be inconsistent or alternative hypotheses or theories. The passage quoted contains the following epistemic rule:

(R3) *If H_1, H_2, \dots, H_n are equally confirmed at time t , then the simplest and most fruitful member of the group should be accepted at t .*

Some philosophers hold that in science simplicity is relevant *before* testing. Specifically, simplicity is to be appealed to in order to decide which of a number of competing hypotheses is to be subjected to empirical test.¹⁶ This is not part of simplicity's role according to Friedman. On his rule (R3) simplicity is to be used along with fruitfulness to decide between competing theories that have already been tested and withstood the test. Friedman makes some remarks about the concepts of simplicity and fruitfulness used in (R3). What he says is very brief and sketchy and does not usefully contribute to the analysis of simplicity that philosophers of science have sought.¹⁷ Economists who adopted (R3) would in a large measure have to rely on their intuitive or preanalytic understanding of simplicity and fruitfulness in acting on (R3) in particular situations.

So far nothing has been said about the aspect of Friedman's position in "The Methodology of Positive Economics" which usually receives the most attention. It appears in the following passage:

The difficulty in the social sciences of getting new evidence for this class of phenomena and of judging its conformity with the implications of the hypothesis makes it tempting to suppose that other, more readily available, evidence is equally relevant to the validity of the hypothesis—to suppose that hypotheses have not only "implications" but "assumptions" and that the conformity of these "assumptions" to "reality" is a test of the validity of the hypothesis *different from or additional to* the test by implications. This widely held view is fundamentally wrong and productive of much mischief.¹⁸

In this passage Friedman considers the following pair of claims:

(01) *A hypothesis or theory in economics is acceptable only if its assumptions are realistic.*

(02) *The realism of the assumptions of an economic hypothesis or theory H is distinct from the truth of its predictions, i.e. the realism of the assumptions of H can be determined independently of ascertaining the truth-value of H's predictions.*

Friedman regards these two claims as mistaken and productive of much mischief—those who accept (01) and (02) constitute his opposition in “The Methodology of Positive Economics.” Friedman makes a considerable effort to show that (01) and (02) are mistaken, an effort to be examined later on. For now I want to clarify (01) and (02) after relating Friedman’s rejection of these two claims to the epistemic rule (R1) that he accepts.

The concepts of assumptions and realism in (01) and (02) need explanation. But whatever exactly the meaning of these two concepts, we can say (01) and (02) conflict with Friedman’s rule (R1). (02) implies that the fact that an economic theory H is confirmed, is compatible with the assumptions of H being unrealistic. For (02) says the truth of H’s predictions is one thing and the realism of H’s assumptions another. And on Friedman’s simple hypothetico-deductivist view of testing and confirmation, H being confirmed just consists in its predictions so far having turned out to be true. Suppose then H is confirmed but its assumptions are unrealistic. By epistemic rule (01) H is unacceptable, but by Friedman’s rule (R1) H is acceptable. In sum, Friedman’s acceptance of (R1), together with his simple hypothetico-deductivism, commits him to rejecting the conjunction of (01) and (02).

We need to clarify (01) and (02) in order to get a better idea of what Friedman takes his opposition to assert. It is convenient to first focus on the notion of assumptions used in (01) and (02). Friedman believes that one important kind of hypothesis found in economics is what I will call “as-if hypotheses.” An as-if hypothesis takes the following form: _____ as if _____.

Some economic as-if hypotheses Friedman himself cites are as follows:¹⁹

(1) Business firms behave as if the managers have as their goal maximizing profits and have the knowledge needed to reach this goal (i.e, know the relevant total revenue and total cost functions, know how to calculate marginal revenue and marginal cost, etc.).

(2) American cigarette firms did not behave during World War II as if they were perfectly competitive firms.

(3) In situations involving risk individuals choose as if they were seeking to maximize their expected utility.

All three of these statements count as as-if hypotheses. Now Friedman counts what comes after the term "as-if" in an as-if hypothesis as the assumptions of the hypothesis. This is confirmed by the following passage:

This implies that the distance traveled by a falling body in any specific time is given by the formula $s = \frac{1}{2}gt^2$, where s is the distance traveled in feet and t is the time in seconds. The application of this formula to a compact ball dropped from the roof of a building is equivalent to saying that a ball so dropped behaves as if it were falling in a vacuum. Testing this hypothesis by its assumptions presumably means measuring the actual air pressure and deciding whether it is close enough to zero.²⁰

Friedman not only applies the term "assumptions" in connection with as-if hypotheses taken singly; he also talks about the assumptions of theories. Consider this passage:

In speaking of the "crucial assumptions" of a theory, we are, I believe, trying to state the key elements of the abstract model. There are many different ways of describing the model completely—many different sets of "postulates" which both imply and are implied by the model as a whole. These are all logically equivalent: what are regarded as axioms or postulates from one point of view can be regarded as theorems from another, and conversely.²¹

Recall that an economic theory or model is a set of statements capable of being arranged into one or more deductive systems. In the quoted passage Friedman identifies the axioms of a deductive systematization of the statements in a theory as assumptions of the theory. Friedman notes in the passage that the set of statements belonging to a theory admits of different deductive systematizations. And this makes the question of whether a statement belonging to a theory is an assumption or not relative to a particular systematization of the theory. An axiom and therefore an assumption on one systematization may be a theorem and so not an assumption on a different systematization of the theory.

We now know what the term "assumptions" covers as used in (01) and (02). Unfortunately it is less clear how the notion of realism is used in these two claims. The fact is that "realistic" and its cognates are used in several different ways by Friedman. Consider the following passage:

Euclidean geometry is an abstract model, logically complete and consistent. Its entities are precisely defined—a line is not a geometrical

figure "much" longer than it is wide or deep; it is a figure whose width and depth are zero. It is obviously "unrealistic." There are no such things in "reality" as Euclidean points or lines or surfaces.²²

This passage suggests the following:²³

(D1) A statement is unrealistic if and only if it contains one or more ideal object terms; it is realistic if and only if no such terms are used in the statement.

But consider now this passage:

Truly important and significant hypotheses will be found to have "assumptions" that are wildly inaccurate descriptive representations of reality, and, in general, the more significant the theory the more unrealistic the assumptions (in this sense). The reason is simple. A hypothesis is important if it "explains" much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone. To be important, therefore a hypothesis must be descriptively false in its assumptions; it takes account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained.²⁴

This passage suggests something like the following account of "realistic" and its antonym "unrealistic"—not the term "descriptively false" gets equated with "unrealistic":

(D2) A statement is unrealistic (descriptively false) if and only if it does not afford a complete or exhaustive description of whatever it is about; a statement is realistic if and only if it does provide such a description.

(D1) and (D2) are not at all equivalent. The statement "Reno is a city in Nevada" is unrealistic in the sense of (D2). The statement omits mention of the population of Reno and numerous other features of the city. But the statement is realistic in the sense of (D1), for it contains no ideal object terms. There is still a third meaning of "realistic" and "unrealistic" in Friedman's article. Section III of the article is entitled "Can a Hypothesis Be Tested By the Realism of Its Assumptions?" In the opening paragraph of section III Friedman says: "The application of this formula to a compact ball dropped from the roof of a building is equivalent to saying that a ball so dropped behaves as if it were falling in a vacuum. Testing this hypothesis by its assumptions presumably means measuring the air pressure and deciding whether it is close enough to zero."²⁵ The term "the realism of" does not appear just before "its assumptions"

in the second sentence of this passage. But given the title of section III, we could insert the term without altering the meaning of the sentence. So, the passage quoted in effect says that determining the realism of the assumption "it (the ball) is falling in a vacuum" consists in finding out whether the air pressure is close to zero, that is, in finding whether the assumption is true or approximately true. The following account of the realism of a statement is suggested by all this:

(D3) A statement is unrealistic if and only if it is neither true nor approximately true; it is realistic if and only if it is true or approximately true.

It should be clear that (D3) does not determine the same concept of realism as does (D1) or (D2).

Which of the three uses of the concept of realism described above is employed in (01) and (02)? Friedman wishes to deny (01) and (02) in all three uses of the concept of realism. But only if the notion of realism in (01) and (02) is interpreted in the light of (D3) is Friedman's denial of the two claims of any interest. The first passage quoted in the preceding paragraph, together with the context from which it is drawn, indicates that Friedman believes (01) is false if the term "unrealistic" occurring in it is used in the sense of (D1). But this belief of Friedman's is not all controversial. It is generally admitted that scientific theories which contain ideal object terms may be acceptable. And (01) interpreted in light of (D1) quite unreasonably requires that economic theories lack such terms if they are to be acceptable. The second of the passages quoted in the previous paragraph shows that Friedman wishes to deny (01) if "unrealistic" is interpreted in the sense of (D2). But this too is hardly an interesting move on Friedman's part. (01) is patently false if it affirms that assumptions of hypotheses and theories in economics must afford an exhaustive description of what they are about in order to be acceptable. In section III of "The Methodology of Positive Economics" Friedman argues against (01) and (02). And his line of argument is directed at these two claims when the concept of realism is used in the sense of (D3). So Friedman wishes to reject (01) and (02) when the concept of realism in the two claims is interpreted in the sense of (D3). In this meaning of the notion of realism it is by no means obvious that Friedman is right in denying (01) and (02). Indeed, some of those who have discussed Friedman's methodological views have identified as his major error the denial of (01) when "realism" is taken in the sense of (D3).²⁶ It is when the concept of realism is used in the sense of (D3) in claims (01) and (02) that Friedman's denial of these claims is interesting and controversial. Accordingly I propose to focus on Friedman's rejection of (01) and (02) when "realistic" in (01) and "realism" in (02) mean "true or approximately true" and "truth or approximate truth" respectively.

It is worthwhile indicating how Friedman's methodological views

as I have interpreted them enable him to answer the charge that neoclassical microeconomic models incorporate unrealistic assumptions. But before doing so it might be useful to summarize the main features of Friedman's position. The chief epistemic rules of Friedman's methodology of economics are (R1), (R2), and (R3). Each of these rules presupposes simple hypothetico-deductivism. The notions of test and confirmation are used in the formulation of the three rules, and these two concepts derive their sense from their relation to Friedman's variant of the hypothetico-deductive method. In addition, Friedman rejects the methodological position represented by (01) and (02). That is, Friedman denies that it is necessary for the acceptability of an economic theory or hypothesis that it have true or approximately true assumptions; moreover, he affirms that the *only* way to determine the truth or approximate truth of the assumptions of a theory or hypothesis is by ascertaining the truth-value of its predictions.

As indicated at the outset of this article, one of Friedman's main motives in his "The Methodology of Positive Economics" is to rebut the criticism that neoclassical microeconomics is unrealistic. Let us single out a particular example of this type of criticism of neoclassical theory, an example Friedman himself discusses. One important branch of neoclassical microeconomics is concerned with the behavior of business firms in hiring factors of production and the pricing of those factors. This branch of microeconomics is so-called marginal productivity theory or for short MPT. In 1946 Richard Lester published a paper criticizing MPT. Specific models in MPT—such as the model of the hiring policy of a firm in a competitive labor market—characteristically include as an assumption that a firm hires a quantity of a factor such as labor that maximizes firm profits. Lester attempted to challenge this, as well as certain other asserted statements of MPT, appealing to the results of a questionnaire he sent to 58 firms in the southern part of the United States. The managers of the firms responded by saying, among other things, that profits were not particularly important in their decisions about the quantity of labor they hired.²⁷ The implication of course is that the profit-maximizing assumption of specific models in MPT is unrealistic. Friedman's response to this criticism is that the assumption of profit maximizing does *not* imply anything about what firm managers *say* about their goals or other considerations entering into their hiring decisions.²⁸ In other words, Friedman claims that to say (1) firm managers will respond to Lester-type questionnaires by saying that their firms hire a quantity of labor that maximizes firm profits, is *not a prediction* of the specific models in MPT.²⁹

How does this rebut the charge that the profit-maximizing assumption of MPT is unrealistic? Recall that on my interpretation Friedman holds that the realism of the assumptions of a theory cannot be determined except by ascertaining the truth-value of predictions of the theory. But this is just what Lester is trying to do. He is

claiming that the profit-maximizing assumption of MPT is unrealistic on the basis that (1) above turned out to be false (as indicated by his questionnaire), when (1) is not a prediction of MPT at all. The way I have represented Friedman as answering Lester's criticism of MPT exemplifies the general pattern of Friedman's responses to charges that this or that assumption of neoclassical theory is unrealistic. Charges of this type are typically backed up by claiming some statement S other than the neoclassical assumption being challenged but allegedly bearing on the truth-value of the assumption, does not fit the observable phenomena. Friedman responds by saying statement S is not a prediction or implication of neoclassical theory at all. Given his view that realism of assumptions of a theory can only be determined by ascertaining the truth-value of predictions of the theory, the charge of lack of realism of the assumption in question collapses.

FRIEDMAN AND INSTRUMENTALISM

A persistent theme in the literature on Friedman's methodological views is that he is an instrumentalist. I want to discuss three instrumentalist interpretations of Friedman. Two of them seem to me to be incorrect accounts of Friedman's position. And the other taken on its own presents a rather incomplete picture of Friedman's methodological views.

Those who regard Friedman as an instrumentalist do not attach the same meaning to "instrumentalist." Stanley Wong takes instrumentalism to be the view that scientific theories are not true or false descriptions of the real world, but just instruments for generating predictions about observable phenomena. Wong ascribes instrumentalism in this sense to Friedman. Wong says this:

Instrumentalism is the thesis that theory in science is merely an instrument for prediction of observable reality. Accordingly, a theory cannot properly be called true or false.

That Friedman is an instrumentalist is quite evident. The apparent ambiguities and inconsistencies in his essay can best be sorted out by considering his view as instrumentalism.³⁰

Wong is mistaken in attributing to Friedman instrumentalism in his sense. Wong does not direct our attention to a single passage in Friedman's writings in which he says or implies that economic theories and hypotheses lack a truth-value. To be sure, Friedman does say that the goal or aim of the construction of theories and hypotheses in nonformal sciences like economics is the generation of true predictions.³¹ But this claim does not logically imply that theories and hypotheses in nonformal sciences are neither true or

false. There is no inconsistency in identifying prediction as the goal of theory construction and allowing that theories are true or false. Finally, Friedman often talks in a manner strongly suggesting that he takes economic hypotheses and theories to have a truth-value. For instance, he speaks about the confidence we may place in existing theories and hypotheses in economics.³² And there is no reason to think that by "confidence we may place in" anything else is meant than "confidence we may place in the *truth of*."

In a fairly recent paper Lawrence Boland interprets Friedman as an instrumentalist. He says:

"Instrumentalists," such as Friedman, are only concerned with the usefulness of the conclusions derived from any theory. Unlike conventionalists, instrumentalists may allow that theories or assumptions can be true but argue that it does not matter with regard to the usefulness of the conclusions.

So long as a theory does its intended job, there is no apparent need to argue in its favor (or in favor of any of its constituent parts). For some policy-oriented economists, the intended job is the generation of true or successful predictions. In this case a theory's predictive success is always a sufficient argument in its favor. This view of the *role* of theories is called "instrumentalism." It says that theories are convenient and useful ways of (logically) generating what have turned out to be true (or successful) predictions or conclusions. Instrumentalism is the primary methodological point of view expressed in Friedman's essay.

For Friedman, an instrumentalist, hypotheses are chosen because they are successful in yielding true predictions.³³

The instrumentalism Boland attributes to Friedman in these passages is different than Wong's instrumentalism. Unlike Wong's, Boland's instrumentalism allows that theories may be true. Boland's instrumentalism seems to consist of the following claims:

- (a) The sole purpose of having theories and hypotheses in economics (or any nonformal science) is the generation of true predictions;
- (b) The truth-value of a theory or hypothesis (and any components like assumptions) does not matter for the question of whether the theory or hypothesis generates true predictions;
- (c) A theory or hypothesis should be chosen or accepted if all its predictions have so far turned out to be true.

Friedman does say the goal of theory construction in economics is to generate true predictions. So I readily grant part (a) of Boland's instrumentalism is attributable to Friedman. Part (b) apparently

means that a false theory can generate the same true predictions as a true theory can.³⁴ This would seem to be guaranteed by the principle of the underdetermination of theory by evidence which, as I have indicated in this paper, is accepted by Friedman. Let T_1 be a *true* theory confirmed at time t_1 . By the underdetermination principle there is a theory T_2 compatible with T_1 and therefore false, but consistent with the same empirical evidence that confirms T_1 . Thus at any time *to prior* to t_1 , theory T_2 could have been used to generate any of the true predictions yielded by T_1 between t_0 and t_1 . As for part (c) of Boland's instrumentalism, it obviously resembles epistemic rule (R1) which I have attributed to Friedman. In sum, I have little quarrel with Boland's ascription of his instrumentalism to Friedman.

However, it is worth briefly comparing Boland's instrumentalism with the position attributed to Friedman earlier in this article. The two differ in significant respects. Epistemic rules (R2) and (R3) are part of the methodology of economics I ascribed to Friedman, but neither is included in Boland's instrumentalism. Elsewhere Boland does acknowledge that Friedman advocates appealing to simplicity and fruitfulness to decide between competing theories equally compatible with available evidence, though apparently Boland nowhere attributes rule (R2) to Friedman.³⁵ Also, the simple hypothetico-deductivist view of testing scientific theories is not a separately identifiable element of Boland's instrumentalism, but it is a key part of the position ascribed to Friedman in this paper. Finally Boland's instrumentalism does not clearly and explicitly include the denial of (O1) and the denial of (O2), (O1) and (O2) being the pair of claims affirmed by Friedman's self-chosen opponents in his 1953 article. In sum, Boland's instrumentalism is a rather incomplete account of Friedman's methodological views.

The last instrumentalist interpretation of Friedman that I wish to consider is Daniel Hausman's. Hausman says this:

Milton Friedman, in contrast to the above two defenders of microeconomics, concedes that microeconomic general statements are false, or inapplicable because they contain antecedents that are not true of any real economic situation, at least that is how I understand his view of them as "unrealistic assumptions." He denies that their falsity matters. If the theory is well-confirmed (is a good "predictor") in the class of cases in which economists are interested, it is a good theory; otherwise not. Even assertions as abruptly counterfactual as the attribution of consciousness to tree leaves are perfectly acceptable in theories of leaf distribution. All that matters is how successfully leaf distribution is "predicted."

Friedman's position seems to be a special sort of instrumentalism—which must be distinguished from the kind, discussed above, that Machlup has on occasion espoused. Friedman does *not* deny that theoretical statements have truth-values. In fact the distinction be-

tween theoretical and observational terms is of no importance to Friedman. What he denies is that the truth values of any statements matter if the statements do not result in incorrect predictions concerning the phenomena of interest to us.³⁶

The instrumentalism Hausman here ascribes to Friedman would seem to consist of the following claims:

- (d) A theory or hypothesis in economics is acceptable or good if and only if it generates true predictions which are of interest to economists;
- (e) Predictions generated by a theory or hypothesis but of no interest to economists are irrelevant to its appraisal.

There is similarity between (d) and (e) and rules (R1) and (R2) which I have attributed to Friedman. Let H be an economic theory well confirmed at a given time. Later, one prediction of H turns out to be false. But imagine this prediction is of no interest to economists—perhaps the prediction concerns what firm managers say about their goals in deciding on a level of output for the firm and economists are only interested in the nonverbal behavior of firm managers. In this situation Hausman's (d) and (e) have the result that H continues to be a good theory or acceptable despite the fact that it has generated a false prediction. A similar claim can be made for epistemic rules (R1) and (R2). As indicated in the previous section, in the type of situation being envisaged here (R1) and (R2) make it epistemically permissible for economists to continue to accept H.

However, Hausman's (d) and (e) do not represent Friedman's views in an entirely accurate way. There is little or no textual evidence for saying Friedman relies on a distinction between predictions of interest to economists and predictions of no economic interest.³⁷ Certainly in rebutting charges that neoclassical microeconomics incorporates this or that unrealistic assumption, Friedman does *not* allow that neoclassical theory generates *false* predictions but claims that these predictions are of no interest to economists. As indicated earlier, Friedman counters the charge of lack of realism by saying the false statements allegedly showing this or that neoclassical assumption is unrealistic are *not* predictions or implications of neoclassical theory at all, and therefore are irrelevant to the issue of the realism of its assumptions.

THE DEFECTS OF FRIEDMAN'S METHODOLOGY OF ECONOMICS

The core of Friedman's methodology is his epistemic rules (R1), (R2), and (R3). However, Friedman is not much concerned with

(R3). After the rule is stated in "The Methodology of Positive Economics" it pretty much drops out of the picture. He makes no effort to argue for its adoption by economists.³⁸ But Friedman does argue for the adoption of epistemic rules (R1) and (R2), albeit in an indirect fashion. I will examine Friedman's case for these two rules. Doing so will prove a convenient way to bring out one of the chief defects in Friedman's position.

Friedman argues strenuously for the incorrectness of (01) and (02), the twin claims of his opponents on methodological matters. And Friedman apparently thinks that the only alternative to (01) and (02) is acceptance of his own rules (R1) and (R2). In support of attributing this belief to Friedman, consider the following passage: "As I shall argue at greater length below, the only relevant test of the *validity* of a hypothesis is comparison of its predictions with experience. The hypothesis is rejected if its predictions are contradicted ("frequently" or more often than predictions from an alternative hypothesis); it is accepted if its predictions are not contradicted. . . ."³⁹ The reader will recognize the second sentence of this passage as Friedman's formulation of his epistemic rules (R1) and (R2). The first sentence promises to argue later on and at length for these two rules. It would appear that Friedman fulfills this promise in section III of "The Methodology of Positive Economics." In fact what we find in section III is not any direct argument for (R1) and (R2), but instead a case against (01) and (02). All this suggests that Friedman thinks that he can secure our assent to (R1) and (R2) by disposing of (01) and (02). For ease of reference later on it will be convenient to state Friedman's argument for (R1) and (R2) in the following fashion:

(P1) *Either (R1) and (R2) are correct, or (01) and (02) are correct.*

(P2) *It is false that (01) and (02) are correct.
Therefore (C) rules (R1) and (R2) are correct.*

Premise (P1) expresses Friedman's belief that his own position and that of his opponents who adopt (01) and (02) exhaust the alternatives worthy of serious consideration. Admittedly premise (P1) is not an explicit premise of the line of argument in "The Methodology of Positive Economics." But Friedman must tacitly rely on some such premise as (P1). Premise (P2) alone does not logically imply the above conclusion (C). But (P2) conjoined with (P1) does validly yield (C).

Though Friedman argues energetically for premise (P2) of the above argument for his rules (R1) and (R2), he offers no reason whatsoever for thinking premise (P1) is true. As was said earlier, the conjunction of (01) and (02) is indeed incompatible with Friedman's rule (R1), and therefore incompatible with the conjunction of (R1) and (R2). But this does not mean premise (1) of Friedman's

argument is true. It only means we cannot accept the conjunction of (01) and (02) and *also* regard (R1) and (R2) as correct. Or alternatively, the disjuncts of premise (P1) are merely contraries, they could both be false. Thus there is nothing in the logical relations of the disjuncts of premise (P1) which justifies saying (P1) is true.

The preceding paragraph indicated that we need not accept premise (P1) of Friedman's case for (R1) and (R2). There is no epistemic obligation to accept (P1). But a stronger conclusion than this is in order. It would be unreasonable to accept premise (P1). Let us suppose (01) and (02) are incorrect or unacceptable. Still we should not accept Friedman's rules (R1) and (R2). The reason for this is as follows. Recall that the simple hypothetico-deductivist view of testing scientific theories—for short, the simple H-D view of testing—is presupposed by Friedman's epistemic rules (R1) and (R2). Rule (R1) affirms that an economic theory or hypothesis H is acceptable if it has passed one or more tests as described by the simple H-D view and failed none. Rule (R2) asserts that H is unacceptable if H has often failed tests of the sort the simple H-D view describes. But as will be argued in the next three paragraphs, the simple H-D view of testing theories and hypotheses—whether in economics or any other nonformal science—is seriously flawed. Thus, even if we reject claims (01) and (02) as incorrect, it would still be unreasonable to accept Friedman's rules (R1) and (R2). Rejecting (01) and (02) does not negate the fact that (R1) and (R2) presuppose the erroneous simple H-D view. In sum, premise (P1) of Friedman's case for his rules (R1) and (R2) is unacceptable. If, rejecting (01) and (02), it is still unreasonable to accept (R1) and (R2), then premise (P1) must itself be unreasonable.

In showing that the simple H-D view of testing is flawed, what I will say should be familiar to philosophers of science. But it is important to indicate to philosophically minded economists and other social scientists that the simple H-D view cannot be maintained. What is perhaps the chief difficulty with the simple H-D view is best conveyed by an example.⁴⁰ Consider the following pair of statements:

(1) *Changes in the price of a stock selling on the New York Stock Exchange (NYSE) are statistically independent of one another, i.e. there is zero correlation between a change in the price of a stock at time t and a change in the price of a stock at time $t + 1$ ($t + 1$ could be $t + 1$ day or $t + 1$ week, etc.).*

(2) *There are invisible and otherwise undetectable leprechauns present on the floor of the NYSE during trading.*

Statement (1) is a well-attested hypothesis in the study of financial markets often called "the random-walk hypothesis." Statement (2) has been invented to make a philosophical point. Now the following

prediction can be deduced from the random-walk hypothesis and suitable auxiliary statements:

(3) *During March 1990 a change in the price of Procter and Gamble stock on any given day will be statistically independent of the change in its price one day later.*

Suppose in March 1990 we observe changes in the price of Procter and Gamble stock on successive days. And perhaps by plotting our observations on a scatter diagram, we discover that the prediction (3) is true. By the simple H-D view of testing, the random-walk hypothesis has had a successful test. But note that since statement (1) above implies the prediction (3), the conjunction "(1) and (2)" has also had a successful test according to the simple H-D view. The prediction (3) is deducible from the conjunction of (1) and (2), and the prediction has turned out to be true in the situation we are envisaging. This constitutes a successful test according to the simple H-D view.⁴¹ However, something has surely gone awry here. Presumably we want to say that a successful test of a theory or hypothesis H positively affects or increases the credibility or worth of H, or at least it does so in the absence of any previous unsuccessful tests or disconfirmations of H.⁴² But I do not think we wish to affirm that the credibility of the conjunction of statements (1) and (2) has increased should the implied prediction (3) turn out to be true. If we were to affirm this, we should have to say the existence of leprechauns is more credible after we discover (3) is true than it was before. For the existence of leprechauns is logically implied by the conjunction of (1) and (2), and presumably an increase in the credibility or reasonableness of a statement spells a rise in the credibility of its logical consequences. As the example involving statements (1), (2), and (3) indicates, a major difficulty with the simple H-D view is that it sunders the connection between successful testing and an increase in the worth or credibility of the theory or hypothesis tested. The simple H-D view counts hypotheses and theories as successfully tested whose credibility has not increased at all on account of the test.

The criticism of the simple H-D view in the previous paragraph should not be seen as an objection that can easily be met by some minor adjustment to the simple H-D view. In support of this I will discuss two minor adjustments to the simple H-D view which represent prima facie plausible attempts to avoid the criticism in the previous paragraph.⁴³ According to the simple H-D view, testing a theory or hypothesis H is deducing a prediction P from H and then determining the truth-value of P; the test is successful if P turns out to be true, and unsuccessful should P turn out to be false. Let us try to supplement Friedman's simple H-D view with the following condition:

(C1) *If a prediction P of a hypothesis or theory H turns out to be true, then the test of H is still not successful as long as H is a conjunction "A and B" such that P is a prediction of A itself, i.e., P can be generated from A without relying on B.*

Now if (C1) were tacked onto the simple H-D view, the resulting view of testing would not be open to the criticism presented in the preceding paragraph. For the random walk hypothesis, statement (1) of the preceding paragraph, generates the prediction I labeled (3) without relying on the leprechauns hypothesis (2). Thus, by condition (C1), discovering in March 1990 that (3) is indeed true would *not* constitute a successful test of the conjunction of statements (1) and (2). However, condition (C1) is not satisfactory. It is much too strong or restrictive, ruling out as successful tests which in fact are successful. Reasoning from Newton's theory and suitable auxiliary statements, the English astronomer Edmund Halley made the following prediction:⁴⁴

(P1) *The great comet of 1682 will be visible from the Earth in December 1758.*

(P1) turned out to be true and Newton's theory received a successful test. The credibility of Newton's theory was significantly increased in the minds of the members of the European intellectual community of the day. Now (P1) was implied by the conjunction:

(4) *Newton's theory, and comets are on occasion visible from the Earth from 1750 on.*

The term "Newton's theory" in (4) abbreviates the set of statements comprising Newton's theory. (P1) turning out to be true in 1758 could rightly have been regarded as a successful or favorable test of statement (4). It is hard to see how this could be denied given that (P1) turning out to be true constituted a successful test of the left conjunct of (4) and logically implies the right conjunct. However, condition (C1) above would *exclude* the truth of (P1) from constituting a successful test of (4). For (4) is a conjunction whose left conjunct—to wit, Newton's theory—is capable of generating prediction (P1) *without* relying on the right conjunct of (4). Thus condition (C1) is erroneous. Adding it to Friedman's simple H-D view of testing yields an account of testing which illegitimately narrows the class of successful tests.

A second attempt to make a relatively minor adjustment to the simple H-D view would supplement it with the following condition:

(C2) *If a prediction P of a hypothesis or theory H turns out to be true and H is a conjunction "A and B", then the test is still not successful unless P is relevant to both A and B taken separately.*

