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Reason, Science, Criticism

JOSEPH AGASSI INTERVIEWED ON HIS 90TH BIRTHDAY BY ZUZANA PARUSNIKOVÁ[1]

Joseph Agassi was born in 1927 in Jerusalem. In 1956 he received his PhD in logic and scientific method in the London School of Economics under the supervision of Karl Popper. Between 1954 and 1957 he worked as Popper's assistant and importantly participated in the preparation of the English translation and the revisions, and the *Postscript*, of *Logik der Forschung*. Joseph Agassi developed his own version of critical rationalism and has always endorsed Popper's suggestion that philosophy should not be sectarian but should apply the open-minded critical attitude to other subjects beyond science. In his books he has addressed a broad range of issues from history and philosophy of science to aesthetics, politics, education, psychiatry, medicine and the relation of science to society and culture. Joseph Agassi was appointed a Professor of Philosophy at Boston University, York University, Toronto and Tel-Aviv University. His rich bibliography includes among other books *Science in Flux, Popper and his Popular critics*, *Towards an Historiography of Science, Faraday as a Natural Philosopher*, *The Continuing Revolution: A History of Physics From the Greeks to Einstein*, *Science and Society, Science and Culture, A Critical Rationalist Aesthetics* (with Ian Jarvie). One should

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also mention his intellectual autobiography *A Philosopher's Apprentice: In Karl Popper's Workshop*.

Zuzana Parusniková (**ZP**): You received your secondary education at a theological school. After finishing it you decided to give up further religious (rabbinical) training and entered the university to study physics. That seems to me like a suicidal decision, given that you had no background in science or mathematics. Why physics?

Joseph Agassi (**JA**): I knew that it would be difficult, but not how much. I knew I could study the humanities and social sciences on my own. I also knew I could not do so with physics but I believed that to be serious one had to know some physics beyond popular physics. It is easy for me to explain it now but then it was very difficult, hazy indeed – the popular physics of the time was a mix of inductivism and instrumentalism, both of which sounded to me fishy and some idealism that I knew was rubbish. I had read Eddington and that made this clear to me: he was a philosopher and his philosophy was wrong; I found his idea that metaphysics is private ridiculous. (Popper did, too.)

ZP: Don't you find the Talmudic culture close to rational discourse based on criticism, and thus to science? For instance, Menachem Fisch (in his *Rational Rabbis* and elsewhere) compares Popper's critical rationalism with the Talmudic discourse and argues that the anti-traditionalist camp encourages the critical scrutiny and even revision of the halakhic tradition.

JA: Yes, I found the Talmud rational. But its rationality was insufficient for me. Fisch is a former student of mine yet I often fail to comprehend him. His idea in his *Rational Rabbis* is right but he is apologetic about it. I consider the term "apologetics" in the clear and traditional meaning. Look at Maimonides, one of the most rationalist thinkers of all times. He said, it is permitted to argue rationally in favor of the commandments, but not against them. This is apologetic. Yet Talmud is staunch on one thing: always let serious science have the last word.

ZP: When you came to London you were trying to find your own philosophical way. As you say, meeting Popper was a crucial, eye-opening

moment in this search; could you describe what appealed to you most, in Popper as a person and in critical rationalism? At that time, *The Logic of Scientific Discovery* had not yet been published, and you had not read *The Open Society*.

JA: I met Popper in January 1953. I decided to be his student after I had read a paper of his. And when I listened to a lecture of his I was sold. I became his assistant in fall 1954 to fall 1957. His logic lectures showed logic as goal directed – erotetic – and his epistemology was utterly skeptical but utterly optimistic. He chatted with students in class as equals. I read *The Open Society and Its Enemies* in Easter vacation 1953 and was very impressed. Edited the translation of the book into Hebrew a few years ago and was more so. I read drafts of the translation of *The Logic of Scientific Discovery* in 1955 and corrected them without having the original. Worked on it till it appeared, including the index.

ZP: It seems you were attracted mainly by two things in Popper – by his open-minded attitude to philosophy and by his willingness to discuss a wide variety of issues beyond science.

JA: Yes, Popper's broad cast annoyed Eva Cassirer. She said: "How dare he mention Mozart in a logic lecture?" It delighted me. I had much dialectical background before having met Popper and I left the Jewish dialectic as too apologetic. When I heard Popper present logic as dialectic I was thrilled. I realized at once that his view of science was the view of scientific method as dialectic. That delighted me, too.

I was attracted by his genuine freedom from convention. He was ready to discuss logic and music together if the discussion went this way, he allowed ignorant people to express their opinion freely as long as they were easy about it, he zeroed in on interest without bothering why it was there, as long as it was not fleeting and taken seriously by the one who exhibited it. He did not pull rank when he was at a loss, and he admitted being at a loss with no hesitation. Of course, these are qualities that we all have and that none of us has enough of. He stood out. The most impressive thing I found about him when we first met was his genuine interest in science and lack of power-worship. I was a physics student very frustrated by my teachers. Popper was very interested in my observations and also shared some of them. That was a boon for me.

ZP: Interesting that you use the term "dialectic", given Popper's dismissal of dialectic on the grounds that it violates the law of contradiction?

JA: You use the word "dialectic" in Hegel's sense. The default sense is the Socratic: criticism by the book.

ZP: For both Popper and you, Hegel is the deterrent example of "bad" dialectic. But whatever your view on Hegel is, don't you think that Popper is unfair in accusing Hegel of "confusing dialectic and logic"? Surely Hegel would never claim that formal logic should be redefined dialectically!

JA: It surprises me that you think well of Hegel's dialectic. It is all sheer rubbish. Nevertheless, I do think Popper undermined Hegel. (He could not say a good word on a charlatan, and this is a serious mistake.) Popper said, the only valuable thing in Hegel's output is his critique of the Enlightenment movement, and this Burke did earlier and better. The fact is, Hegel did influence much valuable biological research and revolutionized historical writing. True, it is mostly reactionary and contributed positively to the rise of militarism and thus to the disasters of the 20th century. Nevertheless, he did revived historiography.

ZP: I meant that Hegel never claimed that the law of contradiction should not be valid in formal logic (as Popper accuses him). Hegel's Logic is not logic but metaphysics. I can understand that Popper, you and many others dismiss Hegel's metaphysics. But Popper makes the mistake of transposing metaphysical terms (of Logic) to logic and the philosophy of science. For Hegel, the dialectical logic reflects the ontological structure of reality. But he recognizes the authority of logic in the sciences – in the sphere of understanding (*Verstand*). In science, contradictions are detected errors stimulating the further growth of knowledge.

JA: Making error is regrettable, though unavoidable. Detecting error is the motor of intellectual progress. This we agree about and this should do. I do not know of any modern philosopher who said it except Einstein and perhaps also Russell, and much later Bunge, Wisdom, Watkins and Gellner; but it is the cornerstone of Popper's philosophy and no one did it half as well as he did in his *Logik der Forschung*. There is nothing like this in Hegel or even in Marx or Engels.

ZP: OK, let us then define "good" dialectic as Socratic critical debate in a detachment from belief (*dogma*). These characteristics also define rationality. Yet, not every critical debate is rational. Can we debate metaphysics critically? How can we then determine which critical debate is rational?

JA: Your definition of dialectic and of rationality is agreeable to me, provided we remember that they are an ideal only approximated at rare moments. Even in science, the stronghold of rationality, much is fashion and authority and the love of mysticism.

ZP: But here exactly is Popper's point! Let scientists be mystics, as long as they can formulate a coherent, interesting and empirically testable hypothesis. The rationality of science lies, for Popper, in the methods of the refutation-aimed testing.

JA: There are no conflicting opinions here: Popper – and Russell before him – preferred not to argue about mysticism and encourage the mystics to talk clearly. They do not. You ask, how can we determine which critical debate is rational. Who says we can? The right question is not yours (how can we determine which critical debate is rational) but this: which critical debate is rational? Or, what makes some critical debate rational? Popper said, all critical debate is rational; not so, in my opinion. My rubberstamp refutation is scholasticism. The Roman Catholic Church just as the Socialist quasi-Church advocated the latest defense of the True Doctrine, but without insisting on it. When the latest defense is refuted, they accept the refutation and advocate the next defense. This is the dogmatic use of dialectic. Popper and Quine both viewed Carnap rational because he always accepted criticism. To me he seems a caricature of the rational, a kind of buffoon. He repeatedly offered a new variant of Wittgenstein's anti-philosophy. Wittgenstein seems to me even worse, yet he was a powerful critic and his criticism, at least of Russell when they cooperated, was admirably rational, his narrow-minded dogmatism notwithstanding.

ZP: In which aspect was Wittgenstein dogmatic?

JA: Wittgenstein said there are no philosophical problems and no proper philosophical assertion. Yet he considered his philosophy – including the

morality and politics that he absorbed from Tolstoy – "unassailable and definitive": not open to debate!

ZP: It seems we agree that criticism is the heart of rationality, that scientific rationality has an additional requirement for theories to have empirically falsifiable predictions, and that the growth of (any) knowledge is facilitated by new problems and hypotheses that are pressing and engaging (daring and risky). Further, critical debate is rational if it genuinely aims at the revision (even at the cost of a refutation) of the proposal.

JA: You say, rightly, since rationality is the characteristic not only of science, then criticizability is not a sufficient characteristic of science. What is, then? The answer seems to me to be criticizable explanation. This too may be too wide. Possibly an explanation is scientific if it invites criticism by relatively new experiments. It is hard for me to adjudicate.

ZP: You have often discussed the relation between science and metaphysics; Popper did not follow the positivist judgement that metaphysics is meaningless but you have always given metaphysics more importance than Popper did. Popper later allowed for a positive role of metaphysics in stimulating science. If I understand you well, you say – unlike Popper – that the interaction between physics and metaphysics works both ways. Metaphysics provides a certain context in which science (all knowledge) operates. Our criticism, then, not only moves knowledge forward but also alters the metaphysical framework.

JA: The interaction between science and metaphysics described here is optimal; most people and even most cultures are not science oriented at all and even the best scientific societies are steeped with superstitions and with positivism. My description of metaphysics and science is idealized: it is the logic of the researches of the best researchers in town. Popper said in his *Logik der Forschung* that he was concerned only with a part of their researches, not with heuristic, for example. My effort is to complement Popper's work by discussing a part of heuristic. It is what I learned from Einstein (who fully approved of Popper's theory and even found it trivial and unsatisfactory due to the omission discussion of the need that research has for some metaphysics). Late in life Popper was drawn into heuristic and into metaphysics, but not sufficiently and with some inconsistencies that are easy to remove.

ZP: Should critical rationalism thus not view metaphysics as a servant for science?

JA: Yes, AND *vice versa*. The idea of cooperation then is better than that of service. Metaphysics is thus not the queen of science and not an outcast. Still, science is in a stronger position – metaphysics without science stagnates but science without metaphysics is not quite blind, as some claim.

ZP: Popper says that metaphysical research programmes contain a possible framework for testable scientific theories. He described Democritus' atomism as metaphysics that was useful for science (in contrast to Hegelian dialectics). He also showed that later atomism became testable and he saw that as the optimal development. However, there is good and there is bad metaphysics, depending on what framework it provides for science. Within the good metaphysics he favoured rationalism, indeterminism, realism and later Darwinism. How do you exactly enrich this picture?

JA: I agree what you say about Popper although with a proviso: in Popper's first book he preferred to overlook this as there he discussed only given theories and their testability. He used atomism as a refutation of the philosophy of Wittgenstein and the "Vienna Circle", explicitly refusing to include in his methodology.

You ask a question that I wish to word thus: how do we render an untestable theory testable? Both in Plato and in Galileo there is a procedure: you ask more detailed questions; the more detailed a theory is the more we can hope to find a way to test it. I differ from Popper only in MINOR ways. There are a few differences that you have not mentioned. As to metaphysics and science, I do discuss the interaction between scientific theories and the metaphysical theories that they abide by. I also say that researchers often face a discrepancy between a scientific and a metaphysical theory and they try to reduce it. Try, not necessarily succeed. Popper says, a satisfactory (scientific) theory is a testable explanation. I say, it is more satisfactory when it integrates with other scientific theories within a metaphysical system. I say, the question what is a metaphysical theory depends on a metaphysics. After all, the problem of demarcation between science and metaphysics is metaphysical, not scientific.

ZP: Why is the problem of demarcation metaphysical?

JA: Obviously: the theory "a theory is scientific iff it is empirically testable" is not empirically testable. Popper sticks to deductive explanation that is testable, which is a positivist idea. Consider this historically. The two central metaphysical systems are realism and idealism. The conflict was also methodological: between the view that science rests on *a priori* truths and the view it rests on experience. When we give up certainty, the dispute remains: does science begin with *a priori* or *a posteriori* assumptions? Why not both? Because they may be in conflict with each other. Indeed, Kant employed both but first he divided science between the *a priori* and the *a posteriori* given, so as to insure the absence of conflict. Popper required this very conflict.

ZP: Let us look at realism now. We cannot prove that realism is true but we accept it because it provides the best framework for the development of science. Yet, Popper says, we should not accept realism dogmatically. If there is ever a more science-friendly metaphysical theory we should be ready to consider it as a matter of principle.

JA: You say Popper accepted realism. If you mean, he deemed realism true, then yes, of course he did. He considered every scientific hypothesis realist: he considered science cosmology. Idealism is a crazy theory that wise people like Berkeley advocated with ingenious arguments. Popper noted that as a Christian believer Berkeley could not believe in idealism, and believe that Jesus Christ went up to Heaven bodily. It was for him a methodological necessity, not an ontology. It is true of all versions of idealism. They are all obsolete. Reichenbach said, when we look at an object twice we have no right to assert that it existed between the two events. He calls this existence inter-phenomenon. I think he was a fool as he did not think the object disappears and re-appears. The assertion that the object does not disappear between the two observations is realist.

ZP: Yes, science is about real nature so let us declare true, but not dogmatically, immune from a revision (though that is very unlikely).

JA: You say, a revision of realism is very unlikely, as a matter of principle, and this makes me laugh. What is unlikely? What principle are you talking about? The only principle I know is the decision to be willing to consider alteration due to some criticism. I do not see that we need or even

can decide these things, but if so, my decision is to remain open to criticism as much as is within my (intellectual) powers.

ZP: Popper explicitly proposes "to accept realism as the only sensible hypothesis – as a conjecture to which no sensible alternative has ever been offered". It is a conjecture open to criticism and it is our decision to accept it as true (because it gives point to our search for truth). But let us turn to the decision to accept rationalism. It must be done prior to any argument. Hence, as Popper says, rationalism is not self-contained.

JA: Suppose irrationalism is an attitude, the attitude of distrust in reason. Then, to cite *The Open Society and Its Enemies*, "We could then say that rationalism is an attitude of readiness to listen to critical arguments and to learn from experience. It is fundamentally an attitude of admitting that '*I may be wrong and you may be right, and by an effort, we may get nearer to the truth.*'" This is not to decide that attitudes are or are not open to criticism but to draw attention to the fact that criticizing attitudes differs from criticizing theories, (and Bartley overlooked this fact).

ZP: Popper says that neither logical argument nor experience can establish the rationalist attitude. Therefore, we have to accept is as a matter of an "irrational faith". Some philosophers (e.g. Bartley) criticized Popper for fideism. What is your opinion?

JA: This is an odd question. Fideism is a qualified uncritical rationalism. Critical rationalism therefore cannot possibly be fideism. Bartley called Popper a fideist in a sense that is an extension of the initial sense of the word: as a qualified critical rationalism, not as qualified uncritical rationalism. Now fideism comes as a result of a criticism of uncritical rationalism. Bartley's criticism of Popper comes to show that there is no such criticism of critical rationalism. But Bartley said, as Popper said he qualified his critical rationalism, he was a fideist in the extended sense of the word. The fideist says, endorse an axiom uncritically and with no justification, and then you can demand that every later step be justified. This initial axiom is known by Descartes' term "the Archimedean point". The fideist says, the Archimedean point is not given so you have to choose one arbitrarily. Popper said, if you want to choose an Archimedean point arbitrarily, it is advisable to endorse the minimal assumption. And the minimal assumption is that criticism is beneficial. This brilliant idea Bartley

foolishly called fideism. To be more specific, the word "faith in reason" is not to be taken too seriously. Perhaps "life of reason" is more accurate.

ZP: I think that this problem can only be understood in the social context. Popper says that accepting rationalism is a moral choice. Applying criticism and rationalism in society (social theory and politics) is a defence against totalitarianism, utopianism and fanaticism. Social changes should be done by piecemeal steps that can be revised and reversed relatively painlessly.

JA: Yes, to be uncritical is irresponsible and even childish. Traditional social thinking was largely conservative: make as little change as possible, because reform – social or political – is very difficult and very unpredictable and fraught with undesired unintended consequences, especially weakening the social fabric. The exception was almost all utopian: ignore current social settings and start afresh. The last option was, ignore all social systems and do not replace them: live a-socially and take good care of yourself. Around 1900 the industrial revolution led to a new attitude and to a broad development: solve social problems. The first such move, it seems to me, came earlier: it was the cooperative movement discussed and summed up by Sam Smiles and by George Jacob Holyoake. It was developed by the Fabians and the London School of Economics and Political Science (especially Beatrice and Sydney Webb, Bernard Shaw and Bertrand Russell); their discussion between the choice of evolution or revolution made them famous. Popper made this the principle of piecemeal social engineering. This was read as his advice to undertake minor projects before major ones. This is not true. His idea was to tackle specific problems and not the ills of society as such, as the later leads to an overhaul of society which is utopian engineering, and he wanted social engineers to be aware of the possibility of big errors that make the cure worse than the illness. In my opinion all this is right but outdated by the need to save humanity from extinction. This need demands rethinking Popper's ideas.

ZP: You regard Popper's concept of corroboration as too rigid. Can you explain why?

JA: The idea that criticism is valuable is just terrific, it is Socratic. Nevertheless, my greatest dissent from Popper is my claim that we can admire different parts or aspects of science for different reasons.

ZP: What other different aspect of science do you have in mind? Popper – as I understand him – allows all sorts of practices in science. Valuable are those which are bold (improbable), provocative, offer new ideas, have high empirical and informative content. Criticism consists (in science) ONLY in ruthless falsification attempts. Popper's originality lies for me in his radicalism. No other method of testing is allowed or needed. This rules out any form of justification – for instance a weak appeal to criticism (after which the theory acquires some evidential support that makes it "stronger" than it was before the testing). Do you consider this too strict?

JA: Before answering your question, let me add a few points to what you say of Popper, which is very nice indeed. First, William Whewell, who was a justificationist, of course, outlined the hypothetico-deductive method. Popper's view of it is in agreement with it, even though Whewell was not as clear and emphatic. (Quine, it seems, did not like Popper's emphasis although he had no criticism of his methodology). In Whewell's view refuted theories play no positive role in science. Acknowledging that Newton's theory of light had been rejected (in the year 1818), he declared it worthless. Popper could not possibly declare worthless Newton's theory of gravity, and so he had a better challenge and showed better results. Popper's rejection of all justification and all justification surrogates is his greatness. So he had a problem: what is the value of corroboration? He said in the first place it is the information that a search was a failure. This is obviously true. Also, the corroborated hypothesis has an increased explanatory power and thus fewer competitors for the time being. Popper wanted more, and there he faltered; demanding more he made a concession to the inductivists: it is, to use his words, his admission that the view includes a whiff of inductivism. He took back this concession at once. And so, to answer your question, Popper's additional inductivist requirement is not too strict but too lax. He should not have made it and I am glad he withdrew it.

ZP: Do you mean the whiff of inductivism that you found in Popper's "third requirement for the growth of knowledge"? (Theories should not only be refutable but they should pass some tests; this makes it possible to get nearer to the truth). I thought Popper said that a corroborated theory simply stays in the game – it advanced some interesting problems and new

solutions and if it is not refuted (it is corroborated) it is conjecturally true. Nothing else is gained by corroboration.

JA: What you say of Popper on corroboration is true. It is his third requirement that is puzzling, the requirement that a theory be corroborated before it be refuted. This is new and redundant at best, perhaps also refuted by cases like that of the Bohr-Kramers-Slater 1924 theory that is very important and that was refuted upon its very first test. In any case, the question why some theories are corroborated is scientific, not methodological or epistemological. Thus, according to Einstein, Newton's theory of gravity is highly corroborated because the sun is so much heavier than the planets and the size of the solar system is not too small. Popper later said, the "whiff of inductivism" in his theory is the hope that the history of science shows an increased verisimilitude. This is more interesting.

ZP: I know that you demand some positive role of corroboration alongside with Watkins, Worrall and Zahar, especially in practice. But rationally, good reasons are not available or necessary. Do we need more?

JA: Yes, we do: the practice that the inductivists discuss is psychological as you suggest. The practice that we live is social, including the need to license practices. My study of technology rests on the fact – a scientific fact – that the laws of civilized countries require the corroboration of certain hypotheses before launching them in the market is permitted. These hypotheses are guarantees that the promised performances of the technologies in question are valid and that there are no harmful unintended consequences. The law often specifies what consequences are empirically excluded and how severe the tests should be to exclude the undesired effects. In other words, some justifications of technologies are required by law. They differ from induction, from the philosophical justifications that are impossible: they are valid only within certain restrictions. When these restrictions are violated, these technologies may fail. The failures of the technologies in question are then absolved as act of God (*Force majeure*).

ZP: Yes, this is the notorious practical problem of induction. For Popper, there is no guarantee of success. He says: "in spite of the rationality of choosing the best tested theory as a basis of action, this choice in NOT

rational in the sense that it is based upon good reasons for expecting that it will in practice be a successful choice".

JA: The practical problem of induction is whether the technology we successfully use today is promising in the long run. The theories used in such technologies are insufficient – many techniques have no theoretical background – and usually false – we still use Galileo's theory and Pasteur's theory, both very well refuted. And indeed, there is no guarantee that the sun will rise tomorrow. Science was invited to prove this and it proves the opposite instead: the sun is a nuclear furnace so it can explode any day. In my view although there is no guarantee that the sun will rise tomorrow it is rational to assume that it will and not that it will not. For, if the sun will not rise tomorrow, then it does not matter what we do today but if it will it does.

ZP: However, in Popper's intention corroborated theories do not promise anything (in the sense of reliability) from the rational point of view; they may cause some subjective psychological reassurance, that's all. On a smaller scale it is the same as the dilemma whether to jump from the window or take the lift.

JA: We take it for granted that jumping through the window is disastrous, and that taking the elevator is not sufficiently safe either. The discussion of this case in the literature is the lie that the elevator is safe.

ZP: Back to our question of why corroboration matters. You know I am a supporter of Miller's version of critical rationalism and thus I would say that corroboration does not matter. The question rather stands: "why NOT accept a corroborated theory?" Questions about the reasons FOR accepting a corroborated theory beg a justificationist answer.

JA: What is the good of corroboration? Firstly, it is the refutation of a competitor; when the competitor was the consensus, the public-relations spokespeople of science did not like to notice refutations and so they stressed corroboration. This holds for the vulgar. Secondly, technology needs corroboration, and by law, and in order to show responsibility. Look at history: the theory of rational belief rests on the great discovery (Bacon, Galileo) that observations are theory-laden. Bacon recommended to stop believing in any theory and to rely on naïve realism (commonsense). Galileo refuted this idea

(you might very well see the moon jumping from rooftop to rooftop when strolling down the moon-lit street). He recommended using mathematics, a recommendation that is a cornerstone of Kant's theory of science. Now all this is past history. There is no reason to suppose that we can be free of erroneous theories in research situations or in any other situation. Rather, we can try different theories and see which functions better under which conditions. In sum, we should not worry about acceptance of theories. Rather we should always seek explanations and, when possible, competing ones.

ZP: Let me close the discussion on corroboration with this question: your view on corroboration entails the possibility of empirical support of a theory. In that case it entails induction. Do you allow induction in methodology?

JA: Induction as one way to generate testable hypotheses is fine. Induction as justification is silly. Corroboration as a crucial experiment is a refutation. Corroboration as the increase of explanatory power is fine. What other option is there?

ZP: The option is that a proposed theory deserving our research interest already entails new information, new explanations and predictions. It is either refuted or retained (corroborated); the increase of explanatory power is not due to corroboration. If you claim it is, do you, then, allow induction in methodology?

JA: Yes, Popper did so already (in his third requirement).

ZP: But you said that he withdrew it! So you propose an inductive-critical model of knowledge?

JA: No. Heavens forbid! It does not deviate from Popper's hypothetico-deductive model, since that model does not apply to heuristic. It was never meant to apply there: the model does not say how a hypothesis is generated. And so it may be inductive although this is unlikely.

ZP: Lakatos argued that it is impossible to falsify an isolated theory since theories are interlinked in a research program. He draws on the Duhem-Quine thesis. Do you think we can test an isolated theory?

JA: The Duhem-Quine thesis says that verification is not possible, not even by crucial experiments. The argument for it is the observation of Duhem (1906, 1954): crucial tests are no verifications as they employ

unverified working hypotheses. What this has to do with Popper I have no idea. Nevertheless, I expanded on it in my *Popper and his Popular Critics*; the popular critics all thought the Duhem-Quine problem/thesis/argument was a fatal objection to Popper's characterization of scientific character as falsifiability. Roughly, it relates to a brief rider of Duhem to his discussion of his thesis: a crucial experiment is no proof; at most it is a disproof. But, he added as a rider, even that is not clear-cut since we can always rescue the refuted hypothesis from refutation by blaming the working hypothesis for it – logic always allows for this move.

ZP: Irrespective of Duhem, do you think it is possible to apply a crucial falsifying test to an isolated theory?

JA: Of course when we use the same instrument it is harder to say that the instrument mislead against one theory without also saying that it mislead against the other. And if it misleads against both yet the experiment goes one way, it is a challenge to examine the situation afresh. When a situation looks challenging, it seems to me a Good Thing and to Elie Zahar a Bad Thing. Duhem said all tests involve working hypotheses about the test's environment. This does not change in cases of crucial tests. This, incidentally, is why before testing a hypothesis it is wise to test the working hypothesis involved: a part of it is the calibration of the test's instruments. Every experimenter knows this. My trouble then is in the question, what is it about Duhem's argument that some people say it refutes Popper? Also, why do these people do not say that Popper's *Logik der Forschung* discusses this argument at some length? I have a conjecture: people may think that it is hard to persuade people, and so Popper says, better dissuade them. And then a shock: it is hard to dissuade people, too. Popper said so repeatedly and this is why the Vienna Circle people disliked his views. They wanted to impose the scientific worldview and for this they tried hard to fight dogmatism. Popper said, this cannot be done. Dogmatism is unwise, but it is logically permissible.

ZP: Popper's concept of verisimilitude was proved wrong (Tichý, Miller) because false theories cannot be compared for verisimilitude. Many philosophers of science thought that this was the end of Popper's non-inductive account of the progress in science (Newton-Smith talks about a "full blown storm" of inductivism). What is your opinion on this matter?

JA: The idea of verisimilitude is Einstein's, not Popper's. Popper offered a MEASURE of verisimilitude. He never explained what it is good for. It backfired because he did not examine it carefully as he usually did with his innovations. It was, incidentally, a booboo. I offer a minimal definition in order to save verisimilitude:

Popper has offered his late view of scientific progress (L) in addition to his early view (E),

(E) Progress is empirical success;

(L) Progress is verisimilitude increase;

I have offered a minimal definition of verisimilitude increase: a theory is more verisimilar than its predecessor if and only if all crucial evidence concerning the two goes its way.

ZP: You appreciate Popper's emphasis on criticism and say that he elevated criticism "from hors d'oeuvre to the main dish". I tend to see Popper as elevating criticism to the only (rational) dish; I speak of testing. His new conception of reason – *ratio negativa* – allows NO justification. Don't you betray this legacy when you claim that "when an attempt at empirical criticism misfires the result is positive evidence"? Why not just say that the result is the absence of empirical refutation?

JA: It surprises me very much that you ask why not call it "the absence of empirical refutation", as I do that a few times. Yet the received name is "positive evidence" and there is never a good reason against a received name.

ZP: The name evokes the justificationist interpretation. Popper would say (as he does in the case of "good reasons") – call it positive evidence if you must but it does not involve any support of the theory (any increase of probability, credibility, reliability, certainty etc.).

JA: The claim that seems to bother you is that positive evidence is easy to find if that is what you want. I do not see why. Most unsophisticated people seek positive evidence, and they usually find it. Of course, they seek it in the hope that it makes them feel secure. This feeling is often illusive. At times the illusion that it gives them is dangerous. All this is well known and it is not clear to me what troubles you about it. What you incite me to

say is that the positive evidence that comprises failed criticism, corroboration, is different. And it is: it does not matter whether corroboration strengthens our belief or not; it matters that it is valuable information – even though refutation is more valuable. Consider the corroborations that the science textbooks cite. It is often significant information and it is often enlightening. Popper wanted to know why and he offered a theory of it. We can and should put it to critical assessment. That is all that there is to it. Corroboration takes place also in everyday life. Most philosophers of science say it is probability and Popper has refuted this.

ZP: Well, justificationism troubled Popper. From the point of logic he dismissed positive evidence as flawed (induction). But from the psychological point of view he knew that we (sophisticated or not) have inborn dogmatic tendencies – we want our expectation to be fulfilled. Therefore, we find "positive evidence" reassuring and are after it. This tendency is, for Popper, not rational, does not encourage bold and risky conjectures and is hostile to criticism. You yourself defend the value of positive evidence and claim it is needed.

JA: This is not the way it seems to me. No positive evidence supports any theory. Nevertheless, there are different kinds of positive evidence that require different treatments. First, the positive evidence that is inductive (Hempel calls it instantiation) Popper rightly dismissed; the positive evidence that is the result of tests and that science textbooks cite is valuable, though not as a support and less than as refutations. The question, what is the value of positive evidence, is where my view differs from Popper's, not the previous points.

ZP: Let me elaborate on Popper's attitude to dogmatism. Dogmatic tendencies are strongly ingrained in our nature; they are in our genes – or, as Popper says – biologically *a priori*. Popper urgently felt the danger and the cunning of dogmatism – we could continue to practice dogmatism while proclaiming criticism (dogmatism can sneak in through the back door, as you say): hence his radicalism in identifying the rational approach with criticism. In other words, it is criticism that needs the boost.

JA: The disposition for dogmatism is ubiquitous, but once we learn how to doubt and to criticize, it becomes an unshakeable habit. Popper said – although hardly wrote – that the risk of dogmatism is permanent and we

must keep vigil. This seems to me exaggerated. His criticism of Neurath – for his permission to ignore refutations with no qualification – seems to me exaggerated. It is nice to have Popper's qualification, but that qualification seems to me not necessary and at times even excessive. For example, it is a censure of Faraday for his having refused to accept the verdict of experience when he failed to find what we call today the Kerr effect. Dogmatism is stagnation and boredom. Those who like it are welcome to it. The healthy response is to prefer innovations and the Popper who teaches us that dogma and novelty conflict does better than the Popper who warns us against dogmatism. The refusal to be dogmatic seems to me – not to him – to be possible to take for granted in some contexts.

ZP: My point was that Popper proposed a strictly negative methodology because of his *horror dogmatis*. He saw this as the only way to keep dogmatism behind the door.

JA: My appreciation is to Popper's avoidance of dogmatism without despair in reason. Yes, his view of dogmatism is reasonable; his putting pressure to prevent it is not. Popper's negative philosophy appeals to me very much. His insistence on it is superfluous. It even betrays a measure of distrust that is unbecoming.

ZP: Well, putting pressure to prevent dogmatism is the only effective strategy. You and Ian Jarvie provocatively argue for the rationality of dogmatism. You base this argument on Popper's repeated claim that dogmatism is the necessary initial stage in assessing a hypothesis. The hypothesis must show its strength and prove its mettle before it is critically attacked. I consider this dangerous since dogmatism could then become uncontrollable.

JA: The paper of Jarvie and myself works on the assumption that there are degrees of rationality, so that the rationality of dogmatism is limited, but that we are all dogmatic to this or that extent, and usually unknowingly. The great switch of Popper in the study of rationality, including scientific rationality, of course, is the shift from psychologism to sociologism, from 'How do I learn/know?' to 'How do we learn/know?'. This is not to exclude psychology, of course, but to limit it to social settings. The discussion that Bartley developed concerns the faith in reason of an individual (of first person singular). It was psychological and its sociological aspect is lost.

ZP: Yes, but back to dogmatism. If we allow some degree of dogmatism into methodology we cannot control the extent of dogmatism – it can spread indefinitely, insisting that the theory has not yet proved its mettle. How are we to determine when the dogmatic protection of the theory from criticism should cease? As a radical Popperian I think that in order to keep dogmatism behind the door we must apply radical measures and separate rationality from dogmatism. Dogmatism is inherent in all forms of justificationism; strategies seeking the confirmation of a theory are not only logically flawed and thus irrational, but tend to immunize theories against criticism and thus suppress the growth of knowledge.

JA: Yes, I do not fear dogmatism, even though I agree with you about dogmatism being the default option. Most people I know are dogmatists, including philosophers of course and including scientists to my surprise. I consider the critical attitude incurable, or else dogmatists would have won the day long ago. To follow Popper's theory of tradition, it is possible to destroy the critical approach (as the middle ages illustrate), but for this it is necessary to fight criticism on a large scale and systematically, something like what was experienced in the leading early-twentieth-century non-democracies. They failed because they could not bar the import of criticism, no matter how little. See how isolated Popper was in Vienna, and how powerful Schlick was, yet miraculously Schlick is dead and Popper is not. Think of the amount of radio/TV religious propaganda in the USA and look at its yield. It makes one optimistic.

ZP: Well then, you are more optimistic than I am about the natural willingness of the human species to practice criticism. I hope you are right.

Finally, what do you consider the most valuable and original in Popper's contribution to philosophy? I would mention criticism as the basis of his original negative conception of reason; the encouragement not to be intimidated by any authority; the appeal to appreciate the positive value of erring and to welcome disagreement. The weak point is tying rationality too tightly to logic.

JA: Popper's weak points are his Protestant work ethics (it is misanthropic) and his Kantianism. His anti-metaphysics is Kantian and is to be dismissed as a part of Kant's dismissed justificationism. Kant's categorical imperative that won admiration is absolute and so not applicable. Popper's

vision of science is contrary to Kant. His great achievements are his view of science as a Socratic dialogue with the result of making room for the Einsteinian revolution, and his linking democracy with science thus presenting it as limited but able to progress.

ZP: Joseph, thank you for this interesting conversation.

JA: Thank you, Zuzana, for your interest in my opinions, for your interrogations and for bringing my responses to the public. I hope readers find the discussion interesting and helpful, since it dispels the popular misconceptions of Popper's ideas and enables the interested to pursue the problems that Popper's philosophy raises.