

# REASON PAPERS

*A Journal of Interdisciplinary Normative Studies*

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## COMMUNITY WITHOUT COERCION

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

### HEGEL'S ANALYSIS OF CIVIL SOCIETY

The problem with capitalism, as perceived by Hegelians, is this: if society is to be legitimate, there has to be "universality"; in other words, a sense of citizenship, of people identifying with the state. But in capitalism, or as Hegel called it, civil society,<sup>1</sup> 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,



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

For Hegel, as Michael Oakeshott has remarked, poverty was “the counterpart of modern wealth rather than a sign of personal inadequacy.”<sup>5</sup> 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.<sup>6</sup>

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.<sup>7</sup> 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.<sup>8</sup>

## THE HISTORICAL ARGUMENT

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



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?<sup>13</sup>

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."<sup>14</sup>)

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





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.<sup>16</sup>

## 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



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.<sup>18</sup>

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.<sup>19</sup> 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-



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."<sup>21</sup>

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.<sup>22</sup> 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



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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**I**n 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.”<sup>1</sup>

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.”<sup>2</sup> 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



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.<sup>5</sup> 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.<sup>6</sup>

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.<sup>7</sup> 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,



Weber's value-skepticism, far from being "healthy," amounts to outright value-nihilism.<sup>11</sup> 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.<sup>12</sup>

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."<sup>13</sup> 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.<sup>14</sup> It does not—no more



one answer—that we ought to philosophize.<sup>18</sup> (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.)<sup>19</sup> 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.<sup>20</sup> 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.<sup>21</sup>

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





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.



## 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.<sup>30</sup> 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.<sup>31</sup> 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



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.<sup>34</sup> 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.<sup>35</sup> 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 mistake<sup>36</sup> 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).<sup>37</sup>



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





# 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



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.<sup>4</sup> 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.<sup>5</sup>

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.<sup>6</sup> 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.<sup>7</sup> 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.<sup>8</sup> 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



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.<sup>14</sup>

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.



(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:<sup>19</sup>

(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.





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.<sup>22</sup>

This passage suggests the following:<sup>23</sup>

*(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.<sup>24</sup>

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."<sup>25</sup> The term "the realism of" does not appear just before "its assumptions"



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.<sup>27</sup> 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.<sup>28</sup> 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.<sup>29</sup>

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



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.<sup>32</sup> 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.<sup>33</sup>

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



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.<sup>36</sup>

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.<sup>37</sup> 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





















































































































