

TRUTH, RATIONALITY, AND THE SITUATION

VERDAD, RACIONALIDAD Y SITUACIÓN

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RESUMEN ABSTRACT

El principio de racionalidad afirma que las personas actúan de manera adecuada según su situación, pero no especifica cómo deben actuar para hacerlo. El análisis situacional utiliza el principio de racionalidad, junto con un modelo de situación social, para explicar las acciones en el pasado. A diferencia de la Teoría de la Elección Racional, el análisis situacional no trata de predecir o influir en las acciones en el futuro. Popper consideraba al principio de racionalidad como falso, pero, no obstante, pensaba que debíamos usarlo. Esto plantea un problema para entender su punto de vista acerca de las conjeturas y refutaciones. Popper, sin embargo, pensaban que todos los modelos científicos son falsos, así que si debemos o no rechazar un modelo científico depende del problema que estamos tratando de resolver.

The Rationality Principle says that people act adequately to their situation, but does not specify how they must act in order to do so. Situational Analysis uses the Rationality Principle, together with a model of the social situation, to explain actions in the past. Unlike Rational Choice Theory, Situational Analysis does not try to predict or influence actions in the future. Popper regarded the Rationality Principle as false, but thought that we should use it nonetheless. This poses a problem for understanding his views about conjectures and refutations. Popper, however, thought that all scientific models are false, and that whether or not we should reject a model depends on the problem that we are trying to solve.

PALABRAS CLAVE KEY WORDS

Karl Popper, economía, instrumentalismo, principio de racionalidad, Teoría de la Elección Racional, modelos científicos, análisis situacional, verdad.

Karl Popper, economics, instrumentalism, rationality principle, Rational Choice Theory, scientific models, situational analysis, truth.

If my view of the social sciences and their methods is correct, then admittedly, no explanatory theory in the social sciences can be expected to be true. Nevertheless, this need not trouble an anti-instrumentalist. For he may be able to show that those methods may be very good methods, in the sense that they make it possible for us to discuss critically which of the competing theories, or models, is a better approximation to the truth.—
Karl Popper

I

The Austrian Academy of Sciences' Research Unit for Socioeconomics organized a workshop, which was held in Vienna in October of 1997¹, to discuss the impact – or lack of impact – of Karl Popper's ideas regarding situational analysis upon the social sciences. The catalyst for the workshop was the publication of *The Myth of the Framework*, a volume that includes Popper's 1963 Harvard University lecture 'Models, Instruments and Truth', in which Popper explains the idea of situational analysis that he first introduced in his *Poverty of Historicism*. I am not myself an economist or a social scientist. But I was invited to speak at the workshop as the editor of the volume in which Popper's paper appeared.

I must confess that I was somewhat surprised both by the theme of the workshop and by some of its premises. The working assumption for the workshop seemed to be that Popper had made a 'plea' for situational analysis, that this plea had been ignored, and that situational analysis was not, as a consequence, widely used in the social sciences.

Egon Matzner, one of the organizers of the workshop, articulated the problem in his background paper as follows:

In spite of Popper's forceful plea for 'situational analysis' its impact, compared to the attraction of his 'falsification criterion', was very modest. There are hardly more than a dozen articles in the specialist literature.

Matzner himself attributed this scarcity of articles to 'the fact that social analysis was equated by Popper himself with the application of the rationality principle':

¹ This is a revised version of my proposal of the workshop of Vienna.

The use neoclassical mainstream makes of the rationality principle focuses on the rather simple social situation in which *an agent maximises his/her utility under (monetary) constraints*. This is, in itself, not yet objectionable. The important point, however, is the fact that the logic of a social situation depends in almost all relevant problems on more than budget constraints and other conventional elements. Such a simple social situation misses what Popper himself describes as a social situation.

But he went on to say that the problem of the workshop was to 'inform about the status and signification of Popper's Situational Analysis in various social sciences', to explain why Popper's ideas regarding situational analysis had 'so far had almost no impact on research programs', and to determine what 'potential contribution' they 'can be expected to make'.

I was not too surprised, with this as a background, to find that many of the social scientists at the workshop had only a vague familiarity with Popper's ideas regarding situational analysis and the rationality principle. Some of them, thinking that Popper equated rationality with rational choice theory, attributed too much to the rationality principle. Others, thinking that he ignored the role played by institutions, attributed too little to the social situation. In this way, some of them criticized him and others praised him for 'blind spots' and 'insights' that in my estimation originated largely in their own imaginations.

In my view, whether or not social scientists have written on situational analysis, or have even heard of it at all, has no bearing whatsoever on whether or not it is the method of the social sciences. I have little doubt that Popper's descriptions of situational logic, social situations, and the rationality principle are oversimplifications. But oversimplification, in my view, is a large part of what Popper thought social science, and science in general, is all about. I do not mean this as a criticism. And I will, in the course of this paper, try to explain why I do not.

But I also want to address a more general problem that is raised by 'Models, Instruments, and Truth'. It is the problem of how to reconcile Popper's talk about conjecture and refutation, error-elimination, and truth as the regulative ideal of science with his acknowledgement that scientists work with theories and models that they know to be false. It is, as Popper addresses it in the context of 'Models, Instruments, and

Truth', the problem of how to distinguish his critical rationalism from instrumentalism.

II

Karl Popper used to teach that science is trial and error, and that the aim of science is to get closer and closer to the truth. This is what he meant by conjecture and refutation. And it is also what he meant by his tetradic schema:

$$P_1 \rightarrow TT \rightarrow EE \rightarrow P_2^2$$

We put forth theories in an attempt to solve our problems, and we subject those theories to criticism in an attempt to eliminate their errors. In this way, we make progress in solving our problems and, in so doing, get closer and closer to the truth. Popper, in the late fifties, offered a mathematical definition of 'verisimilitude' in an attempt to formalize his idea of getting closer and closer to the truth. And Pavel Tichy, in the mid-seventies, showed that the problem of formalizing the idea of 'verisimilitude' is not as easy to solve as Popper had initially thought.

Some people have concluded from this that the task of science is *not* to get closer and closer to the truth, but to get truth itself and to eliminate falsity *per se*. David Miller, for example, writes, *as a restatement and defense of critical rationalism*, that:

The task of empirical science, like that of other investigative disciplines, is to separate as thoroughly and efficiently as it can the true statements about the world from those that are false, and to retain the truths.

I do not, however, think that this statement about the task of empirical science is true. For suppose that there are no true universal statements about the world, but that we can determine whether any singular statement about the world is true or false. It would then follow that all universal theories about the world are false and should be eliminated from empirical science. But we might still find it preferable to work with false universal theories instead of true singular statements – and *especially* if the counter-examples to such theories are well-known –

² Here, P_1 is a problem that we want to solve, TT is a hypothesis (or theory) that we tentatively propose or put forth as a conjecture to solve it, EE is an attempt to eliminate the errors in our hypothesis (or theory) through criticism, and P_2 is a new problem that emerges as a result EE . Popper once told me that this little formula contained his entire theory of scientific method, and indeed his entire theory of rational discussion, in a nutshell.

since there are simply too many true singular statements to remember. Be this as it may, Miller's account of the task of empirical science is certainly false – if for no other reason than that there is an infinite number of true statements about the world that no empirical science has ever found interesting enough to record.

Consider the simple fact that I am here in Budapest writing this paper. This fact can be represented by a true statement: 'Notturmo is in Budapest writing a paper'. And this, no doubt, is a true statement about the world. But no empirical science that I know has found this truth interesting enough to record, let alone to separate as thoroughly and efficiently as it can from the false statement that I'm not.

There is nothing special about this particular statement. There is, on the contrary, an infinite number of true statements about the world that no empirical science would ever, or should ever, take notice.

So what has gone wrong here?

At this point, the naïve response would be that 'Science does not deal with any old truth. Science deals with *scientific* truth. The task of empirical science is not to separate the true statements about the world from the false ones. It is to separate the scientific truths from the scientific falsehoods, and to retain, once again, the scientific truths.'

This, however, would be too naïve.

For the problem lies precisely in determining which of the true statements about the world are the scientific ones and which are not.

But even were we to solve this problem, the naïve response would still be false as a restatement of *Popper's* position. Consider the rationality principle, which says that 'Each person acts adequately to the situation'. The rationality principle animates the so-called 'situational logic' that Popper said we use to explain actions and events in social science. It is, according to Popper, 'an integral part of every, or nearly every, testable social theory' (Popper, "Models, instruments" 177). But Popper thought that the rationality principle is false,³ and he also thought that social scientists should retain it despite the fact that it is false.⁴

³ Indeed, he argued that it is not *a priori* true, because it is false.

⁴ As Popper put it: "There are, as I have indicated, good reasons to believe that the rationality principle, even in my minimum formulation, is actually false, though a good approximation to truth. Thus it cannot be said that I treat it as *a priori* valid. I hold, however, that it is good policy, a good methodological device, to refrain from blaming the rationality principle for the breakdown of our theory. For we learn more if we blame our situational model. The policy of upholding the principle can thus be regarded as part of our methodology". (Popper, "Models, instruments" 177)

If what Miller said were true, then one might expect Popper to separate the rationality principle as thoroughly and efficiently as he could from the true statement that people do not always act adequately to the situation – and to retain *that* statement instead. But this is *not* what he does.

On the contrary, Popper addresses ‘the problem raised by *the known falsity of social theories*’, (Popper, “Models, instruments” 176) arguing that we should retain the rationality principle despite the fact that it is false.

I conclude from this that Miller’s statement, as a restatement of Popper’s epistemology, is false.

There is, however, a problem here. The problem, once again, is how to reconcile Popper’s falsificationism with his seemingly contradictory acknowledgement that scientists work with theories and models that they know to be false. It is, once again, the problem of how to distinguish critical rationalism from instrumentalism.

III

It is tempting to dismiss Popper’s account of the rationality principle and his talk about the known falsity of social theories as an inconsistency, and to try to explain it with two words: ‘Social Science’.

It is well-known that Popper had ambivalent feelings about social science and about its relationship to the natural sciences. He used to joke that social science began with the idea that we need a special science to get rid of our social problems – and that our greatest social problem now is how to get rid of the social scientists. And he vacillated as to whether and how the methods of the social and natural sciences differ. But there is no inconsistency here, and ‘social science’ is no explanation. Popper thought that the natural sciences also work with theories that are false and – what is more important – with theories that we *know* to be false, and *how* they are false (at least as well as we know anything at all).

Natural scientists, for example, frequently work with models. But according to Popper:

In every case in which we operate with a model, however far we may go, we are operating with a false picture of the

facts. It is a false picture of the facts because it oversimplifies the facts. So no model is really true.

Our astronomical models may represent the planets as mass-points, or the sun as an ellipsoid. But:

We actually know very well that the sun isn't really an ellipsoid, that it instead has craters and all sorts of bulges owing to the fact that it changes. We know that all sorts of things are going on there, that the sun has bulges that are not really stable, protuberances, and all sorts of things. And we know that the earth has mountains and seas, and that its possession of mountains and seas plays a certain role in connection with the theory of the tides.⁵

But now suppose that we want to explain why Slovenia was not invited to join the North Atlantic Treaty Organization in 1997. We could, like many Slovenians, say that NATO is run by madmen, and leave it at that. And if we did, then our statement may even be true. But it would not be an *explanation*. It would, on the contrary, be tantamount to saying that we cannot give an explanation.

To say that someone did something because he is a madman is to confess that we cannot really explain it at all.

This is the fundamental insight, and the methodological point, behind the rationality principle.

The rationality principle is not the *empirical* hypothesis that each person acts adequately to the situation. *That* hypothesis is clearly false. It is, on the contrary, a *methodological* principle that places restrictions upon what will and will not count as a rational explanation. It says that if we want to explain a social event rationally, then we must assume that the people in it acted adequately to the situation or, at the very least, that they acted adequately to the situation as they saw it.

Some people will say that only a madman would elevate an empirical falsehood into a methodological principle. But the rationality principle has analogues in empirical science, and even in philosophy. This is

⁵ These two passages are taken from Popper's 'Introduction to Scientific Method' course lectures, as transcribed in The Karl Popper Archives.

because its fundamental insight and methodological point pertain not so much to social science as to explanation in general.

We do not explain the perihelion of Mercury by saying that there are no general laws of planetary motion. And ironic as it may sound, we do not explain the Copernican Revolution by saying that it was a scientific revolution. We might as well say that a miracle occurred. Even if abstract universal laws did not exist, our attempts to explain natural phenomena would have to assume that they did – just as our attempt to say something that is true must assume that one of two contradictory statements is false. We can argue about what constitutes a law of nature, and about whether or not laws of nature actually exist. But to assume that laws of nature do not exist, *even if it were true*, would be to assume that natural phenomena cannot be rationally explained.

The primary task of science is not to differentiate the true from the false. It is to solve scientific problems. It is, as Popper saw it, to explain the things that we want to understand, but are not yet able to understand or explain rationally. This is what is primary. The truth or falsity of the theories that we propose as solutions to our problems pertains to this task. But the only real grip that we ever get upon the truth or falsity of our theories is through their success or failure in solving the problems for which they were created to solve. And it is clear, since we are willing to work with theories that we know to be false, that the thorough and efficient differentiation of the true from the false remains secondary to the solution of scientific problems.

IV

Popper used to say that science begins and ends with problems. He would say that we cannot really understand a theory unless we understand the problems that it is supposed to solve and the problem situation in which it was introduced. He thought that science teaching could be improved by focusing upon problems and problem situations instead of upon theories. And he proposed a new format for writing science articles that would highlight the problems that they discuss.

This is what Miller leaves out of his account of critical rationalism.

We are searching for truth, no doubt. But for truth that is interesting and pertinent to what we are trying to explain. And we may, everything else being equal, well prefer false theories that are interesting and pertinent over true theories that are not.

Truth and falsity are not themselves relative to our problems and problem situations. But our decision to work with a false theory, as opposed to eliminating it, certainly is. This is why $P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$ is an oversimplification. Whether we should work with a theory that we know to be false or eliminate our error will depend almost entirely upon our alternatives, and upon the problem that they are supposed to solve.

V

This is where models come in. Popper distinguished problems of explaining or predicting *singular events* from problems of explaining or predicting a *kind or type* of event. 'The difference between these two kinds of problems' according to Popper, 'is that the first can be solved *without constructing a model*, while the second is most easily solved *by means of constructing a model*'. (Popper, "Models, instruments" 163)⁶ A model, according to Popper, consists of certain elements placed in a typical relationship to each other, plus certain universal 'animating' laws. (Cf. Popper, "Models, instruments" 165) Models differ from theories in that theories use abstract universal laws that allow them to make statements about *singular events*, whereas models try to capture the typical aspects of a situation so as to make statements about a *kind or type of event*. Models may be called 'theories'. But real theories represent abstract universal laws, whereas models represent typical (and not necessarily actual) initial conditions. This, according to Popper, makes models especially important in the social sciences, because the 'method of explaining and predicting singular events by universal laws and initial conditions is hardly ever applicable in the theoretical social sciences'. (Popper, "Models, instruments" 165-66).

I am not sure that this is how we understand models today. Today we do use models to explain and predict singular events. And today, we are more likely to regard a model, be it in physics or in economics, as a description that attempts to capture the essential aspects of a system in a form that is simple enough for the mathematics to be solved.

⁶ Popper's italics. The difference here is not exclusive. Problems that *can* be solved without constructing a model may, nonetheless, be *most easily* solved by means of constructing a model, and *vice versa*.

One thing, however, is clear. Models are oversimplifications, and as oversimplifications, they give false descriptions of the systems that they represent.

Does this pose a problem in and of itself?

I do not think so. We often work with oversimplified rules of thumb that would soon prove disastrous were we to follow them strictly in each and every case. Paul Feyerabend thought that this refutes Popper's epistemology. But I think that it shows that Feyerabend did not really understand it. Conjecture and refutation must always be supplemented with judgments regarding problems and problem situations and what will and will not work well within them. Popper, insofar as this is concerned, used to describe his own formulations about method as oversimplifications that should not, strictly speaking, be taken as true descriptions of how science actually works, or even as prescriptions of how scientists ought to work in each and every case. But lest this be misunderstood, he would quickly add that science is in general an oversimplification, and that the issue is not whether you oversimplify but whether or not you oversimplify well.

This explains at once how Popper's critical rationalism differs from Miller's restatement. Both are oversimplifications. But Popper's oversimplification is better, since it explains what is happening, and why, when we decide to work with a theory that we know is false instead of eliminating it. It also explains why formal logic cannot capture the idea of verisimilitude. Formal logic is also an oversimplification. But since it deals with form instead of meaning, its oversimplification is not sensitive enough to distinguish falsehoods that might be interesting and pertinent to a given problem situation from those that would not. And it explains, in the end, how critical rationalism differs from instrumentalism. Instrumentalists and critical rationalists agree that we use models to solve scientific problems. But the problems that instrumentalists want to solve are primarily problems of prediction, and the problems that critical rationalists want to solve are problems of explanation. We may well believe that our explanations are false. But some explanations are closer to the truth than others. So even though we may never be able to say that our theories are true, we need not say that they are merely instruments, or tools, for making predictions.

On the contrary, it is more likely that our predictions are tools for determining which of our theories is closest to the truth.

Still, supplementing conjecture and refutation with judgments about our problem situation poses problems of its own. The main problem, if our decision to eliminate or work with a false theory depends upon our problem situation, is that our problems are not always clear while we are working on them, and may very well change as we work ourselves through them. This is what $P_1 \rightarrow TT \rightarrow EE \rightarrow P_2$ is all about. It means that we may have only a vague idea of our problem situation while we are in it. And it means that we are likely to make mistakes when we have to decide whether to eliminate or retain a theory that we think is false. I do not think that there is any way to avoid this problem. But I think that we can, by working with models, and by constructing better and better models, continually improve our understanding of our problems and problem situations.

VI

A model can be likened to a map, and a map may be more or less accurate. We may criticize and correct a map if it does not represent what we want to represent with the detail and accuracy that we need. But whether or not we will actually do so will depend upon our needs and, in particular, upon what we want to do with the map.

A map of Vienna is inaccurate if it locates the Stephansdom on the outskirts of the city instead of in the center. But such a map may be perfectly adequate if the only thing that we want to do with it is to show that the Stephansdom is in Vienna and not in Graz.

We should not expect – and I do not think that anyone really does expect – our maps to be perfectly accurate and detailed in every respect. On the contrary, a map that was perfectly accurate and detailed *in every respect* would be entirely useless as a map, if indeed we could regard it as a map at all.

Imagine a map of Vienna in which everything in Vienna – including the Stephansdom, the archbishop, and each of his altars – is represented *exactly* the way it appears in Vienna itself. This would be a dynamic map representing not only streets and buildings and airports and tram

stations, but cars and people and insects and flowers moving exactly as they move in Vienna itself. It would even represent me, as I drive my Toyota into the city and search for a place to park. But even if we could arrange this map so that each of its objects lay exactly on top of the one that it represents, it could still not provide a perfectly accurate and detailed representation in *every* respect. Since no two objects can occupy the same place at the same time, its spatio-temporal coordinates would necessarily be just off.

One of my postmodernist friends has suggested that we could correct this flaw by taking Vienna as a map of itself. And this, no doubt, is a postmodern suggestion. But I don't think that we need to think about it too long in order to see that such a postmodern map could not possibly serve any of the functions that maps are supposed to serve.

Maps and models are and ought to be oversimplifications. But whether or not they are *good* oversimplifications will depend upon what we want to do with them, and upon whether and to what extent they enable us to do what we want to do with them. It will, in other words, depend almost entirely upon the problems that we want to solve, and upon the alternatives that we have available. It will, in a nutshell, depend upon our problem situations.

Newton's problem was to explain the motions of the planets. His laws of motion describe how bodies move in an ideal state. Newton's first law says that 'Every body continues in its state of rest, or of uniform motion in a right line, unless it is compelled to change that state by forces impressed upon it.' But no body has ever continued in its state of rest or in a right line *over infinite space and time*. And, indeed, no body, if Newton's theory were correct, ever could – if only because all bodies, according to Newton's theory, influence each other by the force of gravity.

Newton's universal theory of motion was an abstract idealization. But Newton also constructed a model of the solar system in order to explain how the planets move in a way that people could understand. Newton's model, like all models, is an oversimplification. It represents the planets as mass-points, and it leaves out the asteroids and the cosmic dust. It represents neither the pressure of the light of the sun nor the pressure of cosmic radiation. It does not even represent the action of the distant masses upon the bodies of the solar system – let

alone the magnetic properties of the planets, or the electrical fields that result in their neighborhood from the movement of these magnets. (Cf. Popper, "Models, instruments" 172) But it is difficult to see how anyone could possibly have understood it, let alone worked with it, had it not oversimplified things in this way. The interactions between all these bodies, and the mathematics needed to describe them, would simply be too complex. Indeed, even as things stand today, we need models and approximation techniques when dealing with Newton's theory because it is too difficult to obtain exact solutions to problems involving interactions between more than two bodies.

Newton's theory of motion was also an oversimplification. And we have, despite some early hopes, known for a long time that it is an oversimplification. It does not explain all of the observed phenomena. But we used it, knowing that it does not explain all of the observed phenomena, partly because we had no better alternative, partly because we hoped to improve it, and partly because it explained how things are in the abstract in a way that allowed us to understand what we observed in the concrete in a way that was satisfactory enough for our purposes until our purposes and alternatives changed. I emphasize that Newton's theory was an oversimplification not in order to criticize it, but simply to underscore the fact that *all* scientific theories are oversimplifications. No scientific theory can represent the world exactly the way it is. But this is not so much a flaw in our scientific theories, as it is a prerequisite for them to be able to solve the problems that we want them to solve.

VII

But what about verisimilitude? And what, more importantly, about Slovenia?

Popper, despite his frequent criticisms of definitions and 'What is' questions, seems to have had a weakness for them.⁷ He admired Tarski's definition of truth. And he was proud of his own definition of 'verisimilitude'. Popper attempted to define 'verisimilitude' in terms of truth and falsity contents, and to measure a false theory's verisimilitude by counting and comparing the number of its true and false consequences. Popper's definition of 'verisimilitude' does not work because every false theory has exactly the same number of true and false

⁷ Indeed, one of his most famous articles is 'What Is Dialectic?'

consequences as every other. And Popper long ago acknowledged the fact. Many critics seem to regard this as a great embarrassment. But few of them, as far as I can see, think that 'verisimilitude' is meaningless or that one theory cannot be closer to the truth than another.

In my view, trying to measure verisimilitude by counting a theory's true or false consequences always missed the point. Every false theory has the same number (if we can really talk this way) of true and false consequences as every other. This is a consequence of the truth-functional nature of our logical connectives and the truth-functional definition of validity. But some false statements are still closer to the truth than others. All of our models of the solar system are false. But some say that the earth moves in a circle around the sun, and others say that it doesn't move at all. Our best model to date — the one that seems to explain more than any of the others — says that the earth moves in an ellipse around the sun. Let's assume, for the sake of argument that it does. There is, *given the problem situation of determining whether and how the earth moves*, then a perfectly clear sense in which a model that shows the earth moving in a circle around the sun is closer to the truth than one that fails to show it moving at all.

VIII

Slovenia is more difficult, but not very different. It seems false, *if we want to explain why Slovenia was not admitted into NATO in 1997*, to say that it did not satisfy the criteria for admission. But it seems even more false to say that it was not admitted because NATO did not expand in 1997 at all. Indeed, part of the problem situation is to explain why Slovenia was not admitted while Hungary, Poland, and the Czech Republic were.

I mention Slovenia not in order to give another argument for verisimilitude, but because I was surprised to read that situational analysis has had such little impact upon the methodology of the social sciences, and because I am wondering whether or not it really is true.

I am not an economist or a sociologist. So what I have to say may reflect nothing more than my own ignorance. But I did attend a conference in Budapest on 'NATO Enlargement, Reforms of the European Union and the Central European Region'; and I was struck by the fact that each of the social scientists who spoke relied entirely upon situational analysis.

Their models of the social situation were different. Some relied primarily upon political considerations, others upon economic considerations. But each of the speakers analyzed the situation in an attempt to explain why NATO decided not to admit Slovenia. And each did so in a way that, given the assumptions of his model, represented the decision as rational.

None of these speakers mentioned Popper, situational logic, or the rationality principle. But the analyses that they gave were all examples of it.

So I would be tempted, as a first attempt at answering Dr. Matzner's questions, to say that the reason why Popper's views on situational analysis and the rationality principle have not had much impact on research programs and have not inspired a greater response in the literature is that there was never any real controversy about them in the first place — as there was, for example, about his ideas that falsifiability is the criterion of a scientific theory and that scientists should actively try to falsify their theories.

There may, however, be more to it than this. So let me briefly mention two points that may be somewhat interrelated. First, the problem of situational analysis in the theoretical and historical social sciences, in Popper's view, is not to construct models that predict or prophesize the future. It is to construct models that help us to explain and understand the past. When we try to explain why Richard made all those funny movements while crossing the street, we are trying to *explain* an event that has already happened. We are not trying to *predict* how Richard will move the next time he crosses the street. Similarly, the speakers at the NATO conference were trying to explain why something that had already happened had happened. Many of them predicted that Slovenia would be admitted in 1999. But they typically added that the prediction might prove false. And I only wish to add that if the prediction does prove false, then we will, come 1999, be analyzing the situation once again, in an effort to give a rational explanation as to why it did.

This issue — whether a model is supposed to be a tool for explanation and understanding, or a tool for prediction and prophecy — is precisely what separates critical rationalism from instrumentalism. And this brings me to the second point, which is that Popper appealed to his definition of verisimilitude in order to explain how his treatment of the known falsity of social theories differs from instrumentalism.

I don't mean to be pedantic, but Popper did not characterize situational analysis as the fundamental problem of the social sciences – as Dr. Matzner suggests in his background paper for this workshop – but as the fundamental problem in the *theoretical* and *historical* social sciences. The fundamental problem, in a nutshell, 'is to explain and understand events in terms of human actions and social situations'. (Popper, "Models, instruments" 166)⁸

'It is to trace the unintended social repercussions of intentional human actions.' (Popper, "Prediction and" 342)⁹

This is important, because if we adopt the instrumentalist philosophy, then economics and sociology would not be theoretical or historical social sciences primarily interested in problems of explanation, but applied social sciences primarily interested in problems of prediction. And I want to emphasize this, because some people might think that Popper's failure to give a formal definition of verisimilitude means that there is no real difference between critical rationalism and instrumentalism after all.

But what do economists think? Is economics a theoretical or an applied science? Is it more interested in problems of explanation or in problems of prediction?

In his *Economics: Problems, Principles, Decisions*, Edwin Mansfield writes that 'the best way to get an idea of what economics is all about is to look at some of the problems it can help illuminate'. Mansfield goes on to list the following questions as 'typical economic problems': What determines the extent of unemployment in the American economy, and what can be done to reduce it? What determines the rate of inflation, and how can it be reduced? What determines the rate of increase of labor productivity? Why has this productivity slowdown occurred in the United States? What measures can and should be adopted to cope with it? Why is business competition desirable? Why does poverty exist in the world today, and what can be done to abolish it? These all sound like problems of explanation. But Mansfield quickly turns to a discussion of models. And when he does, we find that the purpose of models in economics is to make predictions.

⁸ Popper's italics.

⁹ Popper's italics.

Economics is based on the formulation of *models*. A *model is a theory*. It is composed of a number of assumptions from which conclusions – or predictions – are deduced. (Mansfield 23)

Mansfield then states the following three ‘important points’ about models:

1. *To be useful, a model must simplify the real situation. [...]*
2. *The purpose of a model is to make predictions about the real world; and in many respects the most important test of a model is how well it predicts. [...]*
3. *A person who wants to predict the outcome of a particular event will be forced to use the model that predicts best, even if this model does not predict very well. The choice is not between a model and no model; it is between one model and another. (Mansfield 23-4)*

I do not think that there is anything special or controversial in Mansfield’s account. I cite it, on the contrary, because I think that it is representative of what most economists think. But here my suggestion is that *if* Popper’s ideas about situational analysis and the rationality principle do *not* play the role in economics that Popper described, then it may be because economists today are interested more in predicting the future than in understanding the past.

I can, perhaps, go one step further and say that the models that economists use today are actually geared toward *shaping* the future.

Consider, for example, the models that economists use to ‘describe’ consumer behavior. The standard model assumes:

1. That the consumer, when confronted with two alternative choices, can order his preferences, so as to say whether he prefers the first to the second, the second to the first, or whether he is indifferent between them;
2. that the consumer’s preferences are transitive; and
3. that the consumer always prefers more of a commodity to less. (Mansfield 549)

Such a model is often said to be a theory of rational choice. And it is often presented as if preferring more of a commodity to less is, *as a matter of fact*, part and parcel of what it means to be rational. But in my view, the theory of rational choice seems more like a theory that is intended to indicate how one *ought* to go about making choices.

Or consider the old debate between the Keynesians and the Monetarists. The point is not simply to explain the relationship between money, government policy, and economic stability. And it is not simply to predict future events, or even the types of events that are likely to happen under various circumstances. The point of the debate between the Keynesians and the Monetarists is to shape the future by influencing economic policy.

Here I want to return to Matzner's background paper. Matzner says that 'the use neoclassical mainstream makes of the rationality principle focuses on the rather simple social situation in which *an agent maximises his/her utility under (monetary) constraints*' and that 'this is, in itself, not yet objectionable'. But in my view, specific aims like maximizing utility under monetary constraints are not part of the rationality principle as Popper understood it. They are, together with the constraints upon achieving them that are imposed by our environment, part of what he regarded as our social situation. Our aims may be clear and consistent, or vague and contradictory. And if they are vague and contradictory, then that fact alone may put further constraints upon our achieving them. But such constraints, for Popper, are part of the social situation and not part of the rationality principle. The rationality principle, for Popper, is the minimal assumption that we act adequately to our situation – that we act, in other words, in a way to bring about our aims, given our knowledge of the constraints imposed by our environment. Richard's aim to cross the street, in Popper's example, is not part of the rationality principle. It is, on the contrary, part of his social situation.

Please do not misunderstand. I see no reason why we cannot explain 'acting adequately to the situation' as 'maximization of utility under (monetary) constraints' – or why we cannot, if we like, put a dollar price on every aim we may ever wish to fulfill. I also see no reason why we cannot explain our aims in macroeconomic terms as the aims of a community or a society. But nor do I see any reason why we should do any of this, let alone why we need to do it, if doing it tends to obscure our explanations instead of clarifying them.

Here it is interesting that so-called 'rational expectations' theorists who argue that government cannot use fiscal policies to stabilize the economy are, whether they know it or not, echoing what Popper used to say about 'the Oedipus Effect'. (Cf. Popper, *The Poverty* 13-6) (Cf. Popper, *The Open*) They say that our models fail to take into account the influence of our knowledge upon the situation. 'Once firms and individuals learn of

any systematic rule for adjusting government policy to events, the rule will have no effect.' (Mansfield 418) But the Oedipus Effect 'predicts' precisely this problem with regard to economic predictions. Even if it is possible to predict the future over the short-run, and even if it is possible to predict that increases in government spending are desirable over the short-run, the full impact of increases in government spending may not be felt for several years, and they may actually be undesirable when they are finally felt. So the policy may well have unintended consequences. So the prediction may ultimately fail, even when it appears initially to succeed.

But if what I have been saying is true, then many social scientists are still hoping to apply their theoretical models to predict, if not to shape, the future. And this, perhaps, is another reason why *Popper's* account of models and situational analysis and the rationality principle may not seem attractive to them. Popper not only thought that the purpose of models and situational analysis and the rationality principle is to help us to explain and understand events in terms of human actions and social situations. He also explicitly denied that the task of social science is to make predictions or prophecies about the future. And he sharply criticized those social scientists who thought that it is.

This, I should add, is no minor point in Popper's philosophy. It is central to his critique of historicism, and thus central to his critique of scientific socialism.

Instrumentalists and critical rationalists can argue whether the primary task of the natural sciences is to explain or predict. But when it comes to the social sciences, there are good theoretical reasons to believe that predictions can be self-refuting.

IX

Let us now return to the question of how to reconcile Popper's talk about error-elimination and about truth as a regulative ideal with his acknowledgement that scientists often work with theories and models that they know to be false. This problem, I want to suggest, can be solved through a consideration of the type of problems that scientists are trying to solve, together with an analysis of the logic of their situation.

The fundamental problem for theoretical science is to explain. It is to explain those things that we want, but are not yet able, to understand. The fundamental problem for theoretical social science, more specifically, is to explain events in terms of human actions and social situations, so that we can understand their unintended consequences as the consequences of intentional human actions.

But every explanation must end somewhere. And any explanation may be further explained. There are many different levels of understanding. But no understanding is ever complete.

If someone doesn't understand that the earth orbits the sun, then we can begin with a two-body model that represents its orbit as a circle. If he doesn't know that the Stephansdom is in Vienna, then we can draw a circle with an 'X' inside it to represent the city and cathedral. If he knows nothing at all about Slovenia, then we can say that it has good economic and democratic traditions. But once he understands these things, we may want to construct new models to explain things further.

This is how and why science begins and ends with problems. And it goes a long way toward explaining how and why we can explain and understand things with models that are false. If we had to get things exactly right in order to understand them, then nobody would ever understand anything at all.

It does not, however, go all the way toward explaining it.

Popper thought that the aim of theoretical science 'is to find *explanatory theories* (if possible, *true explanatory theories*)'. (Popper, *The Logic* 61n)¹⁰ He regarded the rationality principle as 'an integral part of every, or nearly every, testable social theory'. (Popper, "Models, instruments 177) He wrote that 'to give a *causal explanation* of an event means to deduce a statement which describes it, using as premises of the deduction one or more *universal laws*, together with certain singular statements, the *initial conditions*'. (Popper, *The Logic* 59)¹¹ He said that 'we call this statement a specific or singular *prediction*'. (Popper, *The Logic* 60)¹² And he thought that predictions are of interest to a theorist, especially if they are observable, 'because they may be used as *tests* of theories'. (Popper, *The Logic* 59n)¹³

¹⁰ Popper's italics.

¹¹ Popper's italics.

¹² Popper's italics.

¹³ Popper's italics.

But how can we test a social theory, how can we assess the adequacy of our explanations and understandings, and how can we trace the unintended social repercussions of intentional human actions – if we cannot *deduce* predictions with which to test them?

And how can we deduce predictions to test them if the best theories that we ever have in social science represent initial conditions instead of universal laws?

The statements that we deduce to test our theories may be predictions or retrodictions. But if there are no universal laws in social science, then we will need to construct a new model of explanation and understanding that does not rely upon universal laws and the hypothetical-deductive apparatus that we use to explain and predict singular events in the natural sciences. That model explains an event by deducing it from a general law and initial conditions. And that deduction, in turn, enables us to test our explanatory theory against our experience and observations.

But all of this is impossible if we are unable to deduce events from general laws together with initial conditions. And in the social sciences we do not, according to Popper, usually have general laws, but only initial conditions.

Here, some people may think that we need to come up with a new model of explanation, and a new idea of how to falsify a theory – a model that will explain how we can falsify a theory that we already know to be false. We will need to invent a new model to explain how to decide to put a false model aside and to begin work on another model that we also know to be false, though perhaps not quite as false as the first. And we will need to explain how social science can be science at all, when the rule that ‘guides the scientific investigator in his work’ is that ‘we are not to abandon the search for universal laws and for a coherent theoretical system, nor ever give up our attempts to explain causally any kind of event we can describe’. (Popper, *The Logic* 61) But this, in my view, is precisely what situational analysis and the rationality principle are supposed to provide.

X

Talk of a new model of explanation and a new idea of how to falsify a theory may seem at odds with Popper’s well-known thesis of the unity

of method in the natural and social sciences. This thesis maintains that the methods used in natural and social science are 'fundamentally the same' and 'always consist in offering deductive causal explanations, and in testing them (by way of predictions)'. (Popper, *The Poverty* 131)¹⁴

But the unity of method thesis is all too obviously an oversimplification. Popper thought that the rationality principle enables us to construct comparatively simple models of human actions and interactions, and to use these models as approximations. (Popper, *The Poverty* 140-1) But despite his talk about the unity of method, he also thought that this indicates 'a considerable difference between the natural and the social sciences – perhaps *the most importance difference in their methods*'. (Popper, *The Poverty* 141)¹⁵

I refer to the possibility of adopting, in the social sciences, what may be called the method of logical or rational construction, or perhaps the 'zero method'. By this I mean the method of constructing a model on the assumption of complete rationality (and perhaps also on the assumption of the possession of complete information) on the part of all the individuals concerned, and of estimating the deviation of the actual behaviour of people from the model behaviour, using the latter as a kind of zero co-ordinate. An example of this method is the comparison between actual behaviour (under the influence of, say, traditional prejudice, etc.) and model behaviour to be expected on the basis of the 'pure logic of choice', as described by the equations of economics. (Ibid. 141)

We know that the rationality principle is empirically false. But we use it because it provides us with a general statement that enables us, when it is conjoined with a model of initial conditions, to deduce what would be rational to do in a given social situation. We assume that actors always act adequately to the situation. And we try to explain the actions and events that we want to explain – the ones, in other words,

¹⁴ Popper goes on to say that: "What is important is to realize that in science we are always concerned with explanations, predictions, and tests, and that the method of testing hypotheses is always the same [...] From the hypothesis to be tested – for example, a universal law – together with some other statements which for the purpose are not considered as problematic – for example, some initial conditions – we deduce some prognosis. We then confront this prognosis, whenever possible, with the results of experimental or other observations. Agreement with them is taken as corroboration of the hypothesis, thought not as final proof; clear disagreement is considered as refutation or falsification". (Popper, *The Poverty* 132-33)

¹⁵ Popper's italics.

that we do not already understand – as the consequences of intentional human actions. There is no great difference here between explanation, deduction, and testing. But we use our deductions, in this model, not to test a universal law, as we do in the natural sciences, but to test our models of the social situation.

Here the test, as always, is whether or not our explanation satisfies our understanding, at least for the moment, as to why whatever we want to explain happened. But if we build such specific aims as maximization of utility into the rationality principle, then we not only exempt them from examination, we also obscure the fact that they are in competition with the other aims that we are trying to achieve and with other theories about the aims that we are trying to achieve, as opposed to being part of what it means to act adequately to the situation.

XI

Karl Popper used to teach that science is trial and error, and that the aim of science is to get closer and closer to the truth. But he also taught that science begins and ends with problems. And this is simply another way of saying that it never really ends at all.

This is the way science is, and it the way, I think, science ought to be. For what we are trying to do in science is to explain what happens in a world that we can never completely understand.

We want our explanatory theories to be *true*. So we try to eliminate their errors. But the logic of our problem situation – the situation of trying to explain what happens in a world that we can never completely understand – prescribes that we put up with false explanations, so long as we believe that they are reasonably close to the truth. It prescribes that we do this, since doing otherwise would render us incapable of giving explanations at all.

REFERENCES BIBLIOGRAPHIC

Mansfield, Edwin. *Economics: Principles, Problems, Decisions*. New York: W. W. Norton and Company, 1983. Print.

Miller, David. *Critical Rationalism: A Restatement and Defense*. Chicago: Open Court, 1994. Print.

Popper, Karl R. *The Open Society and Its Enemies*. London: Routledge and Kegan Paul, London, 1945. Print.

---. *The Poverty of Historicism*. London: Routledge and Kegan Paul, London, 1957. Print.

---. *The Logic of Scientific Discovery*. London: Hutchinson, 1959. Print.

---. "Prediction and Prophecy in the Social Sciences". *Conjectures and Refutations*. London: Routledge and Kegan Paul, 1963. Print.

---. "Models, Instruments, and Truth". Notturmo, Mark A. (ed.). *The Myth of the Framework*. London: Routledge, 1994. Print.

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