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The Myth of Universalism: Theories of Science and Theories of Justice

Nancy Cartwright and Marco Del Seta
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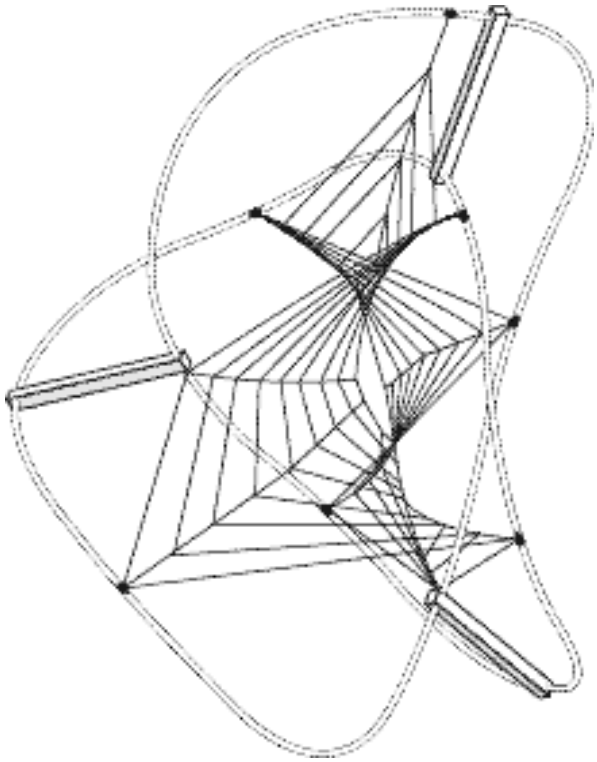

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The Myth of Universalism: Theories of Science and Theories of Justice

Nancy Cartwright
and
Marco Del Seta

Department of Philosophy, Logic and Scientific Method and Centre for the Philosophy of Natural and Social Sciences,
London School of Economics and Political Science

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Abstract

Theories of Justice have traditionally sought for self-standing universal principles to settle questions of fairness. Similarly theories of science have formulated universal canons of method to secure scientific objectivity. In recent years, as pluralism has gained importance, both fields have been questioning their traditional commitment to overarching principles. This talk will discuss the parallel developments in these two different areas of negotiation models as the basis for judgments of fairness and of scientific objectivity.

The material in this paper arises partly from discussions at a conference on negotiation models in science and in politics sponsored by the LSE Centre for Global Governance, which took place on May 6, 1994.

We would also like to acknowledge Francesco Guala and John Worrall for their help with examples.

1 Some Illustrations of Particularism

Vaclav Havel in a short discussion on *Politics and the World Itself* reports on the topic of this lecture: "The world today", he tells us, "is a world in which generality, objectivity, and universality are in crisis" [**?**, p.12]. We should note the three features that Havel mentions, features that come together as one phrase: generality, objectivity and universality. Their disunion in contemporary theory will be one of our central themes. Havel's topic was politics. Our studies focus on science. The discourse on disunion is the same in both. Surprisingly we can now take whole chapters from political theory and read them without tripping into the middle of texts from philosophy of science.

Theories of justice have traditionally sought for some self-standing universal principles to settle questions of fairness. Similarly, theories of science have formulated universal canons of method to secure scientific objectivity. In recent years, as pluralism has gained ground, both fields have been questioning this traditional commitment to overarching principles. In both areas we now find a focus on the local as opposed to the universal and a search, not for basic principles that will justify the choice among competing claims, but rather for the mechanisms by which some agreements can be achieved that all sides can live with.

The association of *the objective* with what can be grounded in general universal truths—both in science and in politics—is ancient. Most of us know it from Plato's doctrine of the cave. We are like people living in a cave. We see around us only what is fleeting, shifting, with no stable pattern, mere shadows of genuine truth, which is eternal, unchanging and universal. The doctrine had, then as now, practical consequences, as when Plato recommends against turning to cookery for alleviating physical suffering rather than to the 'rational' art of medicine.

> I declare that [cookery] is dishonourable ... and I maintain that it is merely a knack and not an art, because it has no rational account to give of the nature of the various things which it offers.[1]

But however rational medicine is by contrast with cookery, it is not genuine knowledge either, because it is not exceptionless and hence not truly universal. This doctrine from Plato, then Aristotle, was taken up by the Scholastics, who distinguished genuine knowledge, *scientia* or science, knowledge of universal truths, from *opinio*, opinion, true at best for the most part. Thus it entered our modern scientific tradition. Much changed with the empiricism of the scientific revolution but the equation of objectivity with universality remained.

The Scholastic doctrines on *scientia* are not the only ones on this topic that we inherited from the Medievals. For we also have the particularism of Duns Scotus. We can only really

[1] Plato, *Gorgias* [**?**].

know the concrete particular, not the abstract universal; and if we want to understand the behaviour of individual things in nature, be they avalanches or economies, we will have to look as much at what is special to that individual as to what it has in common with others. It is this realisation that moved the famous 19th century French physiologist, Claud Bernard, to resist the introduction of statistics into medicine and to insist instead on detailed and precise studies of the individual body; the German Historical School at the end of the century to write thick histories of individual societies rather than provide political economy with universal laws on the model of physics; and the bubble-chamber physicists of the 1960s, studying neutral currents and hunting for the W-particle, to trust the golden event rather than statistical demonstrations using computer simulations. We cite just one remark to show the attitude typical of a particularist to universal laws, from Eric Auerbach, one of the foremost philologists of the first half of this century, writing in defence of criticisms of his book *Mimesis*:

> My efforts for precision are directed towards single and concrete cases, while the general formulas *indispensable* for comparing, grouping together and defining phenomena with respect to one another were supposed to be fluid and elastic. They were supposed to adapt on a case by case basis to the capacities explicit in the single object, and must be interpreted on a case by case basis in terms of the context.[2]

To give you a sense of the particularist stand in moral philosophy, we shall cite a long portion from an interview with Stuart Hampshire, whose "negotiation model" of justice I will describe shortly. The interview records a story that Hampshire has told often, in different ways, in conversation and in seminars. Hampshire reports:

> In the closing months of the war I went with an American sergeant who spoke French and English to Angers in France. We went during a snowstorm in a jeep. There was a young man there we were to interview; he had been captured by the Free French and was their prisoner. Before being allowed to see him we gave a guarantee that we would not ask for his release into British and American custody. (The guarantee was given to the general in command of Communication Zone - *Comm Zee*).
>
> We were invited to lunch by the Free French in a restaurant. The food was good; it was very friendly and uproarious all round, the Free French there being primarily Communist. The issue was raised again about how to treat the young man. They reiterated their insistence that he could not be released into our custody. I said that this might raise difficulties when we came to ask him very specific

[2]Eric Auerbach, "Epilegomena zu Mimesis" [**?**], our translation from italian original.

questions about technical wireless procedures for their clandestine broadcasting; and this was what interested us because we had some difficulties, technical difficulties, with intercepting the broadcasts from the place where the Germans were training agents whom they were parachuting into France. This young man had been parachuted into France and would know the techniques that were used to keep in touch with their agents. And one of the French said, "I can't see what the difficulty is. You can simply say what you like to the young Fascist. It's up to you. Sometimes I think", I remember he said, this Frenchman, charming and tough, "...sometimes I think you English are not serious either in peace or in war".

So then we went to see the young man. I should explain that it was very, very cold. There was almost no food available to ordinary people and certainly a very minimum in the prison. All the food was in the hands of the resistance leaders. When we got to the prison we found the young man had chains on his feet and was in a cell which had written on the wall "Condamnè à mort". In other words he was in the condemned cell.

We entered into conversation with him. I discovered that he was a young student and he had been greatly influenced by some Fascist writers, in particular by Ramon Fernandez, who was expert on Proust and was of the extreme right. I knew of his existence and had even read an article of his about Proust. He was a well-known extreme right Fascist writer. And another even more famous and more influential was Cèline who wrote a famous novel *Voyage au bout de la nuit—Journey to the End of the Night*—and this had turned him into a Fascist, or so he said. He was thin. He looked rather like Jean-Louis Barrault, the film actor, and very typically a student. Not particularly charming but obviously very literary and full of aspirations to be a writer in the style of those who had influenced him. I can't remember exactly his age but I believe it to have been eighteen.

When we got near to the point of exactly how he was instructed to operate his wireless and what was the call sign and the method of tuning in so he could get through to his controller (I think the control was at a place called *Sigmaringen*), he said, "Well, I think I remember it all, but what is in it for me if I tell you?". And I should explain, we had talked quite a lot about literature and he had started to smile; we were getting on more or less all right. Then came the point when we had to decide whether to lie to him or not.

Now what was important is that there was a technique of interrogation used in 12th Army Group—the Americans—by a member of the Warburg family which was designed to find out what formation the pilots were likely to be flying in the next day from a captured pilot, who often could say "Well, next day they will be

flying such and such a formation". In this case there is a very quick calculation you can make that if you get the answer you may save some person's life. Therefore you might tell a lie, however outrageous, to induce the person to tell you what you want to know—a so-to-speak "short causality". You could not allow a moral scruple *you* had lead to the death of some pilot. This would be to me totally unacceptable. You ought not to be there at all if you were going to endanger people's lives by moral scruples, within limits I mean. It doesn't follow you had to torture the man in order to find out, but anything short of torture or outrage. But in this Intelligence matter you couldn't directly say that someone's life would be lost if you didn't get a means of capturing these spies who were being dropped in France.

I tell this story not to exhibit the fact, which everybody knows, that there are often situations where both lines of conduct seen unacceptable, but to illustrate a quite different point. When you tell the story you put in a certain amount of detail. I just put in detail about the setting: the Free French, the cold, the starvation, the fact that he was in the death cell, that he was a young student and that he was not a punitive personality or a tough of any kind. Now whether that has anything to do with it is what's interesting. I tell the story to you now, and I have told it on several other occasions, and each time I put in different features of the story with the idea of giving you a picture of what it was like. But what features are relevant? Who knows? Was it relevant—it *seems* to be relevant—that he wasn't a kind of Fascist tough, but quixotic—in the sense of Don Quixote—influenced by books. I put that in because it *does* seem to be relevant. I think I nearly always put it in. The remark of the Free Frenchman—"you're not serious in war or in peace"—I put in because that is a concept of seriousness which I understand. If you're in a war you can't start thinking, "Well I can't lie to a man who's going to be shot tomorrow and tell him that he isn't."

2 Universal Views of Science and Political Theory

Karl Popper has been the great spokesman for the contemporary view of science as an enterprise worthy of the labels 'rational' and 'objective'. Popper was part of the great movement for the Scientific World View in Austria, Germany and Poland in the early 1930s, before he was driven away by the Nazis, eventually to England. Popper was upset by the fact that in certain domains of inquiry the results have too much fluidity about them: anything goes. He tried to put forward a criterion which could separate out genuine science, worthy of being called objective, from irrational non-science, for him, particularly Freud, Hegel, Marx and

religion. The demarcation criterion, as it became known, equated science with testing: to be scientific, statements must be subject to empirical refutation, or *testable* by strict procedures. The demarcation criterion was to provide not only an instrument for distinguishing science from non-science but also a methodology for doing science. Not only should scientific statements be testable and refutable, but the scientists themselves should consciously attempt to produce such statements and subject them to the strictest tests in order to produce refutations. Later work, most famously by Thomas Kuhn, Gaston Bachelard and Imre Lakatos, developed considerably more detail and shifted the account somewhat, but kept intact the idea of a universal method that can distinguish what is rational and objective from what is not.

Importantly for the parallels we are pointing to between political theories and theories of how to conduct science, the insistence on proper scientific method was at its outset expected not just to build a better science but to build a better world. That is reflected in the title for the philosophical movement we just mentioned, the *Scientific World View*. Scientific method, when properly spelled out, was not just to provide the means for rationality and objectivity in the judgments of science but was to provide the means to rationality and objectivity *full stop*. In Vienna the Positivists who struggled for the Scientific World View worked hand-in-hand with the Bauhaus movement and shared their Enlightenment ideals. Some were Marxists, most were Socialists. They believed that clean lines, clear thought and strict adherence to canons of rationality and objectivity could provide a weapon to resist the evils, obfuscations and lies of the Church, established morality and, desperately, the Nazis.

Probably the most sophisticated liberal account defending a universalist attitude in politics since the '30s is *A Theory of Justice* by John Rawls. Rawls is concerned to find the principles that determine a fair or just distribution of goods. He settles on two. The first codifies rights to human freedom. The second is called the *difference principle*: no unequal distribution in society is justified unless it benefits the person worst off. That is his *substantive theory*. The important factor is where it comes from.

The central element in Rawls' scheme is the notion of procedural justice. Justice is not rooted in some formulae, in some independent standards, for judging all distributions, such as his own substantive theory embracing freedom and the difference principle, standards that are supposed to wear their correctness on their faces. It comes rather from an institution (or institutions) which is able to implement the correct *procedures for deciding what is just*. Rawls' primary concern is with pure *procedural justice*.

> [This] obtains [even] when there is no independent criterion for the right result: instead there is a correct or fair procedure such that the outcome is likewise correct or fair, whatever it is, provided that the procedure has been properly followed.[3]

[3]John Rawls, *A Theory of Justice* [**?**, p.12].

What is the correct procedure? The 'tool' Rawls selects to answer this question is the idea of the original position. This echoes Rousseau's claim "that we must always go back to a first convention"[4]. If the people are to give themselves a king or choose an electoral system they must first consider in what way they regard themselves as people. They must go back to the prior position and consider what Social Contract has been established in the first place defining them as people.

Similarly for Rawls, the original position is the situation in which we have to put ourselves if we are to be able to choose the appropriate principles of justice. The precise details of what constitutes returning to the original position, then, are crucial in justifying the choice of his two substantive principles. The idea behind the details he chooses is the time-cherished one of impartiality: we are to choose our principles behind a veil of ignorance. We shall not know our place in the social state that will result from our joint deliberations and we are to be ignorant of our own individual features too. This insures "fairness" for the principles selected, as these are picked through a choice with no vested interests. Rawls then goes on to argue that every human being, if put in the original position, will choose the two principles of justice he outlines.

There is a structural similarity we should notice between the two stories we have just told, one of belief in scientific method, the other in the hunt for the procedures to establish principles of justice; and the similarity is a clue to problems that seem to overcome the hope for universals in both cases. Neither defend at base a *substantive* theory as universal, but rather a *meta*theory or a methodology. We are not enjoined to adopt the wave theory of light over a particle theory, for instance, nor Einstein's theory of relativity over Newton's mechanics; rather we are urged to accept as universally legitimate a *method* for choosing among substantive theories. Similarly with Rawls, whose real contribution lies not in his two principles that just distributions should satisfy, but instead in the construction of a putatively universally valid method for choosing these principles. The reason for the retreat from substance to method is the same in both cases: at the level of substance it is clear that there are no self-validating claims, no theory that commands universal assent nor that uncontroversially bears within itself the reason that it should be chosen when others claim superior legitimacy. If one is to be legitimated from among the others there must be some method for choosing, and that method itself must merit universal assent. So we have theories not just of what counts as a fair distribution or what the nature of matter and energy is, but methodologies for how to choose when such theories conflict.

But these metatheories have lost ground in the last decade and a half. We have been forced to admit what we had hoped to avoid, that methods no more than content can command universal assent and that the grounds for their right to do so are equally shaky. The

[4]Jean Jacques Rousseau, *The Social Contract* [**?**, 1.V].

Positivists believed that all societies across the globe would gradually discard their traditional attachments to supernatural forces because of the need for rational, scientific and experimental methods of thought that a modern industrial economy involves. This is the old faith, widespread in the 19th and early 20th centuries, that there must be a step-by-step convergence on liberal procedures. The Positivist optimism was mistaken. History has not brought convergence and the procedures for the choice of values remain as controversial as the values themselves. Someone whose conception of good and evil is founded on a supernatural authority which represents any tolerance of a contrary moral view as evil will not accept liberal procedures of justice. And in any truly liberal society such persons are to be expected.

In science too the procedures have turned out to be empty. When they have sufficient bite to dictate a settlement they will necessarily involve a commitment to controversial scientific assumptions about how instruments work, how data is to be processed and what constitutes evidence. For example, in the early 1920s the American physicist Arthur Holly Compton confirmed the particle-like nature of light by showing that quanta of light—photons—are momentum carrying corpuscles, traveling in a specific direction, that exchange momenta with material particles in the same way that billiard balls do when they bump into each other. He did that by showing that when a beam of light with a known frequency strikes a stationary electron, the momentum imparted to the electron plus the momentum of the outgoing light beam sum to the momentum of the incoming light, using a hypothesised relation between the frequency of the light and its momentum. And he showed *that* by directing a beam of x-rays of known frequency emanating from a mobyldenum anticathode against a graphite target. And to do *that* he had to know about the characteristic x-rays emitted by molybdenum and that electrons are weakly bound to their atoms in graphite so they can scatter. And so forth. What legitimates the procedure as a fair one for testing the claims of photons to be regarded as particles depends in a precise way on the details of the experimental set up.

Or consider a case in the social sciences[5]. We choose one in economics, regrettably abstract, because the tendency to believe in an articulable universal method for deciding among competing scientific claims grows dimmer as the claims become messier and more realistic. So it is important to see that the difficulties are still there even if we focus on precise claims in high theory. A number of experiments have been performed in the last twenty years in order to test the *axiom of transitivity* of Expected Utility Theory. That is, if a is preferred to b and b to c then a is preferred to c. Most of the work was stimulated by the phenomenon of preference reversal 'discovered' in 1971 by two psychologists, Sarah Lichtenstein and Paul Slovic. Lichtenstein and Slovic predicted, and in fact claimed to observe, that the majority of people when facing a *choice* between a gamble with a high chance of winning a small sum (H), and another gamble with a low chance of winning a large sum (L), would choose the

[5]Our thanks go to Francesco Guala for suggesting this example.

former; but the same subjects, when asked to *price* these gambles, would set the low bet at a higher price than the high one.

What makes the experiments of Lichtenstein and Slovic good ones? What requirements must their courtroom meet for us to have confidence that it will provide a fair judgment on preference reversal? To insure that it does so, a lot of knowledge is required. This includes both *theoretical* knowledge and *experimental* knowledge. For instance[6]:

Incentives: are the incentives in the experiment sufficient to get people to behave as they would in real life when significant decisions have to be made?
Income effects: as people acquire more wealth they may rationally come to be willing to gamble more. This change in aversion to risk as a result of increases in wealth could contaminate the results in experiments in which many gambles are played and wealth changes between separate choices.
Indifference: are we sure that some reversals are not the result of subject's indifference among two bets?
Strategic pricing: sometimes it is rational (in the economic sense) not to say the truth when asked to state a price. It is often advantageous to ask more than one would truly be willing to accept.
Independence: are we sure that other theoretical assumptions, e.g. the independence axiom, hold in the specific experimental situation? Independence is assumed by the payoff scheme adopted by most experimenters in order to give agents incentive to state their *real* price for the gambles. The most popular payoff scheme is the *Becker-deGroot-Marschak (BDM) procedure*, and consists in running a random lottery after the subjects state their price of a gamble, and in paying the sum generated by the random process if the stated price is lower than this, in playing the gamble if it is higher. Unfortunately, if agents are not expected utility maximisers and if they do not obey the independence axiom, we cannot be sure that the price corresponds to the cash equivalent they would *really* assign to the gamble.

When the answers to all these questions are right the Lichtenstein and Slovic experiments can provide us with a method for making judgments about preference reversal. But the claim that these are the questions to ask—that the experiments must be *just so* before they can rule on preference reversal—comes not from some neutral consensus-commanding position—a view from nowhere. It comes, must come, from a view deep within a set of theoretical and practical commitments.

Now let us consider the two studies together. What do these studies have in common that is interesting, that has a real bite to it? What aspects of the procedures laid down in whatever is your favourite version of "the scientific method" that could adjudicate when the claims they are supposed to be establishing are called into question? There is by now

[6]The following are taken from Dan Hausman, *The Inexact and Separate Science of Economics* [**?**].

widespread agreement that the answer is *nothing*. The hope that there might be some universally legitimated method that could settle disputes of content—as Rawls' use of the initial position or *the veil of ignorance* was supposed to do for claims of justice—has been dashed. It is another Harvard Professor, Hilary Putnam, who has in the end reduced scientific method to truism: think clearly, look at the evidence, listen to all sides.

In the same way Stuart Hampshire, whose story of the young French captive we cited above, reduces the procedures for legitimating claims about justice and the good to a platitude[7]:

> Particular institutions, each with their specific procedures of deciding between rival conceptions of what is substantially just and fair, come and go in history. Only the one most general feature of the processes of decision is preserved as the necessary condition which qualifies a process, whatever it happens to be, to be counted an essentially just and fair one: that contrary claims are heard.

What then is wrong with metatheories for procedures of justice that have more specific content, enough content to determine decisions between competing substantive concepts of what is good or just? What, for instance, of Rawls? Hampshire argues:

> Rawls declared that his rationally chosen principles of justice must be independent of conceptions of the good. But he has in the end acknowledged that his principles are to be rationally chosen specifically by those who live in a liberal and democratic society, where they may represent an overlapping political consensus about the nature of justice. There is a harmony, but a harmony within the liberal stockade.
>
> This confinement of reasonably acceptable principles of justice to liberal and democratic societies bypasses the outstanding political problem of our time, which is the relation between, on the one hand, self-consciously traditional societies, where priests of the church, or rabbis or imams or mullahs, and other experts in the will of God maintain a single conception of the good which determines the way of life of the society as whole; and on the other hand there are the liberal democratic societies which permit, or encourage, a plurality of conceptions of the good.
>
> Liberals such as Professor Rawls and I believe that there is no great moral significance to be attached to the accident of our place of birth and of our inheritance. Our moral opponents, whom liberals sometimes call fanatics, are apt to see destiny, intention, or design in their inheritance, and from their ancestry they

[7] All the following quotes by Hampshire are taken from "Justice is Strife" [**?**].

> infer a very specific mission, a specific set of duties, and a clear design for their lives.
>
> As a liberal by philosophical conviction, I think I ought to expect to be hated, and to be found to be superficial and contemptible, by a large part of mankind. In looking for principles of minimum justice, one needs to see that one's own way of life and habits of speech and of thought, not only seem wrong to large populations, but can be repugnant in very much the same way in which alien habits of eating, or alien sexual customs, can be repugnant.

We are apt to cling to the view that it is easier in science; that disputes about procedures depend on differences about matters of fact that can ultimately be settled. Compton's experiment may not be a good procedure for adjudicating about the particle nature of light if it is mistaken about how electrons behave in graphite. But that's a matter that in principle, many believe, can be resolved to every one's satisfaction.

Consider then a case of a pure methodological dispute, still raging—the necessity for randomised clinical trials for legitimating medical treatments[8]. The ECMO case is one good example. The Bartlett group at the University of Michigan announced in 1985 that they had very good success in treating premature babies suffering from persistent pulmonary hypertension with Extra Corporeal Membranous Oxygenation (ECMO). Historically the death rate was 80%; in the sample they treated it was 80% survival under ECMO. Because of the strength of orthodoxy about the absolute necessity of clinical trials, Bartlett's group felt reluctantly forced to conduct a study of this kind. They chose a play-the-winner design: two balls in an urn—say white for ECMO, black for conventional treatment. Draw one randomly to choose the treatment for the first patient. If that treatment is successful, add a ball of that colour to the urn before the next draw; if not, add one of the other colour. Thus it becomes increasingly likely that patients later in the study will not be given the treatment that failed on previous patients. The results: the first baby was given ECMO and survived; the second received the conventional treatment and died; the next eight received ECMO and survived. At that point the protocol for the study declared ECMO the winner, but a further two patients were treated with ECMO outside the formal design and survived. So altogether twelve patients were in the study. Eleven received ECMO and survived. One, who received the conventional treatment, died.

But this kind of study was thought by many not to be good enough and a conventional, fully randomised experiment was demanded, with results at the 5% significance level—that is, toss a fair coin to decide what treatment each patient gets. The experimental scheme determined that the experiment would end when four deaths had occurred in either treatment

[8]This example is taken from the work of John Worrall on evidence-based medicine, in "Popper's Legacy" [**?**]; we would like to thank him for providing it.

arm. This study ended up with nineteen patients: nine treated with ECMO, all of whom survived; and ten with the conventional treatment, of whom four died and six survived. In an after-experiment extension, 20 further babies were treated with ECMO, nineteen of whom survived. We believe ECMO is now fully accepted in the U.S.A. as the standard treatment. But the randomized clinical trial methodology is very powerful, so to decide about use in Britain another study was set up there, though we think it has now been suspended. Those who admit other methodologies are convinced that a significant number of babies has died unnecessarily.

We see here a pure methodological dispute, and it is an intense one. We have a colleague, a cautious man, careful with argument and evidence. He has been pointing out that randomized clinical trials, as it is usually possible to conduct them, are generally not capable of delivering the appropriate levels of certainty, and that, alternatively, the use of judgment, experience, and individual observation does not automatically make a method non-objective. One famous Oxford proponent of randomized clinical trials is outraged. He finds our colleague pernicious and hopes that he will never be able to publish again. This man's great desire is to enroll all of China in one gigantic clinical trial. What are we to do in these disputes? Scientific method was supposed to adjudicate between competing claims to truth, but what is supposed to adjudicate between competing scientific methods?

Nor must we think that these kinds of problems are special to the human sciences, that they disappear when we move to hard science, like physics. The example we mentioned at the beginning, of bubble-chamber physicists looking for the single golden event, versus the crudely identified events and large statistics of spark chambers and computer simulations is a case in point. It comes from a series of studies in the history of physics by Peter Galison on how experiments end[9], how a resolution is settled upon in the physics community. In the case of neutral currents, for example, Galison tells the story of a large number of physicists at different sites—one of them in Geneva—with different experimental techniques, different initial objectives and different technical concerns. There was no adjudication among the methodologies. Each group accepted the results only when they could produce them in their own way. Even that can be misleading though. For Galison reports how different groups continue to have a quite different understanding of what is supposed to be the same result. This is particularly striking with regard to theoreticians versus experimenters. The elaborate mathematical structure that the theoretician takes to be the essence of what, say, a W particle is, does not matter to the experimenter, who would continue to believe in the very same particle even if the mathematical description shifted dramatically; and the reverse for the theoretician. On Galison's account consensus about results in the physics community depends on a broad tolerance for differing views about what those results really mean.

[9]See Galison, *How Experiments End* [**?**].

Before leaving the topic of scientific method, we would like to give one example that points in the opposite direction from our last remarks; an example where the results are only of value if a common understanding is achieved. The example concerns the photographic mapping of the heavens known as the Carte Du Ciel, or the Great Star Map[10]. Only the combined and prolonged efforts of almost a score of observatories in both the northern and southern hemispheres could produce what promoters hailed as an "imperishable monument", a photographic record of "the authentic state of the universe visible from the earth at the close of the nineteenth century". The problem with undertaking this kind of project is in co-ordinating and making uniform all the different standards for all these observatories. The debate over the kind of telescope to be used in the photographic work of the Carte Du Ciel shows how dearly uniformity and globality were sometimes purchased. Entering into this project meant relinquishing control over instruments and methods and also over research interests. Certain laboratories ended up having to give up their superior instrumentation and finer methods in the name of commensurability, like the British astronomers Common and Roberts. Others, like the Australian observatories, took eighty years to complete their work, at the cost of pursuing other, more fruitful, investigations. Launching the project and keeping it on track took one international congress in 1887 as well as five further meetings of the Permanent Committee (1889, 1891, 1896, 1900, 1909); and in 1919 a commission of the International Astronomical Union was established to oversee the development of the project. Standardised astronomical practice was reached not through easy planning, but through long negotiations. It took not just deliberate and self-conscious co-operation, but real sacrifice, not only of research careers of individuals and of institutions, but also of hard-won and highly prized standards of propriety and excellence in one's own work.

Returning at last to political theory, we should mention one of the better known works in this field that renounces universalism. In Michael Walzer's *Spheres of Justice* [**?**] the claims about equality (his particular concern in that book) are to be judged on the basis of what equality has come to mean and the role it has come to play in different particular spheres.

> Every social good or set of goods constitutes, as it were, a distributive sphere within which only certain criteria and arrangements are appropriate. Money is inappropriate in the sphere of ecclesiastical office; it is an intrusion from another sphere. And piety should make for no advantage in the marketplace, as the marketplace has commonly been understood.[11]

The argument for the existence of the spheres is, like Hampshire's—and also David Hume, who is the great early spokesman for these views—a historical and cultural one. Chapters of

[10]This example has been discussed in a series of papers by Daston (see, for instance, [**?**]).

[11]See Walzer [**?**, p.10].

Walzer's book are devoted to a discussion of a great many different contexts, and problems are posed for setting up a notion of equality in these contexts; the contexts are defined historically, by our concerns and preoccupations with them. We have money and commodities, hard work, free time, kinship and love, political power. The justification for these spheres as well as for the rules of justice pertaining to them is historical.

While this is no place to discuss the merits and demerits of Walzer's ideas, it is clear that one of the main objections to them is that they provide little advice about what should happen when we have no universal principles that command assent across spheres. This is particularly worrying when we look for a means of establishing a theory of international adjudication. An LSE colleague, Brian Barry, has argued this well[12] and we can illustrate the problem by means of an example. Suppose that nineteenth century Britain and India, because of different histories, different social conditions and different geographical characteristics develop different notions of equality to treat their problems. They are in different spheres of justice, so how can we decide how to deal with the problem of Britain exploiting India given the different standards? We may be able to say that we do not want someone who acquires power in the Britain sphere to dominate India simply because of the power he has acquired in Britain (this Walzer allows himself to do), but can we meaningfully decide claims of equality between different spheres without a universal theory of what is just?

This kind of objection is well motivated; it clearly picks out uncomfortable facts about Walzer's views. It shows up just why we should like so desperately to be able to appeal to some overriding principles that both can—and deserve to—command universal assent. And it shows why, failing such principles, the development of theories of negotiation is so critical in both philosophy of science and in political theory today. But how much can be hoped for? We shall close with the answer that Stuart Hampshire gives, an answer which is essentially the same answer as those of who worked to chart the heavens:

> There normally is in any modern society a chaos of opinion and of moral attitudes. A reasonable person knows that there is this chaos, and those with strong opinions, or with fanatical hearts, deplore the chaos and hope for a consensus: usually for a consensus in which their own opinions and attitudes are dominant. A socialist by conviction, I consider poverty alongside great wealth a great unnecessary evil and a substantial injustice, and I expect a continuing political fight with those whose conception of the good and whose idea of fairness is an incompatible one. This is the proper domain of politics.

[12] See, for instance, Brian Barry, "Spherical Justice and Global Injustice"[**?**].