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No. 11

SPRING 1986

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REASON PAPERS is published by the Reason Foundation, 1018 Garden Street, Santa Barbara, CA 93101. Send orders (\$5.00 per copy in US and Canada, \$6.00 elsewhere) to Reason Papers, Box 40105, Santa Barbara, CA 93140. Manuscripts should be accompanied by return postage and envelope. Copyright © 1986 by the Reason Foundation. All rights reserved.

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. 112-113.

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 imbroglis. 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 coexistence 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.*

It might be claimed that prediction (3) above is not relevant to, has no bearing on, the leprechaun hypothesis (2). Thus, by condition (C2), discovering the truth of prediction (3) in March 1990 does afford a successful test of the conjunction of the random-walk hypothesis and the leprechaun hypothesis. However, adding (C2) to Friedman's simple H-D view would be of little use. The view of testing resulting from such an addition is subject to the same type of criticism I urged against the simple H-D view posited above. Let us conjoin the following statement with the leprechaun hypothesis (2):

(5) If leprechauns are on the floor of a stock exchange during trading, then they arrange it so that changes in the price of a given stock are statistically independent of one another.

The conjunction of (5) and the leprechauns hypothesis (2) implies the prediction (3) during March 1990 a change in the price of Procter & Gamble stock on a given day will be statistically independent of a change in its price a day later. Now consider the conjunction whose left conjunct is statement (1), i.e., the random-walk hypothesis, and whose right conjunct is the conjunction of (5) and (2). This statement, which can be written '(1) and [(5) and (2)]', also implies the prediction (3), as each of its two conjuncts separately implies (3). Now imagine that in March 1990 we discover that the prediction (3) is indeed true. According to the simple H-D view, '(1) and [(5) and (2)]' has had a successful test. For an implied prediction has been verified. Can we invoke condition (C2) above to *deny* that '(1) and [(5) and (2)]' has had a successful test? I do not see how. The prediction (3) would seem to be relevant to *both* conjuncts of '(1) and [(5) and (2)]'. The conjunction of (5) and (2) implies the prediction (3) just as (1) does. Thus, adding (C2) to the simple H-D view taken on its own, commits us to regarding certain hypotheses as successfully tested whose worth has *not* been raised by the test. I am certainly not prepared to say the credibility of '(1) and [(5) and (2)]' is increased by the prediction (3) turning out to be true in March 1990.

In the second paragraph of this section Friedman's own argument for his epistemic rules (R1) and (R2) was set out. We can now see that the argument breaks down because its premise (P1) is unacceptable. But the line of argument employed to show (P1) is wrong at the same time shows Friedman's rules (R1) and (R2) are themselves incorrect. Again (R1) and (R2) both presuppose Friedman's simple H-D view of testing scientific theories. And as indicated at some length above, the simple H-D view is erroneous. Moreover, the problems with the simple H-D view also spell trouble for Friedman's epistemic rule (R3). (R3) asserts that if a number of alternative or competing theories are equally confirmed, then the simplest and most fruitful should be chosen. I certainly have no quar-

rel with saying that simplicity and fruitfulness may be used to decide between competing theories when both have withstood testing equally well. Thus in part Friedman's rule is acceptable. But (R3) is formulated using Friedman's notion of confirmation which is defined in terms of the erroneous simple H-D view. To the extent (R3) presupposes the simple H-D view of testing it is objectionable. In sum, the three epistemic rules of Friedman's methodology of economics are all defective. And what makes them defective is that they presuppose the mistaken simple H-D view of testing. This is a major part of what is really wrong with Friedman's position in his 1953 article.

The remainder of this section will be devoted to examining Friedman's criticism of (01) and (02), the claims of his opponents on methodological matters. In rejecting these two claims Friedman commits himself to a view of the role of the realism of assumptions in appraising economic theories and hypotheses that has made him the object of a good deal of criticism. In my view Friedman is correct in regarding (01) as mistaken, though there is an important element of truth in (01) which Friedman overlooks. But Friedman is wrong to reject (02) as it is true.

By way of supporting this assessment of Friedman's attitude toward (01) and (02), let us examine Friedman's effort to dispose of (02). What Friedman does is argue, by means of an example drawn from the physical sciences, that *in general* the realism (truth or approximate truth) of assumptions of a theory or hypothesis can be determined *only* by ascertaining the correctness of its predictions. This is of course inconsistent with the claim (02) makes about the realism of the assumptions of *economic* theories and hypotheses. My statement of Friedman's argument in the next paragraph will make reference to the following piece of reasoning—call it "argument A".⁴⁵

(1) *A compact ball dropped from the roof of a certain building will behave as if it is falling in a vacuum.*

(2) *The distance traveled by a body falling to the Earth in a vacuum is given by the formula $s = 16t^2$, with s = the distance traveled in feet and t = the time in seconds.*

(3) *The distance the ball will travel from the top of the building to the ground is 256 feet.*

Therefore, (4) the time the ball will take to travel from the top of the building to the ground is approximately four seconds.

Imagine statement (1) here is an as-if hypothesis the realism of whose assumptions we wish to investigate. The assumptions of the hypothesis are of course the statement "it (the ball) is falling in a vacuum." Statement (2) is the application of the law of freely falling

bodies to the Earth. Statement (3) is verified by measuring the distance from the top of the building to the ground in the situation envisaged by argument A. Statement (4) is a prediction of the as-if hypothesis (1). This prediction follows by logic and mathematics from (1) in conjunction with the auxiliary statements (2) and (3).

Now those who accept (02) would no doubt be willing to say that the realism of the assumptions of statement (1) of argument A can be determined *apart from* ascertaining the truth-value of prediction (4). Friedman represents the proponents of (02) as attempting to determine the realism of (1)'s assumptions by measuring the air pressure in the situation envisaged by argument A, and seeing whether it is close to zero.⁴⁶ As Friedman notes, at sea level the air pressure is about 15 pounds per square inch. This means the assumptions of hypothesis (1) are not *exactly* true—the measurement would have to be zero for this to be the case. But the assumptions of statement (1) of argument A are still realistic as long as they are *approximately* true, or sufficiently close to the truth. But how is *this* to be determined? Friedman's answer is that the *only* way to do so is to ascertain whether *prediction* (4) of argument A is accurate or true.⁴⁷ We can find out whether or not the assumption "it (the ball) is falling in a vacuum" is approximately true only by seeing whether the ball takes about four seconds to fall from the top of the building to the ground. This is the time the fall would take were the ball to fall 256 feet in a vacuum. Friedman concludes that the realism of the assumptions of an as-if hypothesis like (1) of argument A is only determinable by ascertaining the accuracy of predictions of that hypothesis. The application of this conclusion to economics means that (02) is mistaken.

Prima facie Friedman's case against (02) may seem cogent. Friedman is surely right in suggesting that measuring the air pressure in the situation argument A is concerned with cannot determine whether the assumptions of statement (1) of the argument are sufficiently close to the truth. As Friedman notes, "it is falling in a vacuum" is close enough to the truth when "it" refers to a compact ball, but very far from the truth when it is a feather that is being dropped from the building.⁴⁸ Yet whether it is a ball or a feather that is being dropped, measurement of the air pressure in the situation would give the same 15 pounds per square inch figure. And if measurement of the air pressure is powerless to determine whether the assumptions of statement (1) are realistic, what else is there to do the job but accuracy of the predictions of (1) such as step (4) of argument A? Well, there is something else to do the job which Friedman overlooks.

We can make a distinction between two ways of determining whether or not a statement P is realistic (true or approximately true). One way to do so is by ascertaining the accuracy of predictions of P, or better, by subjecting P to empirical test and seeing whether the test is favorable or positive. Another way is by ascertaining the logical relations of P—inductive as well as deductive—to *other*

statements which are *already* justifiably accepted. An example from economics will illustrate the distinction. In a paper published in 1940 and widely read by economists, Friedrich Lutz attempts to construct a theory or model about the market for bonds or debt obligations that would explain the different shapes yield curves can assume. (A yield curve shows the relationship between yield to maturity and term to maturity of different bonds at a single point in time.) Lutz's approach is to start out with a version of his theory that has unrealistic assumptions, and then, by relaxing the assumptions piecemeal, move in the direction of a model whose assumptions are realistic. The assumptions or axioms of the initial version of Lutz's model are as follows:⁴⁹

(A1) *All participants in the bond market (i.e., lenders and borrowers) have accurate or correct expectations about future short-term interest rates.*

(A2) *There are no costs for either lending or borrowing in the bond market.*

(A3) *There is complete shiftability, i.e., neither any lender nor any borrower has a preference for debt obligations of one maturity rather than another.*

Let us focus on assumption (A1). (A1) is unrealistic. But to determine this it is *not* necessary to subject (A1) to empirical test and get an unfavorable or negative result, i.e., infer one or more predictions from (A1) and then find out that the predictions are inaccurate.⁵⁰ We *already* accept the following statement:

(5) *Participants in the bond market often turn out to be wrong in their expectations about the course of future short term interest rates; and indeed, bond market participants usually lack any specific expectations about short term rates beyond one or two years in the future.*

Statement (5) implies that (A1) is rather far from the truth. Thus, whether assumption (A1) of Lutz's model is realistic is determinable by ascertaining its logical relations to already accepted statements like (5). To be sure, (A1) logically implies the denial or negation of (5). But the negation of (5) is not a prediction of (A1), as it is not a statement whose truth-value is as yet undetermined by us. Thus discovering the unrealistic quality of (A1) by reference to its logical relations to already accepted statements like (5), is distinct from determining (A1)'s lack of realism by examining its predictions.

The rather commonsensical distinction drawn in the preceding paragraph applies to Friedman's argument against (02). Friedman is wrong in suggesting that the *only* way to determine the realism of the assumption of statement (1) of argument A is by checking the ac-

curacy of predictions of (1). We can determine the realism of the assumptions of (1) by ascertaining the logical relations of those assumptions to already accepted statements. One pertinent statement we already accept is this:

(6) *When bodies such as compact balls and decent-size rocks fall to the Earth, the effect of air resistance is negligible.*

We can judge the assumptions of the as-if statement (1) of argument A to be realistic on the basis of (6). Given (6), we can say "it (the ball) is falling in a vacuum" is an approximation to the truth. We need *not* await what observation tells us about (1)'s assumptions.⁵¹ From what has been said I conclude that Friedman's case against (O2) described several paragraphs back is unsound. Moreover, the distinction drawn in the preceding paragraph clearly warrants saying (O2) is *correct*, and thus Friedman is wrong to reject it. (O2) affirms that the realism of the assumptions of an economic theory or hypothesis H is determinable apart from H's predictions. The distinction of the preceding paragraph applied to economics justifies saying this is true.⁵²

Though Friedman is mistaken in rejecting (O2), he is right to reject (O1). Recall that (O1) says that an economic theory or hypothesis is acceptable only if its assumptions are realistic. Friedman provides examples of as-if hypotheses outside of economics that he claims are acceptable despite the unrealistic quality of their assumptions. One such example is as follows:

(1) *Leaves are positioned around a tree as if each leaf deliberately seeks to maximize the amount of sunlight it gets, knows the physical laws determining the amount of sunlight it would get in the various positions on the tree, and is able to move quickly to any unoccupied position on the tree.*

Concerning this as-if hypothesis Friedman says the following:

Is the hypothesis rendered unacceptable or invalid because, so far as we know, leaves do not "deliberate" or consciously "seek," have not been to school and learned the relevant laws of science or mathematics required to calculate the "optimum" position? Clearly, none of these contradictions of the hypothesis is vitally relevant; the phenomena involved are not within the "class" of phenomena the hypothesis is designed to explain"; the hypothesis does not assert that leaves do these things but only that their density is *as if* they did. Despite the apparent falsity of the "assumptions" of the hypothesis, it has great plausibility because of the conformity of its implications with observation.⁵³

Here Friedman rightly says the assumptions of hypothesis (1)—what comes after the term “as if” in (1)—are false or unrealistic. But he claims (1) is acceptable or highly plausible. Now we can readily grant Friedman the general point he is trying to make with his example of (1), viz., as-if hypotheses can be accepted though their assumptions are unrealistic and are known to be so. The acceptability of an as-if hypothesis no more depends on the realism of what comes after “as if” than the acceptability of “if it is raining hard, then the streets are wet” depends on the truth or approximate truth of the antecedent “it is raining hard.” Since the point holds for as-if hypotheses in general, it holds for such hypotheses in economics. Thus (01) is false as Friedman claims.

Despite (01)'s falsity, there is an element of truth in it which should not be overlooked. Recall that there are two types of items to which the term “assumptions” applies. What comes after the term “as if” in an as-if hypothesis are assumptions of the hypothesis, and the axioms of a deductive systematization of a theory count as assumptions of the theory. Now (01) concerns the acceptability not only of economic hypothesis, but theories or models as well. (01) says an economic theory is acceptable only if its assumptions are realistic. But surely this is reasonable, bearing in mind that we can only speak of the assumptions of a theory relative to some particular systematization of it. Suppose we discover that the axioms of a systematization of an economic theory are unrealistic, i.e., not even approximately true. In that case the theory includes statements that are not even approximations to the truth. Surely it would not be epistemically permissible to accept such a theory. Friedman's case against (01) discussed in the preceding paragraph focuses exclusively on the assumptions of as-if hypotheses and ignores the assumptions of economic theories or models. But this leads Friedman to overlook the fact that what (01) says about the acceptability of theories in economics is correct.

CONCLUSIONS

I have tried to give a somewhat different picture of Friedman's position in “The Methodology of Positive Economics” than has been presented in the literature to date. On this interpretation, the methodology of economics Friedman recommends to his fellow economists consists of the epistemic rules (R1), (R2), and (R3). Moreover, Friedman rejects as mistaken the view of the role of the realism of assumptions in appraising economic theories and hypotheses represented by claims (01) and (02). My treatment of Friedman has, I believe, enabled me to bring out more adequately than has been done hitherto what is really wrong with Friedman's position in his 1953 article. The defect of Friedman's epistemic rules (R1), (R2), and (R3) is that they presuppose the mistaken simple hypothetico-deductivist view of testing scientific theories and

hypotheses. As for the claims of Friedman's opponents on economic methodology, though Friedman is right in thinking (01) is false, he fails to see that what (01) says about economic *theories* is right. And finally, Friedman's rejection of (02) is vitiated by his failure to recognize that examining predictions of a theory or hypothesis is not the only way to determine the realism of its assumptions; statements already justifiably accepted also provide a way to do this. The fact that Friedman is wrong to reject (02) causes the collapse of his effort to save neoclassical microeconomic theory from the charge that it incorporates unrealistic assumptions. For, as I pointed out, Friedman's effort to rebut this charge relies on his view that (02) is wrong.

1. Milton Friedman mentions such criticisms. See Friedman, "The Methodology of Positive Economics," in Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), p. 15, pp. 30-31.
2. Friedman, "The Methodology of Positive Economics," p. 3.
3. *Ibid.*, p. 11.
4. *Ibid.*, p. 26. Actually Friedman's view of theories is a bit complex. In his writings he offers two characterizations of theories which he does not bother to relate to one another. Friedman regards a theory as the union of a set of so-called tautologies and a set of so-called substantive hypotheses. See *ibid.*, p. 7. But he also regards a theory as a deductively systematizable set of statements containing one or more ideal-object terms such as "perfectly competitive market" or "mass point." See *ibid.*, p. 34. In discussing Friedman's views I have chosen to use "theory" to simply mean a set of statements or sentences susceptible of deductive systematization. No serious distortion of Friedman's views will result from adopting this use of "theory." And doing so provides a clear sense of "theory" in which the term is in fact applicable to economic theories or (as economists are more likely to call them) models.
5. Friedman, "The Methodology of Positive Economics," pp. 12-13.
6. In talking of statements about observable phenomena it must not be thought I am foisting on Friedman the logical empiricist view that the language of a nonformal science like economics contains observation sentences. These are supposed to contain so-called observation terms plus logical and mathematical terms, but no theoretical terms. Nowhere does Friedman indicate that he accepts the observation term/theoretical term dichotomy built into the logical empiricist notion of an observation sentence. Indeed, at one point Friedman denies the independence of observation from theory presupposed by that dichotomy. See Friedman, "The Methodology of Positive Economics," p. 34. However, I am attributing to Friedman the view that a nonformal science includes statements which describe only what the observable phenomena are like. For a discussion of this type of statement and the need to distinguish it from the observation sentences of the logical empiricists, see Bas Van Fraassen, *The Scientific Image* (London: Oxford University Press, 1980), pp. 13-19; 53-54.
7. Friedman, "The Methodology of Positive Economics," p. 9. Friedman emphasizes that in this use predictions are not necessarily about the future. They may be about situations in the past or present whose existence we have yet to determine.
8. *Ibid.*
9. See Carl Hempel, *Philosophy of Natural Science* (Englewood Cliffs, N.J.: Prentice Hall, 1966), p. 18.
10. See Karl Popper, *The Logic of Scientific Discovery* (New York and Evanston, Ill.: Harper & Row, 1968), chap. 10. The relation between Popper and Friedman is discussed by William Frazier and Lawrence Boland, "An Essay on the Foundations of Friedman's Methodology," *American Economic Review* 73 (1983): 129-144.

11. Friedman, "The Methodology of Positive Economics," p. 9.
12. It is generally agreed nowadays by philosophers that usually a scientific theory does not on its own imply any evidence statements; suitable auxiliary statements must be conjoined with the theory to generate predictions. This principle, which might be called "the principle of the underdetermination of evidence by theory," is not discussed by Friedman. But nothing he says about the empirical testing theories is incompatible with it.
13. Terms of epistemic appraisal are terms we use to appraise or evaluate people's acceptance or rejection of statements. Examples are "unreasonable," "justified," "more reasonable than," "acceptable," and "should not reject." An epistemic rule of a discipline is a sentence with this feature: it specifies a condition (necessary, sufficient, etc.) for applying some term of epistemic appraisal to the acceptance or rejection of statements or theories in the discipline. The methodology of a discipline can be regarded as a set of epistemic rules for the discipline.
14. Friedman, "The Methodology of Positive Economics," pp. 8-9.
15. *Ibid.*, p. 10. Some commentators have overlooked Friedman's acknowledgement of the role of simplicity and fruitfulness in theory appraisal. For example, Alexander Rosenberg says Friedman holds theories are to be judged *solely* by their predictive success. See "Friedman's 'Methodology' for Economics: A Critical Examination," *Philosophy of the Social Sciences* 2 (1972): 17.
16. See Willard Quine, *Word and Object* (Cambridge, Mass.: Harvard University Press, 1960), p. 19.
17. Friedman, "The Methodology of Positive Economics," p. 10.
18. *Ibid.*, p. 14.
19. The first two appear in Friedman, "The Methodology of Positive Economics," pp. 21-22 and p. 37 respectively. The third appears in Friedman and Leonard Savage, "The Expected Utility Hypothesis and the Measurability of Utility," *Journal of Political Economy* 60 (1952): p. 463.
20. Friedman, "The Methodology of Positive Economics," p. 15.
21. *Ibid.*, p. 26.
22. *Ibid.*, p. 25.
23. Roughly an absolute general term F is an ideal object term if F is part of the vocabulary of some discipline yet relative to accepted views there could not be anything in the real world denoted by F. Examples are "perfectly competitive market," "frictionless surface," and "Euclidean point."
24. Friedman, "The Methodology of Positive Economics," pp. 14-15.
25. *Ibid.*, p. 16.
26. See Paul Samuelson, "Problems of Methodology—Discussion," *American Economic Review: Papers and Proceedings* 53 (1963): 233.
27. Richard Lester's "Shortcomings of Marginal Analysis of Wage-Employment Problems," *American Economic Review* 36 (1946):65. Lester's questionnaire indicated that actual and expected firm sales were the chief considerations managers took into account in deciding how much labor to hire.
28. Friedman, "The Methodology of Positive Economics," p. 31.
29. Friedman's as-if analysis of motivational hypotheses in economics is behind his claim that (1) is not a prediction of MPT. For Friedman the hypothesis that firms maximize profits is to be construed as affirming (a) firms behave as if the managers have as their goal maximizing profits and have the knowledge needed to reach this goal. Note that (a) does not imply anything about the goals or other psychological states of firm managers, any more than "Jack behaves as if he is insane" implies that Jack is insane. Nor does (a) imply or predict anything about what firm managers *say* about their goals or bases for making decisions.
30. See Stanley Wong, "The F-Twist and the Methodology of Paul Samuelson," *American Economic Review* 63 (1973): 314.
31. Friedman, "The Methodology of Positive Economics," p. 7.
32. *Ibid.*, p. 41.
33. Lawrence Boland, "A Critique of Friedman's Critics," *Journal of Economic Literature* 17 (1979): 507, 508-509, 511.

34. This is supported by Boland, "Friedman's Methodology vs. Conventional Empiricism: A Reply to Rotwein," *Journal of Economic Literature* 18 (1980): 1556.

35. Boland mentions simplicity and fruitfulness in "A Critique of Friedman's Critics," pp. 511-512. He appears to ascribe to Friedman the epistemic rule (a) a theory generating one or more false predictions should be rejected or eliminated. See *ibid.*, p. 511. But (a) is not the same as (R2) which does not make a single false prediction sufficient for rejectability.

36. Daniel Hausman, "Defending Microeconomic Theory," *The Philosophical Forum* 15 (1984): 394-395. Though I am critical of Hausman's views, private correspondence with him has been useful to me in thinking about Friedman's relation to instrumentalism.

37. In his article Hausman does not quote any passages from Friedman's writings. But in support of attributing (d) and (e) to Friedman he does cite pages 14 and 19-20 of "The Methodology of Positive Economics." I cannot detect in these pages the presence of any distinction between predictions generated by a theory that are of interest and predictions of no interest.

38. Friedman says it is generally agreed that simplicity and fruitfulness are relevant to choosing a unique theory from among alternatives that are equally confirmed by available evidence. See Friedman, "The Methodology of Positive Economics," p. 10. Given this Friedman presumably thinks arguing for (R3) would be otiose.

39. Friedman, "The Methodology of Positive Economics," pp. 8-9.

40. One account of the difficulty appears in Stephen Barker, *Induction and Hypothesis* (Ithaca, NY: Cornell University Press, 1967), pp. 155-156. Another version appears in Clark Glymour, *Theory and Evidence* (Princeton, NJ: Princeton University Press, 1980), pp. 29-31. The difficulty with the simple H-D view of testing and confirmation attaches to both the inductivist and noninductivist versions.

41. It might be said—though nowadays few philosophers would say it—that statement (2) is empirically meaningless, and so the conjunction of (1) and (2) is itself empirically meaningless. And, it might be added, since empirically meaningless sentences are not admissible for testing in the first place, the simple H-D view is not applicable to such sentences. But the problem with these remarks is that the alleged distinction between empirically meaningful and empirically meaningless sentences has never been successfully drawn. For a discussion of a variety of unsuccessful attempts to draw this supposed distinction, see Carl Hempel, "Empiricist Criteria of Cognitive Significance: Problems and Changes," in *Aspects of Scientific Explanation* (New York: Free Press, 1965), pp. 101-122.

42. This epistemic principle can be accepted by noninductivists like Friedman. It does not carry a commitment to saying there are any good arguments other than deductively valid ones. Specifically, the principle does not imply that the fact that a hypothesis has withstood testing affords good though less than conclusive grounds for thinking it is true. Though the principle conjoined with this fact logically implies that the hypothesis is more worthy of acceptance than before testing.

43. Both adjustments were proposed to me in conversation by Ted Watkins (Department of Economics, San Jose State University). The adjustments are minor in that their addition to the simple H-D view does not result in a version which is substantially more complex. A complicated version of the hypothetico-deductivist view of testing and confirmation is found in Gary Merrill, "Confirmation and Prediction," *Philosophy of Science* 46 (1979): 103-106. Glymour has argued that Merrill's version is unacceptable. See Glymour, "Hypothetico-Deductivism is Hopeless," *Philosophy of Science* 47 (1980): 322-325.

44. Halley's prediction and the test it afforded of Newton's theory is discussed in Ronald Giere, *Understanding Scientific Reasoning* (New York: Holt, Rinehart, Winston, 1979), pp. 85-88. Giere does not employ the example in the context of criticizing any variant of the hypothetico-deductive method.

45. Argument A does not appear in "The Methodology of Positive Economics." But several component statements of the argument do appear in Friedman's article. And argument A is a useful device in stating his case for thinking realism of assumptions is ascertainable only by examining predictions. Friedman's case is developed in "The Methodology of Positive Economics," pp. 16-18.

46. Friedman, "The Methodology of Positive Economics," p. 16.

47. *Ibid.*, p. 16-17.

48. *Ibid.*

49. Friedrich Lutz, "The Structure of Interest Rates," *Quarterly Journal of Economics* 55 (1940): 36-37.

50. I assume that *in part* testing a theory or hypothesis is obtaining from it one or more predictions and determining the correctness of those predictions. This assumption is consistent with sophisticated or complex forms of hypothetico-deductivism, as well as the Bayesian approach defended by philosophers of science such as Wesley Salmon. In the present context no harm will result from ignoring aspects of testing other than inferring predictions from the hypothesis under test and checking the accuracy of those predictions.

51. It might be said that our basis for accepting statement (6) is that in actual situations resembling the one envisaged by argument A predictions of statements like step (1) of the argument have turned out to be true. That is, our basis for accepting (6) is that in cases we have experienced the actual behavior of falling bodies like compact balls closely fits the behavior predicted by the formula $s = 16t^2$. This may be, but it does not affect my point. The fact is that the realism of the assumptions of a hypothesis such as (1) of argument A is ascertainable without reference to the predictions of (1) itself. And it must be emphasized that Friedman's view is *not* that the realism of the assumptions of a hypothesis such as (1) of argument A is determinable by reference to a different and already accepted hypothesis such as (6) whose predictions have in the past turned out to be true. Such past predictive success of a different hypothesis is, in Friedman's view, irrelevant to determining the realism of the assumptions of the hypothesis at hand. Friedman's view is that the realism of the assumptions of a given hypothesis or theory are only ascertainable by reference to the predictions of that hypothesis or theory itself. This is the view Friedman commits himself to in rejecting and arguing against (02).

52. Among Friedman's numerous critics Peter McClelland is unusual in recognizing that Friedman is wrong to reject (02) because it overlooks the possibility of determining the realism of the assumptions of a hypothesis or theory by examining the logical relations of those assumptions to already accepted statements. See McClelland, *Causal Explanation and Model Building in History, Economics, and the New Economic History* (Ithaca, N.Y.: Cornell University Press, 1975), pp. 136-139. However, McClelland is not justified in saying Friedman inconsistently rejects (02) and also allows that the correctness of the assumptions of a theory can be assessed other than by examining its predictions. In the passage on page 26 of "The Methodology of Positive Economics" which McClelland cites to support this, Friedman is *not* discussing the assessment of the assumptions of a theory but the decision of which of the statements belonging to it to use as axioms or assumptions.

53. Friedman, "The Methodology of Positive Economics," p. 20. Friedman is speaking carelessly in the midsection of this passage when he says such statements as "leaves do not deliberate" are contradictions of hypothesis (1). For, as the latter half of the passage attests, Friedman does not think (1) implies that leaves do deliberate.

Discussion Notes

METHODS OF ABORTING

Elective abortions became legally permissible in the United States on January 22, 1973.¹ The Supreme Court in the *Roe v. Wade* decision argued that a pregnant woman enjoys a constitutional right of personal privacy. Although the Constitution does not explicitly mention “privacy,” such a right may be inferred, it was argued, from the First, Fourth, Fifth, Ninth, and Fourteenth Amendments. The so-called penumbra right, which entails that two conditions must be satisfied if the right is to be overridden, is fundamental. First, the government (state or federal) must have a compelling, as opposed to an important or legitimate, reason for overriding personal privacy, i.e., the governmental goal is perforce. Second, the means by which the governmental goal is achieved must be essential and least restrictive, as opposed to being substantially or merely rationally related to the goal. Under such “strict scrutiny” analysis it was determined that the government had no compelling interest in proscribing elective abortions. Such abortions, so long as they are performed within the first trimester, place in jeopardy only the life of the fetus. Only if the fetus enjoys a constitutionally protected right to life will there obtain sufficiently compelling grounds to abate the pregnant woman’s right of privacy.

The fetus was argued not to enjoy such a right largely because the fetus is not considered a “person” under the Constitution.² The Court noted that the right of privacy enjoyed by the pregnant woman was not absolute and that the government may regulate and even proscribe abortion in accordance with the government’s interest in protecting the health of the pregnant woman and fostering prenatal life. Consequently, it was determined that it is not unconstitutional to regulate the abortion procedure in ways that are reasonably related to the woman’s health during that period when the abortion procedure poses a threat to such, namely, after the first

trimester.³ Furthermore, except where necessary, it was deemed permissible to proscribe abortions to protect the viable fetus from harm.

The decision of the Supreme Court fares well with the position of one pro-choice group according to whom the fetus, at least to the point of viability, is merely a potential person and thus, merely the potential holder of the right to life. Under normal circumstances, it might be argued, the potential to do or be X is never by itself sufficient to grant to the potential X, the status of X.⁴ If the Constitution does not appraise the fetus as a person, then to treat the fetus as a person on the basis that it has the potential to be such, is to do something that is ruled out by the preceding argument.

There is at least one way in which pro-life advocates might respond.⁵ They might suggest the introduction of a constitutional amendment granting personhood and a full right to life to the fetus. It is argued that such an amendment would supply the compelling state interest necessary for proscribing elective abortion. There is, however, one pro-choice position which maintains that even if the fetus were to enjoy a full right to life, abortions would be permissible since they would not violate said right. To inquire into the relationship between a fetal right to life and abortion and to note some possible unhappy ramifications with that relationship shall be the purpose of this note.

According to Judith Jarvis Thomson, a woman's right to an abortion need not conflict with the fetus's right to life.⁶ First, the intention in having an abortion is merely to terminate the pregnancy or the physical dependency of the fetus upon the mother, not the life of the fetus. If the fetus could survive the separation from the mother within the first two trimesters, it would not be acceptable to kill it. Unfortunately, the fetus usually cannot survive the separation. The termination of the fetus's life is not, however, the purpose of the abortion, even though fetal death is an expected result of terminating a pregnancy.⁷

Second, a right to life requires only that the dutyholder not endanger the rightholder's life. It does not by itself require the dutyholder to save the rightholder's life.⁸ In the case of a pregnancy, the body inhabited by the fetus rightfully belongs to the mother.⁹ The mother merely wishes to reclaim her body from the fetus, and thus, fails to save the fetus. Even though the fetus dies as a result of not being saved, the failure to save does not violate the fetus's right to life.¹⁰

Whatever else might be proffered regarding Thomson's argument,¹¹ it seems unlikely that all abortions are merely cases of not saving.¹² Vaginal evacuation by either dilation and curettage or endometrial aspiration kills the fetus (in fact, although not in principle) in the process of separating it from the mother. Killing the fetus in

the process of separating it from the mother is quite different from separating it from the mother with ensuing fetal death. One cannot kill another in the process of not saving them unless saving them would result in the saver's death. For instance, assume that both A and B are diabetics, that the only supply of insulin belongs to A, but that it is in the possession of B and that without insulin each will die. If A reclaims the insulin he merely fails to save B. If he cannot reclaim the insulin without killing B in the process, then B's death is justified only if A's life is also in jeopardy. If A is not himself in danger of dying, he cannot kill B in the process of reclaiming what is his, although he still may reclaim what is his. Consequently, killing the fetus in the separation would only be justified if the mother's life were itself in jeopardy. The mother's life is not in question in elective abortions, but the mother is not justified in killing the fetus in the process of freeing herself from (not saving) the fetus.

It will be to no avail to argue that the previable fetus will die after the separation anyway, as it makes little difference if the fetus is killed in order to effect the separation. It does not follow that one may be killed if one is going to die. After all, one is not entitled to kill the terminally ill patient or kill the prisoner prior to his execution. It seems highly unlikely, therefore, that the ensuing death of the fetus could be used to justify the use of an abortion procedure that killed the fetus in the process of terminating the pregnancy.

Along with the surgical removal of the fetus and the placenta a somewhat different verdict befalls some abortion procedures that initiate uterine contractions. Prostaglandin, but not saline, abortions might prove morally acceptable, since they merely expel the fetus from the uterus with ensuing fetal death. In this case the fetus is merely "not saved" by the mother and this lack of saving is compatible with the fetus's right to life. However, such abortions can only be easily performed toward the end of the second trimester (approximately the 24th week), because instilling the material into the amniotic sac is different prior to that period of gestation. This would mean that unless vaginal evacuation abortions can be perfected so as to eliminate the killing of the fetus in the abortion procedure, it would be morally preferable to have an abortion during the second rather than the first trimester. This conclusion is all the more difficult to accept when it is realized that, not only is the fetus's development approaching viability, but in terms of danger to the mother, second-trimester abortions present higher-complication risks than first-trimester abortions. Both considerations run counter not only to the spirit of the *Roe v. Wade* decision but also to prudent medical judgment.

Nevertheless, if the fetus is granted a full right to life and the arguments presented above are sound and until abortions by vaginal evacuation are perfected so as to circumvent killing the fetus,

second-trimester abortions will be morally preferred to first-trimester abortions, the increased danger to the mother and the near-completed development of the fetus notwithstanding.

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1. *Roe v. Wade*, 410 U.S. 113, 93 S.Ct. 705, 35 L.Ed.2d 147.
2. The Court looked at the use of "person" in Article I, sections 2,3, and 9; Article II, section 1; Article IV, section 2; and the Fifth, Twelfth, Fourteenth, and Twenty-Second Amendments. Prior to 1946, courts denied tort recovery to a child or his/her estate for prenatal injuries, one of the reasons being that there could be no duty posed by one who was not in existence at the time of a defendant's action. See *Drabbels v. Skelly Oil Co.*, 155 Neb. 17, R 50 N.W. 2d 229.
3. Although a saline abortion also initiates uterine contractions (and thereby expels the uterine contents), it does so by destroying the placental and fetal action that inhibit uterine contractions. Thus a saline abortion tends to kill the fetus which is dissimilar to a prostaglandin abortion. In *Planned Parenthood of Missouri v. Danforth*, 428 U.S. 52, (1976) the U.S. Supreme Court invalidated a Missouri provision proscribing saline abortion in favor of the prostaglandin method, the greater likelihood of saving the previable fetus with the latter method notwithstanding. This was due to the Court's reasoning that this provision was not a *reasonable* regulation.
4. Michael Tooley, "Abortion and Infanticide," *Philosophy and Public Affairs* 2 (Fall 1972): 47-66.
5. See Senate Joint Resolution 119 (1973) and 130 (1973). There is at least one other way in which prolife members of Congress have endeavored to respond to *Roe v. Wade* besides the one mentioned. The second is based upon States' Rights and notes that abortion will be countenanced or not depending upon the will of the people of a state. See, for instance, House Joint Resolution 527 (1973) and Senate Joint Resolution 110 (1982) and 3 (1983). The problem with this endeavor is that without a new constitutional amendment, the violation of fundamental rights is not something open to democratic votes. If an elective abortion is protected by one's right of privacy, then a majority vote will not make legal the transgression of the right.
6. Judith Jarvis Thomson, "A Defense of Abortion," *Philosophy and Public Affairs* 1 (Fall 1971): 47-66.
7. Consider recent litigation concerning the killing of live abortuses, e.g., Tennessee Code section 39-306. See also, *Planned Association of Kansas v. Ashcroft* (1983), 462 U.S.
8. This same type of argument was utilized to restrict fundings for abortion. See, *Maher v. Roe*, 432 U.S. 464, 97 S.Ct. 2376, 53 L.Ed.2d 484.
9. This contention has recently been questioned. See Mark Wicclair's "The Abortion Controversy and the Claim that this Body is Mine," *Social Theory and Practice* 7 (Fall 1981): 337-346.
10. With identical intentions, "not saving" and "killing" might be argued to be morally equivalent. See James Rachels, "Active and Passive Euthanasia," *New England Journal of Medicine* 292 (September 1975): 78-80. Thus, if the intention in separating the mother and fetus is to ensure fetal death, it would be morally equivalent to killing the fetus and would consequently violate the fetus' right to life. But since the intended death of the fetus may be absent in abortion, such a separation may not be morally equivalent to killing the fetus.

11. It might be wondered if the mother's special relationship to the fetus does not present the mother with a positive duty to the fetus. If so, not saving would be morally blameworthy unless the limits to positive obligations are satisfied in the case of elective abortions. For example, while a mother could not allow her child to starve to death, i.e., she suffers a positive obligation to feed the child, it is not similarly clear that she could not allow the child's death or need of an organ only she could supply, i.e., she does not suffer a positive obligation to save her child's life by donating one of her own organs. Although the child would die in each case without the mother's intervention, the second might be allowed as there are limits to positive duties.

12. Even if all abortions were cases of "not saving," it might be questioned whether abortions would be palpably permissible, notwithstanding the doubts raised in note 11. In tort law, the doctrine of necessity allows defendants to escape liability if during an emergency, e.g., when life threatens, the defendant trespasses upon the plaintiff's property or chattels. In a personal or private emergency the defendant is only liable for damage suffered by defendant but not for the trespass, and this is due to the defendant's right to the ephemeral use of the property during such an emergency. The differences between one's property, both real and personal, and one's body notwithstanding, might the fetus be analogous to the defendant in the above case and the mother analogous to the plaintiff?

TROUBLES WITH FLOURISHING: COMMENTS ON DAVID NORTON

I would like to serve as devil's advocate by raising some particular questions about David Norton's brief defense in "Is 'Flourishing' a True Alternative Ethics?" of his version of an ethics of flourishing, with the aim of getting him to say a bit more.

"The flourishing of artifacts, organs, and animals is non-moral for they have no choice in the matter; human flourishing fulfills the moral condition of choice, for the will of the individual must be enlisted if flourishing is to occur." Despite the Aristotelian precedent, I think this cannot be right. One might very well believe (as I do) that animals make choices, without being forced to conclude that the flourishing of such animals is moral. The issues are independent.

"Functional evaluation of artifacts, organs, and animals is secondary to and derivative from human flourishing because human flourishing is the agency by which value is realized in the world." This also seems wrong to me. Some plants had good roots before there were any people, and would have had good roots even if there had never been any people.

"At bottom we want to be of worth to others . . ." There are two possibilities here: (1) A person may want to be "of worth" and to be recognized by others as "of worth"; and (2) A person may want to matter to other people—a person may want to be valued by (certain) other people. (2) is weaker than (1), since you can value someone without thinking the person is "of worth," period. I myself do not believe that anyone is simply "of worth," period, but there are many people that I value.

An imperfect society is "not *unreal*; the notion of "what is less than perfect is less than real" is not characteristic of the ethics of flourishing per se, but only of such an ethics as it figures in the metaphysics of Platonic realism or the metaphysics of Absolute

Idealism” This is to misunderstand the relevant metaphysics. To say that a certain coward “is not a real man” is not to say that he is in any way “unreal.”

On the matter of resolving disagreements by having everyone do his or her own thing, that might resolve the “disagreement” that occurs when Jack thinks that for him to flourish involves living a life of sort *A*, whereas Jill thinks that for her to flourish involves living a life of sort *B*, where that is incompatible with living a life of sort *A*. Here Jack and Jill are “disagreeing” (if that is the word) about different people. But they can also (really) disagree about the same person. (It might be Jack, or Jill, or some third person, Jane.) They can disagree over what it is for that person to flourish. Similarly, they can disagree over what it is for people in general to flourish. Clearly, this sort of disagreement is not resolved by having everyone do his or her own thing. (That may be one of the positions under dispute, Jack holding that each person should do his or her own thing, Jill disputing this.)

“If we are to act for the greatest happiness (or utility, or flourishing, or whatever) of the greatest number, then on occasions of moral choice we on average have a tenth of a say in determining our own conduct while others determine our conduct by nine-tenths.” Norton takes this to involve a lack of autonomy. But a similar result will hold if one acts on any moral principles at all that allow for duties to others. An argument of this form can be given for the conclusion that one does not act autonomously whenever one acts in accordance with *principle* rather than in accordance with one’s own unprincipled preferences. I assume that is an unwelcome result.

In any event, I suggested that an ethics of flourishing will be committed to utilitarianism. If, because of a commitment to autonomy (understood in this extreme way), it is also committed to something incompatible with utilitarianism, it may be simply inconsistent. To argue that an ethical theory is committed to something incompatible with utilitarianism is not yet to argue that it is not also committed to utilitarianism.

The argument for “the requirement of autonomy in the ethics of flourishing” is that “it is to be found in all of the advocates of such an ethics from Aristotle to Nietzsche to Emerson and Thoreau.” The name of this argument is “the argument from authority.” It is an odd argument from an advocate of autonomy.

“Each person is innately invested with potential worth, and the responsibility for actualizing our worth is the inherent demand of potential worth for actualization The ultimate justification of an ethics of flourishing, then, is consequentialist: more human values will be actualized this way than any other. But the claim is that the consequence is such that it can only come from flourishing.” This implies that, whenever you have a choice between two actions, *A* and *B*, where more human values will be actualized by doing *A* than by doing *B* (for example, because doing *A* saves the lives of several

people who will go on to flourish), then you will flourish more if you do A than if you do B. In other words, the claim implies that it is impossible ever for a person to sacrifice his or her own flourishing for the sake of a greater good involved in the flourishing of others. It is evident that this consequence of the claim is false.

Finally, about imitation or emulation. Obviously, one can never do exactly what another person does. One can only do what is the same in certain respects. There is a question then of saying in what respects.

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Review Essays & Reviews

Reasons and Persons. By Derek Parfit. Oxford: Clarendon Press. 1984.

Traversing *Reasons and Persons* is the philosophical analog of a drive along the Southern California freeway system. Traffic in ideas is bumper-to-bumper, routes merge into each other with little prior notification, the interchanges leaving one breathless, and the novice motorist might do better to practice on more forgiving roadways. On the other hand, the vast proliferation of paths really does hang together, and one genuinely is led to marvel at the fact that the mind of man succeeded in producing so stunning a design.

This is a *big* book. Its 543 pages include four major sections, each of which could constitute an independent volume, 10 appendices, 27 pages of end-notes, a bibliography, and an index of names. (There is, however, no subject index, an unfortunate omission in a book so rich in interconnected ideas.) Part One discusses ways in which theories can be self-defeating, with special attention to S, the Self-Interest theory of rationality; C, Consequentialism of a broadly utilitarian sort; and M, Parfit's rendering of Common-Sense morality. Part Two, "Rationality and Time" places S under the diagnostic lamp once again, this time specifically targeting the claim that rational self-interest entails equal concern for all temporal stages of one's life. Part Three concerns personal identity, an area of inquiry Parfit has notably advanced in his earlier work.¹ He argues that personal identity is nothing over and above nonbranching psychological connectedness and continuity, and that, contrary to standard belief, whether some future individual will be *me* (rather than someone who merely resembles me closely along salient psychological dimensions) does not very much matter. Finally, in Part Four Parfit addresses the question of which moral principles should guide our decisions when what we do will not only affect the welfare of future individuals but will causally determine which and how many persons will come to be.

Such a book is both the bane and bonanza of reviewers. It is impossible even to mention, let alone examine in depth, all the important issues on which Parfit shines the spotlight of his formidable analytical powers. One must pick and choose. No matter though; one could throw darts at the pages and be assured of striking something of surpassing interest. In sections I through IV below, I shall nod at each of the main divisions of *Reasons and Persons*. Section V briefly assesses the overall thrust and importance of the

work. Even if this provides a tolerably accurate map of the terrain Parfit covers, it cannot begin to convey a sense of the extraordinary subtlety and brio he brings to these explorations.

I

“For each person, there is one supremely rational ultimate aim: that his life go, for him, as well as possible.” (p. 4) So speaks (one version of) the Self-Interest theory of Rationality (S). To it stand opposed other voices, notably those that we identify as demands of morality. While S prescribes ultimate concern for one’s own well-being, every nonegoistic version of morality requires at least some sacrifice of self-interest in order to advance the prospects of other persons. Because S and morality are alike in purporting to provide *definitive reasons for action*, the opposition is not benign. Some writers take it to be self-evident that moral reasons must be definitive. The claim of S to be an adequate theory of rationality can therefore be dismissed out of hand. Others take it as evident that S is the definitive theory, and so they either dismiss morality as nonrational or else make it subsidiary to S.² A third group, of whom Sidgwick is perhaps the most notable representative, finds the opposition between S and morality insufficient grounds to dismiss either. Sidgwick classifies S as a “method of ethics” because it prescribes over the same range and in the same apodictic tones as does morality. Its reason-giving character is not impugned by its opposition to morality, nor is the reason-giving character of (utilitarian) morality defeated by S. We are left with two mutually inconsistent theories of ultimate and definitive reasons for action. Sidgwick quite appropriately views this as a philosophical failure.

Parfit, like Sidgwick, finds the opposition of S and morality insufficient reason to dismiss either. He does not, however, acquiesce in Sidgwick’s reluctant conclusion that the theory of practical reason must remain perpetually schizophrenic. Instead, he adopts the strategy of examining ways in which theories can be *self-defeating*. The lure of S will be resistible if the adoption of S leads to the defeat of the goal that S prescribes as supreme for each agent. This may seem to be a straightforward exercise in demonstrating the internal inconsistency of a theory. Matters are not, however, so simple. Inconsistency is one way in which a theory can be self-defeating, but it is not the only one. Moreover, a theory which, in one respect, is found to be self-defeating may yet survive to prescribe another day.

Parfit’s opening jab is to show that S can be *indirectly individually self-defeating*. Kate is a writer whose strongest desire is that her books be as good as possible. Even though, because of the strength of this desire, she sometimes works to the point of exhaustion, writing books provides her much happiness. Were her strongest desire instead that her life go as well for her as possible, she would not drive herself to exhaustion, but neither would she enjoy the same zest for writing. That is, were she to adopt S she would secure less of what S prescribes as the ultimate goal for a person. Similarly, a person with altruistic concerns may derive more satisfaction than someone all of whose desires are self-directed. To generalize, it can be

the case that one who rejects S as determinative for himself will secure more of what S takes to be of ultimate value than he would have accrued had he accepted S.

Does this amount to a refutation of S? Parfit argues that it does not. That is because, roughly, the failure is not that of S but of the agent. It is simply a fact about Kate that she is unable to substitute for her desire to write books some other motivation that will make her life go better. Should Kate's strongest motivation be to act on S then she will, on each occasion, produce the best possible result for herself. However, having that motivation is what makes things go less well for her. Therefore, S counsels that Kate not be motivated always to try to make her life go as well for herself as possible; she does better instead to try above all to write good books. Although Parfit does not put it in this way, we might say that S is properly conceived of as a metaprinciple for the appraisal of first-order principles that guide action. For some persons S will also be an S-optimal first-order principle, but for others it will not.

It is not only S that is indirectly self-defeating. C, Consequentialism, can also prove suboptimal precisely in terms of what C holds to be valuable. For example, persons who aim above all at maximizing the well-being of everyone may be deficient repositories of happiness because they lack ordinary human concerns for specific persons and projects. A world of individuals who succeed perfectly in acting as C directs may contain less happiness than if each had some ultimate motivation other than C.

Parfit next proceeds to consider how theories can be *collectively self-defeating*. When applied to S, this involves a situation in which each person succeeds in doing what is most in his own interest, but where the result for all of them is worse than if each had adopted some other strategy. The vast literature on prisoners' dilemmas provides a stock of examples that Parfit augments with his own ingeniously constructed cases. Prisoners' dilemmas do not, however, demonstrate S to be inadequate. Each participant in a (multi-person) prisoners' dilemma succeeds in bringing about the best possible result for himself. Were anyone altruistic, the outcome for himself would be yet worse. It is each person's bad luck that every other participant is guided by S.

If potential participants in a prisoners' dilemma could choose for themselves and others basic motivations, they would do better to engender morality rather than the belief that S is true. Even if S is the true account of rationality, it recommends, "Believe morality (to be what is most rational) rather than me." S is, says Parfit, *self-effacing*.

So also, he claims, is M, Common-Sense morality. While precise delineation of M is difficult, on any plausible construal it involves special obligations toward persons with whom one stands in relationships of intimate associations. For example, I have a special obligation to care for the children that are mine. I am not at liberty to be entirely cavalier toward other people's children, but I owe them less than I do my own. This can generate suboptimal outcomes. Suppose that I can extend either some minimal benefit to my child or a substantially greater benefit to your child. Suppose also that you are correspondingly situated. If you and I are both guided by

M, we produce a result less M-good for each of us than if we were motivated otherwise. M, in this case is directly self-defeating. Though we succeed perfectly in doing what M requires, we secure less of what M acknowledges to be of value. The flaw is in M rather than in us. Were we in a position to choose basic motivations for ourselves and all others, we do better to instill a motivation to be guided by concern for overall well-being, to move from M closer toward C.

This conclusion seems to me to be questionable. Parfit misconstrues, I believe, the dominant thrust of M. We are, for the most part, *permitted* to lend extra concern to specially regarded persons rather than *required* to do so. And, to the extent that M recognizes a strict obligation in this respect, it is almost always the case that things *will* go less well if individuals generally fail to acknowledge that obligation. Would the goods that we attain through relations to our children and friends be as readily available if we were to change our belief that we owe them special consideration? In some possible worlds, perhaps. But Parfit has not persuaded me that such is the case for the actual world. Also, his characterization of M as self-effacing with respect to C seems reversible. Suppose that you and I come to realize that we will do better by our children if each of us is guided by C. Does this provide us any reason to attempt to alter our volitional makeup? Only by some such consideration as, "By acting to change myself and others I shall make things better for the child who *matters* to me." My decision to be guided by C is based on my prior acceptance of M as a true indicator of where lies value for me.

No bird's eye view of the argument of Part One can convey an adequate sense of the charm of the scenery closer to the ground. Parfit is a better analyst than most, but it is the freewheeling play of his imaginative powers and the daring with which he throws himself into the densest philosophical thickets that excite admiration and delight. In the Anglo-American philosophical world, only Nozick is comparable. Dozens (perhaps hundreds—I have not attempted to count) of artfully constructed examples punctuate the text.³ Usually they illustrate with grace and economy the philosophical issue in question. Occasionally though Parfit is done in by excessive reliance on this talent. For example, he argues that it is rational to take into account even extremely minute probabilities when the stakes are very high. Designers of nuclear reactors should not ignore probabilities as low as one in a million of component failure when the result of such failure will be catastrophe. Similarly, one has good reason to vote in American national elections even if the probability that one's vote will be decisive is on the order of one in a hundred million. The cost to the individual of voting is less than the product of the net average benefit to each American of the superior candidate being elected, the number of Americans, and the probability that one's own vote will be decisive.

If the analogy holds, Parfit has untangled a major problem of normative democratic voting theory. It does not. Nuclear failure is a bad for virtually everyone affected and bad for each in nearly the same way. Political goods and bads are notoriously less clear-cut, and predictions about what candidates will do if elected are extremely tenuous. If a voter guesses incorrectly, he "cancels out" the vote of someone who has guessed better. Therefore, consequentialist rationality obliges one to apply to one's cost-benefit calculations an "epistemic rate of discount." In particular, one does better *not to vote* if one's Political Judgment Quotient ranks below the median of all

prospective voters. If each prospective voter applied this consideration and could be confident that all others would do so, the outcome that emerges as optimal is not *everyone* voting but only *one person* voting—the Philosopher King redivivus! This may seem too quick to the reader, and indeed it is. But it identifies more of the relevant parameters than does *Reasons and Persons*. The moral is that even a uniquely gifted philosophical fabulist must not overly burden his muse.

A notable corollary of the investigation of collectively self-defeating action is Parfit's demonstration of the ungainliness of ordinary moral reasoning when applied to situations in which one's actions affect each of many other people to a negligible degree. A million polluters collectively impose severe harms on the populace, but no one polluter imposes a perceptible harm on anyone. Parfit argues persuasively that our moral principles have largely emerged in response to the experience of readily identifiable harms and benefits being visited on readily identifiable individuals. They do not transfer easily to large number cases. Yet the importance of large number cases under conditions of contemporary urban life is profound. Unless we radically revise our moral architecture, the results are liable to be very bad. Parfit contends that our moral reasoning must give weight to imperceptible effects of action and to what people *together bring about*. While I have qualms concerning some of the specifics of the diagnosis, the discussion undeniably enriches moral inquiry.

II

In Part Two, Parfit resumes the attack on S. S mandates temporal neutrality; a person acts rationally only if he gives equal weight to every state of his life. Against this Parfit presents what he calls the Present-aim theory, P. According to P, one does best to act to achieve one's present desires. P can take various forms, and the one Parfit commends is the Critical Present-aim theory, CP. According to CP, one does best to act to achieve those of one's present desires that are intrinsically rational. There is an uninteresting way in which CP can collapse into S: one's dominant present aim may be that things go as well as possible for oneself over the course of a lifetime. Another possibility is that it is rationally obligatory to value every period of one's life. This version of CP is not, however, equivalent to S. One may rationally value some periods above others, and one may value things other than one's own well-being. For example, I may be willing to sacrifice on behalf of my friends or my projects. If so, I cannot thereby be condemned as irrational.

The S theorist has a difficult time contending against CP. He can point out that one who always acts to advance his present desires can bring it about that future desires will be less well satisfied. On each occasion one succeeds in advancing present desires, but they are less well satisfied in toto than if one had adopted S as one's policy. That is to say, CP is indirectly self-defeating. But, as we have previously seen, so too is S. If that does not constitute a decisive objection to S, then neither does it disable CP. The S theorist can maintain that to lend weight to only present desires is rational only if those are the only desires that will be one's own. Since one will come to have other desires, one cannot rationally decline to give them weight in the present. But if the complaint is that partiality toward the present is irrational, how can partiality toward oneself be justifiable? Not all desires are *present* desires, but neither are all desires *my* desires. S stands uneasily be-

tween CP and C. By artfully playing the two strings to his bow, Parfit unmercifully harries S. What may have initially seemed to be the obviously correct account of practical rationality emerges battered and bruised.

Has Parfit successfully solved Sidgwick's problem? I believe that he has. However, I do not believe that the problem was ever as deep as Sidgwick took it to be. When the S theorist maintains, "For each person, there is one supremely rational ultimate aim: that his life go, for him, as well as possible," what is the force of the qualifier "for him?" It may mean that I am not to count the outcome of a project of mine as successful unless the beneficiary is me. The only ends at which one rationally can aim are self-referential. So understood, S is palpably false. *Of course* I am rationally entitled to aim at goods that are not uniquely goods for me. (That is why a desire-fulfillment account of practical rationality is more plausible on the face of it than a narrowly hedonistic theory.) Alternatively, the qualifier may be otiose. On this construal, one does well to lead a life in which one acts to achieve what one takes to be valuable, whether or not that value resides within the confines of the self. For example, I can succeed in living a life devoted to the production of beauty even though it is not myself that I render beautiful. So interpreted, S becomes difficult to reject. But then there is no occasion to reject S; it is compatible with any credible theory of what constitutes a well-lived life. The crucial ethical questions become: (1) What lives are well-lived for beings such as ourselves?; and (2) What principles ought we acknowledge as applicable if we are to live such lives? These remain difficult issues of the utmost importance, but what need no longer detain us is the arid question of how to square S with morality.

Parfit apparently agrees. He says:

[C]onsider artists, composers, architects, writers, or creators of any other kind. These people may strongly want their creation to be as good as possible. Their strongest desire may be to produce a masterpiece, in paint, music, stone, or words. And scientists or philosophers, may strongly want to make some fundamental discovery, or intellectual advance. These desires are no less rational than the bias in one's own favour. (p. 133)

However, his major assault on S is waged not in terms such as these but through the consideration of conundrums involving temporal location. S dictates temporal neutrality. Does that mean that I should care as much about events that *have happened* as I do about events that *will happen*? Suppose that I have been given a drug that affects memory, and I cannot remember whether a painful operation I must undergo has already occurred or whether I will experience the agony today. Should I be neutral between these two possibilities? Or suppose that I have now totally and irrevocably lost some desire that consumed me over much of my earlier life. Am I rationally required to lend weight to its fulfillment proportionate to its duration and intensity? Am I required to assign it *any* weight in current deliberations over what I shall do? The obvious answer is that there is a complete asymmetry between the past, on the one hand, and the present and future on the other. It will be difficult for the S theorist to account for the asymmetry in other than a question-begging manner. Moreover, the asymmetry seems to affect the first-person perspective in a way strikingly different from the third-

person perspective. Suppose I have been informed that my terminally ill mother will suffer great pain for several days before she dies. I am disconcerted by the prospect of her future misery. Then I receive a corrected report; her suffering and death have already occurred. Coming to find out that *her* pain is in the past does not bring the relief from distress that the corresponding discovery about *my* paid did. Through these and a host of related constructions, Parfit raises (and illuminates) many puzzles about time, the concept of prudence, and related issues. I do not mean to suggest anything other than the greatest admiration for this section of the book when I say that it is only of secondary importance in its avowed aim of relaxing this grip on us of S.

III

No one has contributed more to the contemporary discussion of personal identity than Parfit. Previous investigators had divided on whether criteria of identity are best to be thought of in terms of physical or psychological continuities. Parfit has shown that both approaches are vulnerable in essentially equivalent ways. Continuity, whether physical or psychological, is a matter of degree, while identity is all-or-nothing. That is, I may share many or few memories and character traits with some future person; the brain of that future person may have been constructed as a replica of mine or may possess any percentage from 0 to 100 of the cells that are now in my brain. But that future person either is me or is someone other than me. If it is me, then I have survived. If it is merely someone closely resembling me, then I am defunct. Between my survival and my demise there is all the difference in the world. Or so we commonly believe. Parfit's major point is that this is a mistake. Identity is all-or-nothing, but what properly matters to us are all matters of degree. Philosophers can, of course, work at developing acceptable criteria for identity over time, as does Parfit himself. But they should not suppose that they are thereby investigating that which is most important about ourselves.

Those familiar with Parfit's previous work on personal identity will find much to admire in these pages but little that surprises. Fission and fusion of persons, split-brain phenomena, teletransportation, and transplants: all the old cast are back and put through their paces again. Interesting new responses to discussions by Bernard Williams and Thomas Nagel are included, but most of this section replicates earlier essays. It is perhaps fitting that an author who argues that psychological connectedness and continuity are what properly matter to persons has constructed an essay so connected to what he has offered before! This is not intended as criticism; Parfit has established a research program of major importance that he here advances with inexorable thoroughness and energy.

It is likely that considerations of *Reasons and Persons* will concentrate on this section. If so, that will be unfortunate. The metaphysics of personhood merits further attention but so too do the numerous other lines of inquiry opened up by this book. Above all, this is a pathbreaking work in *moral philosophy*, and recognition of it as such should not be deflected by the undeniable charms of its metaphysical excursions. Parfit explicitly places

his discussion of personal identity in the context of what we should care about and how we should act to give effect to that care. He deserves to be taken at his word.

Parfit describes his position as *Reductionist*. Identity is constituted by relations of psychological and physical connectedness and continuity. (State A is *connected* to state B to the extent that they share psychological or physical components. If A is connected to B and B to C, then A is *continuous* with C.) That is, connectedness is not a transitive relationship, while continuity is.) Of the two, psychological relationships are of much greater importance than physical ones.⁴ There is no "deep further fact," such as a Cartesian ego, in which identity resides. A corollary of this position is that those questions of great practical import that we characteristically phrase in terms of identity are more usefully considered in terms of the constitutive factors of continuity and, especially, connectedness. What I properly have reason to be concerned about is whether, at some future time, there will exist some person connected or continuous with me, not whether that individual will be numerically identical with me. Whether moral obligations engendered by promises and contracts remain in force depends not on whether the promiser survives but on whether there exists a person psychologically connected to a sufficiently close degree with the promiser. Relations to future stages of oneself should be viewed more on the model of relations to other persons. So, for example, paternalistic intervention is moved from the purely self-regarding realm, into which moral considerations are forbidden entry, and placed within the domain in which principles function that mandate permissible treatment toward other persons. The abortion debate becomes recast once we take the important relationship between fetuses and subsequent persons as a matter of degree rather than identity. And so on.

I shall refrain from discussing the many problem cases Parfit constructs on the way to his reductionist conclusion. Surely they will receive extensive examination elsewhere. Even if some details turn out to be in need of reformulation, it is unlikely that reductionism itself will run into much opposition. Cartesian egos are not, after all, much in fashion these days. And if they were, Parfit's steamroller would have sufficed to lay the ghost to rest. Instead, I wish to briefly question whether Parfit has gotten the normative implications right. If he has, one simple conclusion follows: moral philosophy must be rebuilt from the ground up. At least within the Western liberal tradition, the divide between distinct persons is taken to be fundamental. Propositions about rights and justice rest on it. If what separates numerically distinct persons is instead relatively shallow or, more precisely, if it is approximately as deep/shallow as what separates temporally distant stages of one individual's life, then most of what we say about justice, about rights, about respect for persons is insupportable. Secular ethics minus robustly distinct individuals is as impoverished as theological ethics without God.

There are two ways in which one can take personal identity over time to be a deep fact, a fact that undergirds ethics. First, one can believe identity to be a metaphysical given. The world just breaks down into separate persons who remain the selfsame beings from birth (or conception) to death. This metaphysical fact has numerous significant practical implications, and it is the job of normative ethics to spell out what they are. Parfit has, I shall assume, shown this approach to be unsustainable.

Second, one can, as it were, begin where Parfit leaves off. Identity as an

externally conferred fact is shallow. It is not congruent with that which we have reason to value. The concept of personal identity that matters to the metaphysician turns out not to matter for the ethicist. However, the concept of personal identity that figures prominently in developmental psychology and philosophical anthropology is of the first importance. It is the identity which individuals forge over time through their attachment to special concerns and projects that subsequently have directive force over their lives, that determines what can and cannot figure as potential items of value for them. That robust identity in this sense will obtain is not guaranteed by an external blank check signed by Descartes's God. Individuals, though, have an interest in constructing through their own activities lives that are coherent. Identity so construed is in the *active* rather than *passive* mode. And it matters a great deal. Or rather, it matters a great deal if anything does. For suppose that what holds out abiding value to me is end E. Then I have a fundamental interest in acting to realize E and, thus, in constructing a life coherently regulated by the pursuit of E. My success presupposes continuance as an active being whose identity is a function of his directive concerns. Continuance as a Cartesian cipher is irrelevant.

Much follows from this. Individuals who have reason to value the construction of a life coherent in virtue of the persistence of projects have reason to value the liberty to do so. Requirements of noninterference will be grounded in a fundamental separateness of persons that is not passively conferred but actively developed. Acceptable moral principles will recognize that each person has reason to be partial to the ends that are distinctively his own. Rights that secure to each person a privileged moral space within which his own will is sovereign manifest this recognition. Paternalism is suspect not because future selves are assured of being closely connected to present ones but for nearly the opposite reason. I may be the chief creator or the shape that my future self takes or this job may be taken out of my hands by would-be benefactors. Even if the benefaction is genuine, my interest in constructing *from the inside* the life that is mine has been abridged.

The two preceding paragraphs are perhaps too schematic and abridged to be persuasive. Certainly, a much expanded treatment is called for if we are adequately to assay what it is about the identity of persons that is important for moral philosophy. Still, I hope to have rendered it at least credible that traditional ethics very nicely survives Parfit's reductionism about persons. Actually, I believe a stronger result to obtain. Parfit sometimes presents his account of persons as above all threatening to classical liberalism. Collectivist principles, he suggests, are to be substituted for individualistic ones. Here, I think, Parfit badly misappraises the significance of his own work. Classical liberal views not only can survive on a diet of Parfitian reductionism but, in fact, are better nourished than ever they could be on a fare of Cartesian egos whose identity over time is a "deep fact." A sort of division of labor is to be observed; because metaphysics cannot read much *out* of the identity of persons, practical philosophy reads much *into* it.

I cannot quite leave the matter there because Parfit does not. His "official" view is that reductionism is revisionary of fundamental moral attitudes. (It also strikes yet another blow against S.) But in the closing pages of the book he notes:

On the Non-Reductionist View, the deep unity of each life is automatically ensured, however randomly, short-sightedly, and

passively this life is lived. On the Reductionist View, the unity of our lives is a matter of degree, and is something that we can affect. We may want our lives to have a greater unity, in the way that an artist may want to create a unified work. And we can *give* our lives greater unity, in ways that express or fulfil our particular values and beliefs.

It may appear then that there is no substantive disagreement between Parfit and myself, simply a difference in how widely each of us will employ the term "identity." That is not so; there remains a wide disparity between our appraisals of the implications of reductionism for moral theory. Parfit's revisionistic conclusions are resistable, and in the cited passage, he takes the crucial first step toward doing so. It is indicative of his extraordinarily wide vision and philosophical *fairness* that he offers an observation potentially undermining of theses he has labored to bring forth and defend. I suspect that, in his subsequent work, he will move yet further from the official position.

IV

How we act affects the welfare of persons. How we act also affects which persons there will be. The former proposition has been at the forefront of ethical theory forever; the latter has hardly been noticed. Section IV goes a long way toward righting the balance. If conceptions were to occur between different partners, or more than a few days later or earlier than they in fact do, then the resulting persons would be different persons. Major social or economic policies influence who meets whom, whether a couple conceives a child, and when they do so. Thus they affect which people there will be. The point, once stated, is obvious enough. To the best of my knowledge though, prior to 1970 its implications were entirely unexplored.⁵ And that is a surprising omission because, as Parfit unmistakably demonstrates, the implications are anything but trivial.

Suppose that a couple could conceive a child this month who, predictably, would be severely handicapped, though with a life worth leading. They could instead wait another month to conceive, in which case their child would enjoy normal prospects. Should they do the former, it seems to be a choice that is patently wrong. Yet to whom has the wrong been done? Not to themselves, for even if they have acted imprudently, that's not the mistake we wish to spotlight. Neither is it a wrong done to the child who comes to be, for *that* child is benefited by their decision to conceive earlier. It would not exist, and thus enjoy the life we acknowledge to be worth living, had they waited. Finally, it is metaphysically confused to locate the wrong in the child who would have existed had they conceived one month later. We may want the scope of our moral principles to be generous, but to extend them to nonexistent beings is profligate. Either we withdraw the claim that their act was wrong, or else we recognize it as an act that is not a wrong *to* anyone. But what principle or principles do we invoke to justify that kind of judgment?

Similar problems arise at the macro level. Suppose that the social choice is between two different schedules of depletion of natural resources. One is *steady-state*, in which each generation will predictably enjoy a stock of

resources equal to that of its predecessor, technological advance generating new resource availability at roughly the same rate at which old ones are expended. The other policy is *rapid depletion*. For three centuries resources will be expended at a rate that maximizes current economic prosperity. People live happier lives than they do under steady-state. However, at the end of three centuries quality of life will plummet, though not below a point at which it becomes not worth living. Which policy ought to be adopted? Before a response is hazarded, it should be noted that any decision about economic policy will significantly affect patterns of conceptions. It is almost certain that, if rapid depletion is adopted, there will not exist, three hundred years later, any individual who would have existed had steady-state been adopted. People will be worse off, but they will be *different* people. No one will be able to complain that the choice of policy has rendered him worse off. On the other hand, if the choice is steady-state a large number of people will be worse off than they might have been. That will be the case for all members of the current generation and diminishing numbers from subsequent generations. If we apply only person-regarding principles of choice, rapid depletion wins. Yet that seems to be the wrong result.

Such considerations are the takeoff point for Parfit's investigations. The goal is the discovery of a set of impersonal moral principles, Theory X, that yields some reasonable conclusions and no absurd ones. Modest enough, one might think, yet the quest turns out to be exceedingly difficult. Suppose that one adopts the classical utilitarian principle of maximizing total happiness. Then we should reject a world containing ten billion very happy people in favor of bringing about a world containing a vastly greater number of people each of whom leads a life barely above the threshold at which life is worth living. Maximizing average happiness does no better, for then we would be required to bring about a world containing only a handful of ecstasies rather than one in which billions of people lead very fulfilling lives. An intermediary position is that quality and quantity both matter, but that quantity matters less and less as the numbers grow. (Actually Parfit identifies several distinct variants of the intermediate position.) This receives a good run for its money but is found to lead to other unacceptable results. One interesting reason why it does so is because there exists an asymmetry between happiness and unhappiness. The intermediate position takes it to be a matter of near-indifference whether the world brought into being contains a very large number of very happy people or a world just like it except twice as populous. Our intuitions do not rise in opposition. Yet it is not a matter of indifference whether we act to bring about a world of many beings who lead lives of excruciating agony or instead a world just like it except with twice as many people suffering. The value of increments to the quantity of happiness asymptotically approaches zero, but the disvalue of incremental suffering does not.

It is not possible to chart here the many byways that follow. In this section, as elsewhere, Parfit aims at nothing less than examining *all* feasible alternative views. If he does not succeed, that is because *no one* could, not because of any discernible philosophical omission on his part. Still, the quest fails. Each reshaping of principles yields paradoxical results of its own or else can be shown to entail results that have previously been dismissed. Theory X remains elusive. It may be altogether unobtainable. That will be the case if we demand consistency with our pretheoretic intuitions, but those

intuitions cannot themselves be consistently expressed. There may be some comfort for the theorist in this; the fault will not be in him but in the stars (that deceptively twinkle in people's minds). Unfortunately, this is not a dilemma purely of theoretical interest. Parfit is absolutely persuasive in arguing that we possess unprecedented power profoundly to affect the well-being of future generations. This is a genuine novelty in human history. We do not enjoy the luxury of being at liberty to confine our moral speculations to the range of cases familiar since Aristotle. New potentialities call for theoretical rearmament. What lies ahead of us is vastly more important than the totality of what lies behind: "The Earth will remain habitable for at least another billion years. Civilization began only a few thousand years ago. If we do not destroy mankind, these few thousand years may be only a tiny fraction of the whole of civilized human history." (pp. 453-454) Parfit has not found Theory X but, almost certainly, he has opened up what is bound to become a major area of moral inquiry.

V

What constitutes the unifying thread of this massive work? Parfit tells us, "My two subjects are reasons and persons. I have argued that, in various ways, our reasons for acting should become *more impersonal* (p. 443, emphasis Parfit's). This accurately characterizes much of the text, most especially Part Four. The move from S to C is from personal to impersonal reasons but, as Parfit himself notes (p. 445), CP is, in some ways, *more personal* than S. I have already discussed the extent to which Parfit's reductionism about personal identity entails impersonalism. If I am correct, reductionism is hospitable to recognition of personal reasons. Parfit, in one of his voices, seems to agree. So Parfit's own characterization of the upshot of his text is, at least in part, misleading. Does this matter?

Not very much I think. Tight thematic unity is the primary virtue of some books. Even a narrow range of ideas has some considerable effect if they are kept carefully in line. *Reasons and Persons* though is bursting with ideas. Philosophical discussion of each of the major areas addressed by Parfit will be transformed in the wake of this book. Even some peripheral forays make important additions to the relevant literature, e.g., the treatment of prisoners' dilemmas. Rarely in contemporary philosophy has such breadth of ambition been harnessed to such depth of achievement. That is probably too cautious; without much danger I could remove the qualifier, "contemporary." I noted in section I Parfit's inheritance of concerns from Sidgwick. He explicitly voices his great admiration of Sidgwick and, though he nowhere says so, it is clear that he wants to write a book that can stand shoulder to shoulder with *Methods of Ethics*. I think he has done so. *Reasons and Persons* lacks the full systematic coherence of the earlier book but compensates in virtue of its imaginative richness and the unconstrained delight in doing philosophy that springs from the pages.

I do not wish to be perceived as a *total* enthusiast. (But why not?) This is not a book without flaws. Parfit is quick to conjure illuminating examples but, as noted above, occasionally an excess of confidence in his way with an analogy leads him astray. And beyond citation of particular instances, a question that goes insufficiently examined here is that of the proper place of

fanciful thought experiments within philosophy. Parfit offers some useful justificatory remarks but does not consider whether science fiction scenarios might be of far greater utility in metaphysics than in moral philosophy. The metaphysician's eye ranges over all possible worlds; the moral philosopher has a difficult enough time prescribing for the actual world and its close neighbors. Given concerns rooted in *practice*, it need not be an objection to a moral theory that it stands mute in the face of people fissioning and fusing, that it fails to tell us how we ought to act if each person's intentions were transparent to all others, and the like.

Despite its analytical precision, the book gives the appearance of having been rushed. Some thoughts appear to be second thoughts, not fitting comfortably with the official position Parfit espouses. Further pruning might have eliminated some tensions in the text. This though may not be fair to Parfit. The difficulty may be less too quick a rush to publication than a laudable unwillingness on the part of the author to stem the onrush of ideas. Some philosophers make a deliberate policy of sequestering their arguments from all considerations that might render them uncomfortable. Parfit is a philosopher both honest and forthcoming. It is hard to imagine him engaging in philosophical protectionism. Less controversially, one wishes that this otherwise handsomely printed volume had received more attentive proof-reading. The sentence running between pages 39 and 40 is hopelessly garbled; one or more lines are missing on page 193; a middle paragraph on page 331 is unintelligible; the footnote numbered "2" on page 448 should be "1"; footnote 6 on page 523 is incoherent. A number of other typographical missteps less damaging to the readability of the text are also present. These are quibbles, but when philosophy is done so well, one does not want even one sentence mutilated in the passage from author to publisher.

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1. See "Personal Identity," *Philosophical Review* 82 (1971): 3-27; "On 'The Importance of Self-Identity'," *Journal of Philosophy* 68 (1971): 683-690.
2. Neoclassical economic theory characteristically makes it a matter of definition that S provides the correct account of rationality.
3. In an autobiographical aside (p. 157), Parfit mentions that between the ages of 7 and 24 what he most wanted was to be a poet. Despite his declaration to the contrary, I am not convinced that he subsequently changed his mind.
4. Parfit is prepared to admit (p. 284) that this may not be so for very beautiful people!
5. Parfit raises the issue in "On Doing the Best for Our Children," in *Ethics and Population*, ed. Michael Bayles (Cambridge: Schenkman, 1976), pp. 100-115. It received independent statement in a different context in Robert M. Adams, "Existence, Self-interest, and the Problem of Evil," *Nous* 13 (1979): 53-65.

Goods and Virtues. By Michael Slote. New York: Clarendon Press. 1983.

Michael Slote's *Goods and Virtues* does not offer a synoptic treatment of the virtues or of personal goods, but instead proposes a "corrective" to certain widely disseminated treatments of those topics. Surprisingly, his "fine-grained approach" makes no mention of Alasdair MacIntyre's work.

Slote avers that a good deal of philosophical writing on goods and the virtues is *a priori*, and as so restrictive, falls short of actual moral experience. Instead, he proposes a more worldly and realistic view of moral phenomena. Ultimately, he grounds his own views on "reflective commonsense judgements of value and ideas of the self and human life." (p. 13) Nonetheless, some of Slote's conclusions seem highly counter to such a perspective, as in his recognition of sadism and heroin addiction as possible personal goods: "the goodness of sadistic and addictive enjoyments may be obscured by a partial but perhaps inevitable other-minds problem. It may be our own limitations . . . that make it difficult for us to acknowledge the goodness of what sadists and addicts enjoy." (p. 129)

However, if such "limitations" are inherent in our "reflective commonsense judgements," then one wonders how reliable is recourse to "ordinary moral thinking?" And if we need to dispel such "limitations," then why recommend, as Slote does, an appeal to such indeterminate, pre-philosophical "everyday thinking?"

Moreover, inasmuch as Slote eschews a holistic accounting of goods and virtues—instead relying on "ordinary moral thinking" and a kind of pretheoretic "everyday thinking"—a critic can also appeal to such a framework to counter some of Slote's philosophic conclusions which seem amuck with this kind of grounding. And if Slote were to claim that such counterresponses were against the grain of "ordinary moral thinking," then he would need to provide some sort of theoretical support to show how that is the case. As far as I can ascertain, he doesn't.

Slote defends the temporal aspects of virtues and personal goods, holding that the very temporal occurrence of a personal good can determine that good's efficacy on a person's life; and the view that certain life periods are more important than others. If correct, Slote would claim to have shown that there is not always an internal relationship between virtue and the

good, and that reasons for action are not forceful transtemporally, with the result that temporal egalitarianism (à la Thomas Nagel) is mistaken. For Slote, not all periods of a person's life are equally relevant in providing reasons for action.

Slote also critiques the all-inclusiveness of rational "life-planfulness." He believes that having a general overall life plan can be genuinely counterproductive at times, since some basic goods are not always reasonable as goals within a life plan *per se*. For example: he asserts that life-planfulness is an anti-virtue in childhood.

Slote contends that even in a person's prime-of-life span, there are certain dependent goods and virtues that are such only given the presence of other more absolute goods or virtues. For instance, *conscientiousness* is a virtue only in the company of basic human decency. In marriage, *mutual trust* is a value only in the presence of mutual fidelity; and sex is a good only in the context of love. And the secularized excellence of *humility* is an "unspecifically dependent virtue," supervenient upon the presence of other desirable traits. Indeed, Slote holds "all the virtues of total societies are specifically and unilaterally dependent upon justice." (p. 71) The offshoot of all of this is that traditional thinking on intrinsic/instrumental goods needs to be rethought.

However, despite his views on the relativity and dependence of certain goods and virtues, Slote claims to be an ethical objectivist. "Relative virtues need not entail virtue-relativism." (p. 39) Since, for Slote, personal human goods are the result of basic needs or desires, they are not dependent on subjective choice or belief *per se*. They may not be absolute, but it hardly follows that they are subjective.

Also, for Slote, moral considerations and ideals of excellence are not as *overriding* as philosophers have traditionally held. Slote contends that there is no set, ideal moral perspective that demarcates the range of what can be virtuous or a personal good. He defends the thesis of "admirable immorality," wherein certain character traits that tend to wrongful action can be yet regarded as virtuous, even exclusive of any utilitarian justification. The virtuous life need not be self-denying, for such goods as wealth, power, and pleasure can be in one's best interest. He critiques the view of John McDowell, *et al.* that the virtuous individual has no reason for pursuing such (otherwise advantageous) goods if contrary to the requirements of morality; and that the virtuous individual in forgoing such personal goods suffers no loss or advantage.

Slote is anti-utilitarian, and throughout his book he seeks to emphasize the importance of time preference in the determination of virtue and personal good. He writes: "within a very wide range, the facts of childhood simply don't enter with any great weight into our estimation of the (relative) goodness of total lives." (p. 14) However, *pace* Slote, the situation of a seemingly content and successful, middle-aged, communist, totalitarian bureaucrat, who was raised as a youth only on Marxist indoctrination, would seem to offer a poignant counter to Slote's claim. Moreover, Slote somewhat inconsistently seems to agree, *mutatis mutandis*, when he writes "those who yield to, and succeed under, such pressure can hardly help being emotionally scarred by it as well!" (p. 47, note 9) Here we have an instance of the adage "if youth only knew, if old age only could" with a vengeance. Slote's views

are at loggerheads with the moral phenomenology of many middle-aged persons who are still (metaphorically) sitting on their biological, cultural, or ideological parents' laps.

In comparing childhood misfortunes or successes to *dreams* (and finding a close logical linkage therein), Slote overlooks how dreams rarely affect our total (or even daily) waking lives, but clearly this is not the case with traumatic childhood/adolescent events. Nonetheless, Slote believes "an unhappy schoolboy career" followed by "happy mature years" is such that the latter "wipes the slate clean." This belief leads Slote to rank as superior the pleasures of anticipation over the pleasures of memory. However, *pace* Slote, many people are so constituted that given certain (bad) childhood/adolescent experiences, they are left scarred with not just painful memories but also stamped holding only the sorrows of anticipation. Their earlier and later lives, however unpleasant, are intertwined in a metaphysical unity that makes it difficult to speak of them as creatively forging a personal identity.

In addition, Slote holds that later success can compensate for earlier failure, but not vice versa. But this contention seems false, say in the case of a precocious mathematician who achieves early success, only to spend most of his "prime of life" career in the academic backwoods. Here, perhaps, earlier achievements can counterbalance later disappointments.

As previously said, Slote contends that rational life-planfulness is relative to certain periods of life, and is an anti-virtue in childhood and adolescence. Surprisingly, he illustrates the (alleged) anti-virtue of rational life-planfulness by the case of a tenure-track woman academic, who is delicately balancing her career with her marriage. Slote here recommends a type of passivity as she weighs what to do should she not be granted tenure. However, Slote speaks of *trustingness* as a child-relative virtue, but a decided anti-virtue for adults. But in recommending that the woman academic shun life-planfulness (as a nonapplicable "period-relative virtue") in favor of the *passivity* found in trustingness, Slote rather unwittingly seems to suggest that prime of lifehood (for her) surfaces only with tenure!

Again, some of Slote's (allegedly) commonsense evaluative claims seem highly suspect. He writes: "I cannot think of any example of childhood *prudence* that does not immediately seem odd, inappropriate, even pathological." (p. 49) (To my mind, the difference, for Slote, between prudence and practical wisdom is unclear. Despite this opacity, he wants to hold that wisdom is "always and essentially" a virtue, but prudence is a nonabsolute virtue.) *Pace* Slote, I can think of many such examples, i.e., learning various educational skills, watching out for one's health, striving for economic stability, forging moral autonomy, and so on. And one might also take issue with his claim that many basic goods of life are not under the control of our wills, such as intelligence, friendship, and love. To recommend a sort of passivity here—even for adults—as Slote does, strikes me as both counterproductive and antirational.

Slote darkly claims that childhood-relative *personal goods* do not transpose into adult anti-goods, but childhood-relative virtues do just that. Innocence and trustingness are appropriate for the child but not the adult, just as for the adult life-planfulness and prudence are excellences, but not for the child (whether the latter are vices is somewhat unclear).

However, the cardinal virtues are not, for Slote, time-relative, and neither is patience. But, given the time-relativity that Slote attributes to many virtues, he seems to suggest that if the world were constituted differently, some virtues would not be proper excellences *tout court*. That is, if persons had no temptations, temperance would not be needed; and given that virtues function as *correctives*, then their usefulness seems to depend on a kind of cosmic luck or worldly happenstance. But all of this renders Slote's distinction between relative and absolute virtues highly tenuous. That is, his possible-worlds ontology of goods and virtues, would appear to undercut his posture of ethical objectivity.

Consider the moral phenomena involved in sexual pursuit. Many people typically believe (at least by ordinary moral consensus) that the young have to sow their oats, and many consider it not imprudent or intemperant that pre-prime of lifers sexually involve themselves with sundry partners, so that when they decide to settle down, they can be fully committed to the right person. Yet few people share a similar view regarding middle-aged (married or not) persons, who act accordingly. Why the asymmetry? Suppose, to complicate matters somewhat, those middle-aged persons never sowed their wild oats before, but committed to their "first love" at an early age. To view the promiscuity of youth as admirable (im)morality, but similar conduct in prime of life as nonadmirably immoral suggest that Slote's thesis of relative virtues even affects the cardinal virtues (which he denies). One might note here that even to reject the asymmetry would be to castigate the *absoluteness* of the cardinal virtues; in this case, temperance or fortitude.

Perhaps Slote's most-contentious theme is that of "admirable immorality," wherein he tries to show that moral considerations are not always overriding, when there are admirable but immoral traits of character. He principally illustrates admirable immorality by the case of the artist Paul Gauguin. (Winston Churchill's single-minded passion to secure an allied victory is also an instance of admirable immorality.) As is well known, Gauguin deserted his family to paint in the South Seas, driven by his passionate devotion to aesthetics. Admirable immorality is also found in the problem of "dirty hands," where a person practices torture to learn certain vital pieces of information, a practice that looks more "moralific" as terrorism spreads.

But was Gauguin an admirable immoralist? Slote warns: "We also don't want the person passionately devoted to (his) art to overestimate his own talent. Otherwise, his single-minded behaviour will seem more an expression of pathetic delusion, or megalomania, than of admirable devotion to an artistic project." (p. 103. note 25) This caveat raises the issue as to whether, prior to the final result, Gauguin's behavior was really nonadmirable; and in the end wasn't he just plain lucky? And, *contra* Slote, why couldn't a utilitarian justification be given of Gauguin's successful results, as his passion brought about a publicly, impersonal project that benefited humanity? Or, from a different ethical perspective, it might be argued that the excellence of self-esteem requires that a person know his or her limitations as well as his or her abilities. And in Gauguin's case, as in similar cases, this in turn presupposes the virtue of wisdom. So, if Gauguin correctly perceived his situation, all things considered, he was really wise, and hence not admirably immoral.

Although Slote considers the case of Kierkegaard's "teleological suspen-

sion of the ethical” that dealt with the famous Abraham/Isaac problematic, he is forced to conclude that “there may indeed be no answer” as to whether Kierkegaard was an exponent of admirable immorality. Surprisingly, Slote doesn’t raise the issue as to whether Christ was such an exponent, as when Luke reports to him (*Luke* 14:26) saying: “If anyone comes to me and does not hate his father and mother, wife and children, brothers and sisters, even his own life, he cannot be a disciple of mine.” (Also relevant here is *Matthew* 10:34-36: “you must not think that I have come to bring peace to the earth. . . . I have come to set a man against his father, a daughter against her mother”) I don’t wish to pursue the idea that Christ was a harbinger of Slote’s thesis, for such biblical passages are probably best read with a figural interpretation that emasculates talk of “hatred” and “swords,” as in the analogical tradition that emphasizes love of God as primary and all else as secondary.

To be sure, virtue theories are not generally designed to offer decision procedures for moral quandaries, tending to emphasize instead long-term character assessment rather than discrete moral judgment. While Slote strives to heuristically unpack the complexity of virtues and personal goods, he seems, nonetheless, to underestimate the distinct possibility that there may be no correct moral (or admirably immoral) solutions in many cases of normative assessment. That is, his intriguing scenarios of admirable immorality might instead be utilized as correctives to complacent, smug, moral rationalism. They show how ineffectual not just virtue theory, but other moral perspectives—rights-based or duties-based—may prove.

Consider, for example, this intractable moral dilemma. An only child (now middle-aged) takes care of his septuagenarian mother, who while not terminally ill, is beset with various ailments of old age and very much set in her ways. If the son proposes any constructive solutions to his mother’s various infirmities, he will upset the mother (given her idiosyncratic personality) and worsen her condition thereby (by increasing her blood pressure, etc.); and if he doesn’t propose any helpful solutions, he will fail to assist her and as a result be a delinquent son. Hence he will either worsen her condition or do a moral evil by remaining silent. In either case, his action or inaction is harmful to the mother. Such cases offer no happy (moral) solution, regardless of one’s ethical framework. Of course, appeal to the deontic maxim “ought implies can” may be the answer here. But, even then, we would have a posture of admirable *amorality*, not admirable immorality.

Regarding Slote’s analysis of Walzer’s *les mains sales* torturer case, could not an act-utilitarian justification there be given, with the result that the “overridingness” thesis is not denied—at least from a consequentialist perspective?

Perhaps a more plausible candidate for admirable immorality, not used by Slote, would be that of a Catholic priest who is bound by the secrecy of the confessional. And suppose a penitent confesses guilt for committing several (unsolved) murders. Is it moralistic of the priest not to report the penitent to the legal authorities? Is the priest’s forbearance here a genuine instance of admirable immorality? Clearly there is a (rule) utilitarian justification available for the sacrament of penance and the necessity (inherent in it) not to report the confessant. But I think many people would find the priest not admirable (including some Catholics) in failing to report the criminal. And if we do

believe the priest to be admirably immoral here, it may be due to our failure to realize that the priest is not really immoral after all, for I suspect a priest in such trying circumstances absolves the penitent's sins only on condition that the penitent do an appropriate penance. And the penance here, so very jesuitically, might be to confess his crimes to the police. That is, no *confession*, no genuine confession (i.e., sacrament of reconciliation).

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Kenneth Burke's Dramatism and Popular Arts. By C. Ronald Kimberling. Bowling Green, Ohio: Bowling Green State University Popular Press. 1982

In *Kenneth Burke's Dramatism and Popular Arts*, C. Ronald Kimberling seeks to reform criticism of the popular arts (also called popular culture). He calls to his aid a critical theory—Kenneth Burke's—that he claims can account for elements of the popular arts that many other theories cannot. But he is not simply substituting one theory for another, for Kimberling seeks to permit the critic of the popular arts to address not only the elements of formula and response to formula, not only the consumerism associated with the various media that convey the popular arts, but to address the content of the art works themselves and the intentions of their authors, as well as the efforts of their audiences in receiving, understanding, and interpreting these art works. This part of his program is most welcome. Anyone who is willing to look at TV, movies, or so-called formula fiction as if they were something other than commodities has my vote.

But Kimberling's book is more than just a brief for the popular arts. He defines his audience as "serious" critics and asks a key question: "How does one demonstrate to them—in terms they can relate to—that popular art is worth investigating?" (p. 11) Kenneth Burke furnishes the "terms they can relate to" as Kimberling seeks to explain Burke's theory, to parry the critical thrusts of the various thinkers who have written on the popular arts, and to generate a Burkean, i.e., dramatistic, reading of three different works in three different media—movies (*Jaws*); TV ("Shogun"); and print (*The Dead Zone*).

Kimberling divides the book into four sections. In the first section, devoted to the problem of authorship, he offers us a summary of relevant portions of Burke's theory, an analysis of other theorists of popular culture, and then some actual criticism—Burke's theory applied to the movie *Jaws* and the troublesome problem of collaborative authorship. In a second chapter, he offers us more Burke, more theorists, and a Burkean reading (viewing?) of the TV miniseries "Shogun" and the thorny problem it presents—form. In his third division he offers us Burke again, more critics, and a reading of Stephen King's novel *The Dead Zone*, focusing on its efforts to convince an "empirically minded audience" of the existence of the supernatural.

Burke's dramatic theory of art (it can be applied to any genre) "derives its name from the drama, where the crucial focus is upon *acts* performed by various players." (p. 15) Five terms, called the Pentad, provide for the various actions, actors, and scenes in any purposeful human scenario: Act, Scene, Agent, Purpose, and Agency. These answer (roughly) the questions: What happened? Where? Who did it? Why was it done? and How? I say "roughly" because those questions seem to confine analysis to the work itself, to the compass of the text, or the movie, or episode. But dramatism's chief strength as a theory for the analysis of popular art (as Kimberling is quick to point out) is its sensitivity to the role of the author in creating the work and the role of the audience in receiving and understanding it. Thus the Scene may not be only the fictive setting within a work, but may include elements of the author's and the audience's real-life settings. The Pentad, and Burke's whole theory, is not a set of rules to be rigidly imposed, but a heuristic, and a flexible heuristic at that. It has the advantage of outflanking almost any theory that would ignore one or more important parts of the transaction between author(s), work, and audience.

The all-important Burkean distinction between motion and action surfaces in Kimberling's discussion of Abraham Kaplan's theories of popular culture. "Kaplan distinguishes between an aesthetic 'response' to high art and an affective 'reaction' triggered by popular art." (pp. 24-25) Later, Kimberling explains, "The *reaction* mode of Kaplan would find its place, in Burkean terms, in the world of *motion*, not *action*. The world of human thought and language. . . implies action. . . (I)ndeed, any *social* activity among humans falls necessarily within the realm of action since such behavior involves symbolic transformation." (p. 70) Seen in a context of action versus motion, Kaplan's theory has its basis in mere response—motion—which places it outside the realm of human concerns. Kaplan ignores the very basis of any art—purposeful human action; Burke's theory helps Kimberling see this.

The "masscult" critics Dwight Macdonald and Ernest Van Den Haag succumb to a similar fate. Kimberling identifies their premise: "While others may see a mere correlative relationship between the rise of popular art as transmitted by the electronic media and a 'decline' in Western civilization, the masscult critics, feeling victimized, posit a cause/effect relationship between the two." (p. 21) To put the masscult theory in Burkean terms, the Act here is nothing less than the decline of the West. The Agency is the mass culture, and a Subagency is the electronic technology that disseminates the works.

The problem here, as Kimberling sees it, is that this analysis begs the question from the onset. "In their views (Macdonald and Van Den Haag), popular (or mass) art functions not as Scene, as one might ordinarily expect, but as Agency." (p. 21) If, however, we see the art as Scene rather than Agency, its ominous portents fall away. Suddenly, Agency is freed to be assumed by individuals rather than a faceless mass. We are in the realm of action rather than motion, of thinking creators and audiences with free will. To undermine them, Kimberling first casts the theories of Macdonald and Van Den Haag in Burkean terms, then corrects them by juxtaposing a "correct" Burkean identification of the elements of the Pentad. This could have been more convincing—Kimberling might have argued for his view rather than simply placing it next to the other theorists' inadequate explanation of how popular art works.

His conclusion about Marshall McLuhan's theory is attractive. Instead of a hulking "masscult" acting as Agency, McLuhan's theory ("the medium is the message") posits individual media as Agencies. But as in the previous critics' scheme, the audience can do little but respond in various "response modes." "Dramatism reveals McLuhan as a closet behaviorist!" (p. 22) This, too, could have been spelled out much more carefully. My sense is that Kimberling is right, but this needed much more careful attention and analysis from a Burkean perspective.

Herbert J. Gans falls short because his model "reduce(s) the overall Scene from one wherein multiple Acts of communication and response occur to one focusing solely on response." (p. 24) Gans defined various audiences, called taste publics, to identify the kinds of art they consume. "The main problem with Gans' model is its failure to go beyond the Act of consumption. The Act performed by the artist, producing the work of art, is completely ignored." (p. 23) What dramatism shows Kimberling is the lacunae in the theories, the places where a given consideration of popular art falls short of doing justice to the complicated interchange that communication always involves: "Overall we find that Gans' model is limited in scope, reducing the overall Scene from one where multiple Acts of communication and response occur to one focusing solely on response." (p. 24) Throughout his discussion of the various critics, Kimberling merely sketches, where he should provide a detailed blueprint. In a book with such large ambitions, the mere 108 pages he writes seem too often inadequate.

But what does any of this have to do with a given work of art? To find out, I examine the last of Kimberling's critical test cases, his dramaturgical reading of Stephen King's *The Dead Zone*. The central problem in King's novel, Kimberling says, is "(T)o make the macabre more credible in the eyes of those who grew up believing in the scientific method. . . . In Dramaturgical terms, the challenge that King faces is one of building audience identification with a protagonist who has supernatural powers." (pp. 84-85) "Identification" is a Burkean term, and Kimberling explains Burke's "Hierarchy of Response," a four-part structure: "pure" response to form, physical in nature (the response, e.g., to sheer repetition); "personal identification with the patterns of experience symbolized in a work of art"; "conventional response" (that is, response to artistic conventions); and "'dynamic' response, where the audience encounters patterns or characters alien to their own experience." (pp. 71-73) But having defined this hierarchy, Kimberling virtually abandons it as he lists the elements in the book that create the audience's identification with the protagonist's paranormal powers.

After demonstrating convincingly (although not through Burke) that the audience does indeed come to abandon its skepticism and embrace the paranormal as it is manifested in the protagonist, Kimberling finally returns to the hierarchy when he examines the last, "highest" form of response to art—the "dynamic." The protagonist, Johnny, has foreseen that a man will be elected president who will lead the nation into nuclear war. Faced with this evil and faced with his guilt at not having done enough when he foresaw lesser, though fatal, disasters, Johnny decides that nothing short of assassination will stop this evil but charismatic politician from winning office and, eventually, destroying the world. For Kimberling the audience's conversion to a belief in psychic powers is a "conventional" response,

because it is a response to a convention of this particular genre. "However," he continues, "for the audience to assent to the appropriateness of Johnny's assassination attempt, there must be a 'deeper' involvement, a *dynamic* involvement with the value conflicts raised by the work. To this extent, *The Dead Zone* provides a serious challenge to Abraham Kaplan's distinction between 'reactions' and 'responses.'" (p. 91)

I am sympathetic to this claim that the novel asks readers to do more than just react. But I am not sure that Kimberling has grounded his claim for this response in the best possible way. "Dynamic" response requires that the audience encounter "patterns or characters alien to their own experience." Kimberling claims that this "alien" element is moral. The moral question the book asks is the "Hitler question": Is assassination justified in the face of evil of almost unimaginable proportions? Or, more concretely and personally, Would you have killed Hitler if you had had the opportunity? Substitute for Hitler the politician who will lead us into nuclear war, and we have the question Johnny asked himself. He decided the answer was "Yes," but we see him reason his way slowly to this decision, thus inviting the reader to reach it with him. But surely, this change in the reader's value system is not nearly so large as the change (however temporary) in his epistemological beliefs—where he is willing to believe, at least for the duration of the novel, that paranormal powers exist. Kimberling's application of Burke's hierarchy leads him to label this identification with the paranormal as merely "conventional," while the challenge to our moral system is "dynamic." Devaluing the conventional hides the power of formula fiction and blinds us to the important work that formulas do.

But this shortcoming is understandable. Almost everyone who works with popular art seeks to demonstrate that it, like so-called high art, has ideas, not just formulas. Kimberling's analysis of King's novel—and *Jaws* and "Shogun" as well—places these works in a light that illuminates their art rather than the bottom lines of their creators' bank accounts. Kimberling redirects our attention to the transaction the creators conduct with the reader/viewer rather than typifying the audience as a group of slavering consumers incapable of thought. His desire to "raise" *The Dead Zone* to the highest level of identification is particularly understandable when we remember his audience—critics. Yet it is in satisfying this audience that the book falls short. They will ask why Burke was invoked, only to be left behind in the analyses. They will ask how Burke's theory, which seems so protean in Kimberling's hands, does anything more than answer the sympathetic critic's whims in writing what could be viewed as an apology for popular art. He has the right approach, and perhaps, the right theorist. At least Burke insists on seeing action where others see only motion. Thus Kimberling's intentions—to elevate criticism of the popular arts from the level of mere sociology—are laudable. But to convince his chosen audience of critics—the most-entrenched elitists ever produced—he needed more: more explanation of Burke, more integration of Burke's theory with the practical criticism he offers, and more arguments for the superiority of this way of doing things over business as usual.

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Without Foundations: Justification in Political Theory. By Don Herzog. Ithaca and London: Cornell University Press. 1985.

Don Herzog is in the tradition of William Saroyan, who made fun of a certain species of deep thinker by putting into the microcosmic bar room of *The Time of Your Life* a character with only one line, repeatedly muttered into his beer: "No foundations! No foundations—all the way down!"

Who, outside the building trades, needs foundations? What good do they do? Well, geometry has "foundations" (yes, it is a metaphor): the "solid," "unshakable" axioms that will "bear the weight" of the theorems "constructed" "on" them. Philosophers impressed by the skeptic-proof status of mathematics have long sought all-purpose foundations for the rest of knowledge. The quest began before the days of Socrates, but in modern thought the great foundations man was Descartes. In no conceivable circumstances could it be false to say or think "I am, I exist." A few more clear and distinct ideas, like the existence of God, nailed to this slab and up in safety rise the great edifices of Science and Theology.

Many subsequent thinkers, suspicious of Cartesian structural engineering, have nevertheless sought other foundation materials—typically the immediate deliverances of consciousness, "hard data," as Russell called them. And the winds of doctrine have buffeted them in their turns. But in the latter half of the present century the whole business has been brought into question. Long ago Stephen Pepper suggested that if we have to have a metaphorical description for the development of knowledge, why not try talking in terms of the emergence of a clearly perceived landscape as the sun rises and the morning fog is dissipated? Foundational theory of knowledge has rather suddenly gone out of fashion, Austin, Wittgenstein, Quine, and others having questioned the existence of a class of propositions the members of which are intrinsically certain and exempt from revision.

Herzog aspires to banish foundationalism from political theory also. He views Thomas Hobbes, John Locke, and the Utilitarians as having attempted to do political geometry, an enterprise bound to fail. David Hume and Adam Smith, who eschewed foundations, did the job right. (Later the author admits that the contrast is not stark, neither Hume nor Smith having been "wholly immune" to foundationalism. "Still, each one develops powerful justificatory arguments rooted in social contexts, arguments with no foundations." p. 161)

Two-thirds of the book is devoted to contemplating the rubble of the foundations that Hobbes, Locke, Bentham, and company purported to lay down, and now and again to take another swing with the wrecking ball. Hobbes (Herzog maintains) has three arguments for why we are obligated to obey the Sovereign: (1) From prudence: disobedience tends to put us back into the intolerable state of nature; (2) From necessity: irresistible force obliges; (3) From ordinary language: it follows from the meanings of such terms as ought, right, duty, and justice that opposition to the sovereign is wrong. Herzog holds that (3) is empty, and (2) is odd, so Hobbes's case really boils down to (1); and that won't do either, since as a matter of fact it is not true that all men desire peace; some really like war.

Locke's justificationism is no more effective, being based on contract, which is unhistorical; or tacit consent, which is (as Hume pointed out) hollow in view of the practically insurmountable difficulties of emigrating. Nor can Locke explain why consent is supposed to oblige.

John Stuart Mill, Herzog holds, was not a utilitarian at all but a self-realizationist without realizing it, consequently his philosophy was "untidy." Real utilitarianism, which is "any theory holding that the average or total happiness of the group ought to be maximized," is unsatisfactory, since it demands calculations that are never made and in fact can't be made. "Utilitarianism, for all its vaunted precision, cannot tell us what to do." (p. 157) Moreover, it makes society into a mystical whole, and it "purges information," i.e., requires us on principle to ignore relevant features and differences of experiences, such as their qualitative aspects and the identities of their subjects. He concedes, however, that this philosophy has attractive features: it eschews metaphysical and theological commitments, making human welfare the point of morality; "it offers (or seems to offer) reasons for its conclusions"; "it allows facts to affect and even determine our moral principles"; and "finally, it is in some ways a perfectly egalitarian theory" (p. 159). (Readers of *Reason Papers* will note the cloven hoof emerging from under Professor Herzog's academic gown.)

Herzog's refutations, which are very long, are in my opinion of unequal force: effective against Bentham's utilitarianism (and Sidgwick, Harsanyi, Brandt, and Hare come off no better), less telling against Hobbes and Locke. But I forgo rebuttals because I think there is a more fundamental (you should pardon the expression) trouble with the structure of the Herzogian argument against foundations of political theory.

The three-centuries-long ascendancy of foundations in theory of knowledge should bring home to us what a tenacious grip a metaphor can have. I am afraid that Herzog provides yet another illustration of this truth. Underlying his approach is an unexamined assumption that some of the classical political theorists have argued from foundations in exactly the same sense as theorists of knowledge have done. I shall try to show that this assumption is questionable, at least as it concerns Hobbes and Locke.

Herzog characterizes a foundationalist argument as "grounded on principles that are (1) undeniable and immune to revision and (2) located outside society and politics." (p. 20) But excepting *reductio ad absurdum*, every argument proceeds from premises that the arguer regards as true or at least worthy of acceptance (the etymology of "axiom") by the person being addressed, to a conclusion supposed to be entailed by the premises. So if there

is a contrast between foundationalism and some other mode of argumentative justification, it cannot consist merely in the purported degree of reliability or immunity to question of premises. As for being "located outside society and politics," this metaphor is, as the author admits or rather vaunts, "more suggestive than sharply defined." (p. 21)

In theory of knowledge, the literal homeland of foundationalism, the distinction is this: a foundationalist epistemology is one holding that nothing can qualify as an item of knowledge unless it is either a preferred item or derivable from a set of preferred items according to an approved procedure. The theory tells what kinds of items are preferred and what procedures are approved. In Platonism they are respectively Ideas and "seeing with the mind's eye"; in Cartesianism, clear and distinct ideas and painstaking inferences; in Hume's philosophy, "impressions" and the validation of concepts by tracing them back thereto; in Logical Positivism, protocol sentences and logical construction. A nonfoundationalist epistemology is one such as Popper's or Quine's which lacks a specified set of preferred items, in other words one in which no purported bit of knowledge is intrinsically *hors critique*.

This dichotomy cannot be literally applied to political theory, which is concerned with the right/wrong family of distinctions rather than (primarily) true/false. There is, however, a natural analogy: we may call a political theory foundationalist if it is one according to which evaluations (principles, norms, recommendations, policies, imperatives, value judgments, or what have you) are valid (warranted, true, right, approved, . . .) just in case they are either preferred evaluations or derived by an approved procedure from preferred evaluations, the theory specifying what are to count as preferred evaluations and approved procedures.

In these doubly metaphorical terms Benthamite Utilitarianism is certainly a foundationalist theory. There is a single preferred principle: maximize happiness; and a procedure for deriving specific policies: calculate a felicific index for the consequences of each of the practicable alternative actions and implement the one that comes out highest. It is a telling criticism of the theory to show that the procedure is incapable of being carried out; though this is not to score a point against foundationalism as such, only against utilitarianism.

It is not clear that Hobbes and Locke were foundationalists in the sense I have defined. What is the Hobbesian preferred evaluation? Herzog takes it to be the supreme desirability of peace. Since *Seek Peace, and follow it* is the first Law of Nature, we might suppose that the Laws of Nature have the preferred role in Hobbesianism. And they are indeed held up as Precepts of Reason. Nevertheless, they are dependent on context and recognized to be so by Hobbes; otherwise he would not have troubled himself to show that the seeking of peace, etc., are what Reason requires of us *given the facts about human nature*. If men were not competitive, diffident, and vain-glorious, the advice to seek peace and follow it would be pointless. And rational creatures free of these nasty propensities are not logical impossibilities. Moreover, even given human nature as it is, if conditions were such that the unrestricted indulgence of competition, diffidence, and glory would have no effect other than to add zest to an already sociable, rich, sanitary, refined, and long life—and this too is logically possible—Reason

would revise her priorities. Hobbes showed awareness of these points by his explicit refusal to infer any need for a world sovereign to put an end to the war of every state with every state, on the ground that by making war kings “uphold . . . the Industry of their Subjects” and do *not* produce misery (*sic!* *Leviathan*, chap. 13).

Thus (as far as I can see) Hobbes was a foundationalist only in the “rough” (Herzog’s word, p. 21) sense that he subscribed to principles held to be *de facto* universally applicable in human affairs because they express an invariable human nature—which is no more than Hume did, as Herzog recognizes (and deplors, p. 171, implying that anyone who purports to have “latched onto true and invariant human nature” is a “pigheaded doctrinaire”). But Hume is a Herzogian paradigm nonfoundationalist.

The case for a foundationalist Locke is even more dubious. No candidates at all present themselves for the role of preferred evaluation. Locke’s expressions can be misleading, but it is fairly clear, at least to me, that the apparatus of Contract is brought in not to serve as foundation piers so much as to provide a model for political obligation, which (*pace* Filmer) is to be regarded not as a blank check but as limited to terms spelled out in advance, like the rights and duties created by an ordinary lease or hiring agreement.

Thus Herzog does not make out a general case against foundationalism, nor does he claim to—perhaps out of anxiety not to lapse into laying down foundations to end all foundations. Instead, he offers three cases of foundationalism that don’t work, in contrast to two no-foundations theories that do work, and invites us to make an induction. But, unless I am mistaken, two of his three examples of the wrong way to go at political theory really exemplify his approved method, namely, to show that what you recommend is better than the alternatives available in the circumstances. Hobbes argued that *any* government is better than “the alternative,” which is always anarchy; and Locke tried to convince his readers of the advantages of limited monarchy.

I believe, however, that many of Herzog’s substantive criticisms of the alleged foundationalist theories are right (if sometimes a bit picky and overstated), and that Hume and Smith deserve the praises he lavishes on them. I think, moreover, that he is right about there being no future for foundationalism. And why shouldn’t someone—Herzog in his next book?—attempt a general refutation of the position? All that needs doing is to show, as with the epistemological analogue, that there is no reason to believe in the existence of any preferred evaluations.

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Intermediate Man. By John Lachs. Indianapolis: Hackett. 1981.

John Lachs is a philosopher (Vanderbilt University) and a poet (*The Tides of Time*) whose latest book, *Intermediate Man*, more properly belongs on the shelf of sociology than of poetry or philosophy. Addressing the modern phenomenon known as "alienation," Lachs observes that the concept is vague, ambiguous, and regrettably judgmental. In its stead, he proposes the terms "mediation" and "psychic distance." A mediated action is an action performed for one person by another. The necessary loss of immediacy (direct connection between ourselves and our actions) produced by mediation is called "psychic distance." Lachs argues that from the sharp focus and broad perspective of these two concepts, we can understand the fundamental ills of modern life, untangle the snarls of our complex society, and perhaps, ultimately, unburden ourselves of the emptiness and frustration characteristic of our times. It is a bold and ambitious pursuit, the kind that is bound to offend nearly everyone at some point along the way, the kind necessarily given to generalizations and oversimplifications for its sheer magnitude. One thing for sure, it cannot be done in 145 pages.

Nevertheless, the book is worth reading. It is rich in content: observations on the nature of government, education, parenthood, responsibility, psychology, drugs, the mass media, language. It is stylistically attractive: personal in tone, with clear, direct, and vivid statements. Indeed, at times Lachs waxes poetic, though now and then he comes dangerously close to sermonizing, typical of those who speak as from a vision. Always, one senses, Lachs writes from the heart, even when the reins to his enviably fine mind get slack or tangled. It's as though the poet got better of the philosopher in just those places where we needed the philosopher the most.

In a nutshell, Lachs's position is this: mediation is inherent to, and very nearly synonymous with, society. From mediation we derive enormous benefits: comfort, leisure, efficiency, and productivity, all the advances made possible through specialization and the division of labor. But the costs are equally great. The larger and more complex the society, the longer the chains of mediation, the more we depend on others to perform actions for us. And the longer the chains, the greater the psychic distance produced until, as is the case today, individuals feel impotent, lost, and confused. We lose touch with the world, and "things happen" that affect us significantly, yet

seem beyond our control, influence, and worst of all, beyond our understanding.

Several chapters are devoted toward demonstrating the pervasiveness of mediation. The use of tools, for example, makes it “possible for us to act without coming into direct physical contact with what we act on . . .” (p. 25) Echoing Ralph Waldo Emerson, Lachs observes that what tools offer in efficiency and power, they exact from us in immediacy. More generally, the notion of progress (that stimulating force which drives an industrial society) tends to make us future- rather than present-oriented, and renders what Aristotle called “activities” (ends in themselves) simply means to an end. We move and act for the sake of some distant goal. So there is danger lurking in the very soul of progress.

Even language itself, that instrument which connects our separate minds, stands between us and the world, between us and others. “The person to whom I attempt to convey my feelings gets but a pallid copy of what I live and breathe.” (p. 53) One suspects that Lachs’s own poetry is more vivid and compelling than this quote suggests, but there you have it: Language itself is a mediating force, and where there is mediation, so too is there gain and loss.

Furthermore, we are surrounded by institutions—corporations, the legal system, government, religion—which we tend to view as real agents rather than as abstractions representing individuals. Such a view, which is a byproduct of mediation, diminishes our sense of power and blurs our vision as we seek to place responsibility when things go wrong.

Perhaps the greatest mediator of all is government, and Lachs spends a good deal of time discussing the role of government in our lives. The goal of decentralization and deregulation, of breaking up the immense power of government is, according to Lachs, a noble but futile and misguided ideal. Noble, because it springs from the heart of freedom, but futile and misguided because federal and local governments are inextricably connected, omnipresent, and provide the uniform rules and predictable enforcement that are necessary and inevitable for any society:

Municipal and state government simply do not have the scope to cope with the corporation and the labor union. Big government is not an incidental growth or the product of conspiracy. A rich country with a large population invites broad commerce and big business. Big government is required to create safe conditions for big business, and also to control it. (p. 93)

Enforcement and uniformity of rules are clearly exemplified in totalitarian states, but totalitarianism is hardly justified thereby. Lachs skirts the fundamental and crucial issues here and altogether fails to draw the distinctions and make the analysis that his claims require. Lachs assumes that commerce (big or otherwise?) requires external control, and, furthermore, that it is government’s proper function to provide the control. Neither of these assumptions is necessarily true, as any libertarian economist or philosopher could demonstrate. At any rate, Lachs is much too quick here. He tells us that “Big government must be accepted as a necessary force.” (p. 93) If by “big government” Lachs means to include among other things, government

control of commerce, then he is simply humming the old liberal tune. Indeed, he continues with a plea for the humanizing of government. Big government "must be turned into a medium for self-determination, into an instrument of freedom, not oppression." (p. 93) This even sounds like a politician talking. And who on Capitol Hill would argue with these sentiments?

There is no disputing Lachs's talk of the pervasiveness of mediation, and it does seem almost intuitively clear that mediation would tend toward psychic distance (though it isn't as evident as Lachs seems to believe that "our" lives are hollow and unfulfilled). We can even allow that the claim that government is not "out to get us," is, in fact, benign in intent. Given all this, the solutions that Lachs offers in his last chapter come to little more than an eloquent (he writes beautifully) pep talk. Proper education is the answer. We must integrate education with daily life, make it more practical and relevant. Use the community as a lab. Educate the senses and emotions. Develop the body as well as the mind. "The democratic process and the educational process coincide in their aims. Both focus on the development of personality and through that on the intelligent self-determination of the community." (p. 143) Hence, our leaders must also be our teachers, educating the public "by means of laws." (p.143) Individually, we can convert many of the things we do into activities valuable in themselves; "we can strive for wholeness by refusing to be defined and exhausted in our roles" (p. 129); we can examine the acts we perform, trace out their consequences, and see the extent of our participation; we can do the same of others' actions, "so that all of us in mediated chains become more ready to take responsibility for our acts." (p. 129)

What all of this lacks in genuine information or in hard, detailed analysis and argumentation, it nearly makes up for in style and inspiration. *Intermediate Man* has the flavor of those popular paperbacks from one or another psychologist who has a scheme for getting us back on the right track. One tends to agree with the drift, and, at times, even to get excited; but in the end, though perhaps uplifted, one is not permanently moved. Inspiration is definitely a necessary condition for solving life's problems, but it is far from sufficient.

It would be unfair and misleading to end this review on the above note. Given the brevity of Lachs's book, it is to his credit that he accomplishes as much as he does. He offers an interesting perspective on the condition of modern man and develops this in a way that invites reader response and that promises to sustain serious and lengthy discussion. In short, the merits of *Intermediate Man* far outweigh the flaws, and the reader is urged to give the book a try.

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