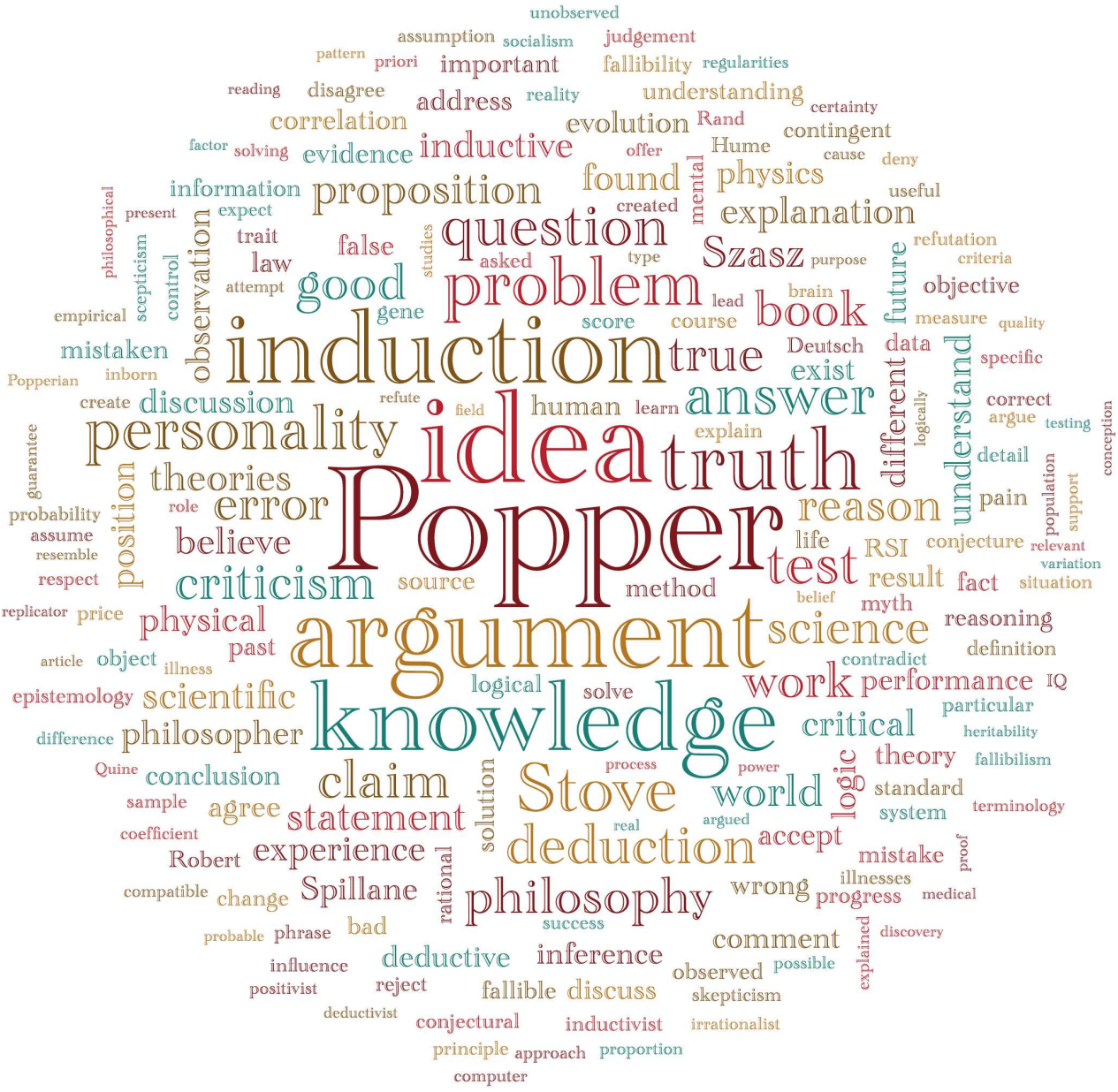
A Philosophical Discussion with Robert Spillane



by Elliot Temple

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elliot@fallibleideas.com

www.fallibleideas.com

www.curi.us

Editor's Introduction

This book collects a wide-ranging conversation between philosophers Elliot Temple and Robert Spillane. Topics include epistemology, logic, induction, deduction, personality tests, and IQ.

Elliot Temple is an American philosopher whose intellectual influences include David Deutsch, Ayn Rand, Karl Popper, William Godwin, and Ludwig von Mises. He has made several important original contributions to philosophy, including the idea of **Paths Forward** and **Yes or No Philosophy**.

Robert Spillane is an Australian philosopher who worked with Thomas Szasz for many decades, and who is familiar with Karl Popper and other philosophers. His book **An Eye for An I: Philosophies of Personal Power** discusses many philosophical ideas.

-Justin Mallone

Repetitive Stress Injury Psychology and Personal Story

Below is an email to **Robert Spillane**. He's a thinker who agrees with lots of Thomas Szasz's ideas, and knows a lot about Popper and other philosophers. His book **An Eye for An I: Philosophies of Personal Power** covers many philosophical ideas. He wrote an article about Repetitive Stress Injury (RSI).

I share my experience with RSI. From my story, you can learn about RSI, and you can also learn how to think about, take responsibility for and solve one's problems.

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http://www.szasz.com/spillaneremarks.html

I had RSI problems, which I solved by myself before reading Szasz. Before reading much of your perspective, I wrote down my existing thinking, below. After reading the rest, I see that we broadly agree. I believe my view adds something you don't say.

I liked your comment on the word "demoralised". I particularly agree with:

> There are serious psychosocial consequences when people with discomfort in the arm are told that they may have a crippling disease which demands urgent medical treatment and cessation of physical activities.

And I found this especially horrible:

> Personal activity is discouraged because insurance companies, facing large payouts, employed private investigators whose evidence, admissible in industrial courts, could prove embarrassing to plaintiffs. Faced with the prospect of jeopardising their claim, workers were inclined to adopt the patient role and assume a state of dependency

I'd be very interested if you think any of my account is mistaken or contradicts Szasz:

I had wrist pain which disrupted my computer use. I wasn't malingering. I wanted to use computers heavily. I didn't have a job at the time ("Occupational Overuse Syndrome" is stupid). I didn't spend much time interacting with doctors about it. I didn't find the doctors useful. I found better info online. I didn't use any RSI medicine beyond wearing wrist splints while sleeping. I could have gotten cortisone shots and probably surgery if I'd wanted to; that would have been a terrible idea.

Bodies have physical limits. My physical problem was real and was addressed with physical solutions: a better chair, ergonomic changes, stretching, breaks, and a temporary reduction in typing. My main problem was typing with bent wrists, which I ceased after educating myself.

I was scared by reading about how RSI could cripple me long-term. What people say about RSI is very dangerous. While learning standard RSI advice, I made myself fearful and stressed about whether my wrists would improve. RSI advice says you're largely helpless – you may be crippled for life with nothing you can do about it. I started worrying.

My physical problem was adequately solved after perhaps a few months, but I didn't notice. I had ongoing pain for several years! Because of my fear, I was oversensitive to minor pain and minor non-pain sensations, and I imagined some pain. I hated my RSI problem rather than benefitting from it.

What really scared me was the claim, which I accepted, that pushing past pain would make my injury worse. That was completely different than my attitude to sports. In sports, I routinely ignored minor pains because I had a rational understanding of which pain indicated a genuine danger and which pain was harmless. I'm good at ignoring pain that I don't consider dangerous.

I had a bad time with RSI because I accepted bad ideas about which pain is dangerous. After the initial physical improvements, I only had mild pain that I could have tolerated if I wanted to. But I was unwilling to because medical authorities told me that ignoring the pain could damage my body and cripple me in the long term. I could have toughened up, as I'd done with sports pains, but medical advice told me not to! I was trying to be responsible and conscientious...

My pain went away when I recognized what was going on and relaxed about it. I'd already solved the physical problem in the past. Introspection and changing

my attitude then solved the mental problem.

I believe on principle and logic (without much direct evidence) that the pattern of my experience is common, minus the solution. But I couldn't estimate how common it is compared to other patterns like malingering. The pattern is:

1. Have a real physical problem while psychologically fine.
2. Learn about RSI and create a psychological problem.
3. Take steps to solve the physical problem, which work.
4. Have an ongoing psychological problem which you confuse with the original physical RSI injury.

Note this pattern explains the development of RSI over time, in contrast to the 8 scenarios you present which state the situation at a particular time.

So I think the standard advice and medical authority associated with RSI is doing immense harm. It scares people, and encourages them to be oversensitive to pain and therefore to exaggerate. Thereby, **"medical" advice causes RSI!**

I was fooled by bad, pseudo-medical advice to intentionally be sensitive to mild discomfort... The reasoning was that pain is a warning sign for injury, so if you try to be mentally tough about the pain then you will cripple yourself. I think serious physical injuries called "RSI" happen, but malingering, exaggerations and mental errors are way more common.

Explaining Popper on Fallible Scientific Knowledge

In *The Logic of Scientific Discovery*, sec. 85, Popper writes:

> Science is not a system of certain, or well-established, statements; nor is it a system which steadily advances towards a state of finality. Our science is not knowledge (*epistēmē*): it can never claim to have attained truth, or even a substitute for it, such as probability.
>
> Yet science has more than mere biological survival value. It is not only a useful instrument. Although it can attain neither truth nor probability, the striving for knowledge and the search for truth are still the strongest motives of scientific discovery.

What does Popper mean when he denies science is "knowledge (*epistēmē*)"? He explains (sec. 85):

> The old scientific ideal of *epistēmē*—of absolutely certain, demonstrable knowledge—has proved to be an idol. The demand for scientific objectivity makes it inevitable that every scientific statement must remain *tentative for ever*.

His point here is *fallibility*. There's no way to ever prove an idea with finality so that there's no possibility of it ever being overthrown or improved in the future. There's no way to be 100% certain that a new criticism won't be invented later.

People consider Popper a skeptic because they see the options as infallibilism or skepticism. Popper does deny infallibilist conceptions of knowledge, but disagrees that infallibilism is a requirement of genuine knowledge.

In the first quote, Popper uses the word "knowledge" in two different senses, which is confusing. The first use is qualified as "epistēmē" and refers to view that we must find a way around fallibility or we don't have any knowledge. The second use, in "striving for knowledge", means good ideas (useful ideas, ideas which solve problems) as opposed to random, arbitrary or worthless ideas. The

view that we have no way to judge some ideas as better than others is the skeptical position; in contrast, Popper says we can use criticism to differentiate ideas.

I'll now discuss individual pieces of the first quote.

> [science] can never claim to have attained truth

Popper means that even if we had an idea with no errors, we have no means to absolutely prove it has no errors and then claim there are none. There are no methods which guarantee the elimination of all errors from any set of ideas.

An idea with no errors can be called a *final* or *perfect* truth. It can't be refuted. It also can't be improved. It's an end of progress. Human knowledge, by contrast, is an **infinite journey** in which we make progress but don't reach a final end point at which thinking stops.

Could there be unbounded progress while some ideas, e.g. 2+2=4, are never revisited? Yes but there's nothing to gain by being dogmatic, and there're no arguments which yield exceptions to fallibility. Just accept all ideas are potentially open to criticism, and then focus your research on areas you consider promising or find problematic. And if someone has a surprising insight contradicting something you were confident of, refute it rather than dismissing it.

> [science] can attain neither truth nor probability

Regarding probability: There's no way to measure how close to the (perfect) truth an idea is, how much error it contains, or how likely it is to be (perfectly) true. The method of judging ideas by (primarily informal) critical arguments doesn't allow for establishing ideas as probable, and the alternative epistemological methods don't work (Popper has criticisms of them, including on logical grounds).

Also, probability applies to physical events (e.g. probability of a die rolling a 6), not to ideas. An idea either is (perfectly) true or it isn't. Probability of ideas is a *metaphor* for positive support or justification. I've addressed that issue under the heading: **gradations of certainty**.

> Science is not a system of certain, or well-established, statements

What's good about scientific statements if they aren't well-established or certain? They aren't refuted. We've looked, but haven't found any errors in them. That's better than ideas which are refuted. I shouldn't accept or act on ideas when I'm aware of (relevant) errors in them.

My judgements are capable of being mistaken in general. But that isn't a criticism of any particular judgement. Ideas should be rejected due to critical arguments, not due to fallibility itself.

> striving for knowledge and the search for truth

The human capacity for error ruins some projects (e.g. attaining absolute certainty, attaining epistēmē). But it doesn't prevent us from creating a succession of better and better ideas by finding and fixing some of our errors.

Reply to David Stove on Popper

Popper and After: Four Modern Irrationalists, by David Stove criticizes Karl Popper's philosophy of knowledge.

But Stove's criticism *doesn't focus on epistemology*.

And Stove writes insults and other unserious statements. These are frequent and severe enough to stand out compared to other similar books. I give examples.

The book's organization is problematic as a criticism of Popper because it criticizes four authors at once. It only focuses on Popper for a few paragraphs at a time. It doesn't lay out Popper's position in detail with quotes and explanations of what problems Popper is trying to solve and how his ideas solve them.

First I discuss the book's approach and style. Then I address what I've identified as Stove's most important criticisms of Popperian philosophy.

My basic conclusion is that Stove doesn't understand Popper. His main criticisms amount to, "I don't understand it." Popper contradicts established philosophy ideas and some common sense; Stove doesn't know why and responds with ridicule. Stove is unable to present Popper's main ideas correctly (and doesn't really try, preferring instead to jump into details). And without a big-picture understanding of Popper, Stove doesn't know what to make of various detail statements.

Stove's Focus

Part 2, Ch. 3 begins:

> Popper, Kuhn, Lakatos and Feyerabend have succeeded in making irrationalist philosophy of science acceptable to many readers who would reject it out of hand if it were presented to them without equivocation and consistently. It was thus that the question arose to which the first Part of this book was addressed: namely, how did they achieve this? My answer was, that they did so principally by means of two literary devices discussed in Part One. The question to which the present Part of this book is

> addressed is: how was irrationalist philosophy of science made acceptable *to these authors themselves*?

Stove says the first part discusses how Popper achieved influence. How did Popper convince readers? What *literary devices* did Popper use to fool people? And part two (of two) discusses the psychological issue of how Popper made irrationalism acceptable to himself.

By Stove's own account, he's not focusing on debating philosophy points. He does include epistemology arguments, but they aren't primary.

The problem Stove is trying to solve plays a major role in his thinking (as Popper would have said). And it's the wrong problem because it assumes Popper is an irrationalist and then analyzes implications, rather than focusing on analyzing epistemology. If Popper's philosophy is true, Stove's main topics don't matter.

Ridicule

Ch. 2:

> It is just as well that Popper introduced this [methodological] rule. Otherwise we might have gone on indefinitely just neglecting extreme probabilities in our old bad way: that is, without his permission.

This is unserious and insulting. Popper's purpose was to discuss how to think well, not to give orders or permission.

> To readers in whom the critical faculty is not entirely extinct, the episode has afforded a certain amount of hilarity.

This is mean.

I point out more examples of Stove's style as they come up.

Neutralizing Success Words

Ch. 1 discusses neutralizing success words. A success word like "knowledge" or "proof" implies an accomplishment. Compare "refuted" (a successful argument) to "denied" or "contradicted" (doesn't imply the denial has merit). Neutralizing

knowledge yields *idea* – knowledge means a good idea, whereas an idea could be good or bad. Neutralizing *proof* yields *argument* – a proof is a type of successful argument, whereas a mere argument may not succeed.

Stove says Popper equivocates. Often, Popper uses success words with their normal meaning. But other times Popper changes the meaning.

> It is the word "knowledge", however, which was the target of Popper's most remarkable feat of neutralization. This word bulks large in his philosophy of science (much larger than "discovery"), and in recent years, in particular, the phrase "the growth of knowledge" has been a favorite with him and with those he has influenced most. Some people have professed to find a difficulty, indeed, in understanding how there can be a growth-of-knowledge and yet no accumulation-of-knowledge.

There is accumulation-of-knowledge. Stove gives no cite, but I have a guess at what he's talking about. This quote is from C&R (*Conjectures and Refutations*) ch. 10 sec. 1, and there's a similar statement in LScD (*The Logic of Scientific Discovery*).

> it is not the accumulation of observations which I have in mind when I speak of the growth of scientific knowledge, but the repeated overthrow of scientific theories and their replacement by better or more satisfactory ones.

The growth of knowledge doesn't consist of accumulating ever more *observations* (we need ideas). Nor are we simply accumulating more and more ideas, because scientific progress involves refuting, replacing and modifying ideas too. The growth of knowledge is more about quality than quantity.

Continuing the same Stove passage:

> But then some people cannot or will not understand the simplest thing,

More ridicule.

> and we cannot afford to pause over them. Let us just ask, how does Popper use the word "knowledge"?

> Well, often enough, of course, like everyone else including our other authors, he uses it with its normal success-grammar. But when he wishes to give expression to his own philosophy of science he baldly neutralizes it. Scientific knowledge, he then tells us, is "conjectural knowledge". Nor is this shocking phrase a mere slip of the pen, which is what anywhere else it would be thought to be.

Expressing shock and talking about slips of the pen is not how one debates ideas seriously. But let's discuss *conjectural knowledge*.

Knowledge is good ideas. Sorting out good and bad ideas is one of the main problems in epistemology.

Conjectural serves two purposes. First, it indicates that knowledge is *fallible* (and lacks authority). Popper doesn't mean **justified, true belief**. He's not looking for perfect certainty or absolute guarantees against error.

Second, conjecture is the original *source* of the good ideas that constitute knowledge. Conjecture is, intentionally, an informal, tolerant, inclusive source. Even myths and superstitions can qualify as conjectures. There's no quality filter.

I think Stove's negative reaction has a thought process like this: No quality filter!? But we want *good* ideas. We *need* a quality filter or it's all just arbitrary! "Anything goes" can't achieve knowledge, it's irrationalism.

Popper has an answer:

Standard approaches do lots of quality filtering (sometimes all) based on the *source* of ideas.

Instead, *all* quality filtering should be done based on the *content* of ideas. This is done with *criticism* and *human judgement*, which lack *authority* but are good enough.

So we do have a quality filter, it's just designed differently and put in a different place.

For more, see Popper's introduction to C&R, *On the Sources of Knowledge and of Ignorance*. Excerpt from sec. XV:

> The question about the sources of our knowledge can be replaced in a similar way [to the 'Who should rule?' issue]. It has always been asked in the spirit of: ‘What are the best sources of our knowledge—the most reliable ones, those which will not lead us into error, and those to which we can and must turn, in case of doubt, as the last court of appeal?’ I propose to assume, instead, that no such ideal sources exist—no more than ideal rulers—and that *all* ‘sources’ are liable to lead us into error at times. And I propose to replace, therefore, the question of the sources of our knowledge by the entirely different question: ‘*How can we hope to detect and eliminate error?*’

Continuing the same Stove passage:

> On the contrary, no phrase is more central to Popper's philosophy of science, or more insisted upon by him. The phrase even furnishes, he believes, and as the title of one of his articles claims, nothing less than the "solution to the problem of induction" [28].

Note the lack of discussion of Popper's position.

> In one way this is true, and must be true, because any problem clearly must yield before some one who is prepared to treat language in the way Popper does. What problem could there be so hard as not to dissolve in a sufficiently strong solution of nonsense? And nonsense is what the phrase "conjectural knowledge" is:

More insults.

> just like say, the phrase "a drawn game which was won". To say that something is known, or is an object of knowledge, implies that it is true, and known to be true.

This is ambiguous on the key issue of fallibility.

Is Stove saying all knowledge must be infallible and known to be infallible? It must be the proven to be the perfect truth, with complete certainty, so that error is utterly impossible – or else it's not knowledge?

If that's Stove's view of knowledge, then I think he has a choice between irrationalism or skepticism. Because his demands cannot be met rationally.

Or if Stove's position is less perfectionist, then what is it? What allowances are made for fallibility and human limitations? How do they compare to Popper's allowances? And why is Popper mistaken?

> (Of course only `knowledge that' is in question here). To say of something that it is conjectural, on the other hand, implies that it is not known to be true.

Does "known to be true" here mean *infallibly proven*? Or what?

> And this is all that needs to be said on the celebrated subject of "conjectural knowledge"; and is a great deal more than should need to be said.

What's going on here is simple. Stove is scornful of a concept he doesn't understand. He doesn't appreciate or discuss the problems in the field. And he doesn't want to. He's unable to state a summary of Popper's view which a Popperian would agree with, and he wants the matter to be closed after three paragraphs.

Sabotaging Logical Expressions

Ch. 2:

> What scientists do in such circumstances, Popper says, is to act on a methodological convention to neglect extreme probabilities

For example, how do you know a coin which flips 1000 heads in a row is unfair? Maybe it's a fair coin on a lucky streak.

Well, so what? I'm willing to risk a 2^-1000 chance of misjudging the coin. I'm far more likely to be struck be lightning than get the coin wrong. And the downside of misjudging the coin is small. If the downside were so large that I couldn't tolerate that much risk, I could flip the coin additional times to reduce the risk to my satisfaction (assuming I get more heads, that reduces the probability it's a fair coin).

So Popper offers: if you judge it's not a worthwhile issue to worry about, then don't worry about it. This judgement, like everything, could be a mistake, so it's always held open to criticism. That openness doesn't mean we think it's mistaken

or spend our time searching for a mistake, it just means we recognize we have no infallible guarantee against error. We have to make fallible, criticizable judgements about what areas are problematic to focus attention on.

Stove dislikes this approach because he thinks you could do it to dismiss any problem. Stove fears arbitrarily creating a methodological convention to neglect any difficulty. The solution to this is criticizing bad methodological conventions. Stove (correctly) sees problems with some conventions that could be proposed, and those problems can be expressed as criticism.

The problem here is Stove's unfamiliarity with Popperian methods. Plus I think Stove wants methodological rules to guide thinking and reduce the scope for human judgement and creativity.

> ... Popper actually anticipated it. This is `the Quine-Duhem thesis': that "any statement can be held true come what may, if we make drastic enough adjustments elsewhere in the system [...]. Conversely, [...] no statement is immune to revision" [23].

There's an important logical point here. I wonder what Stove's answer to it is (he doesn't say). Popper offered some help with this issue, but not a full solution. That's OK because Popper's general approach of *fallible judgement combined with error correction* still works anyway.

Philosopher David Deutsch addressed the Quine-Duhem issue better. His **two books** offer refinements of Popper. (FoR ch. 1, 3, 7-8; BoI ch. 1-4, 10, 13.)

In short: You may try modifying whatever you want to rescue a statement, *but* those modifications have meaning and can be criticized. Ad hoc modifications commonly ruin the explanation which gave the idea value in the first place, or contradict vast amounts of existing knowledge without argument. If you can come up with a modification that survives immediate criticism, then it's a good contribution to the discussion (sometimes the error really is elsewhere in the system).

Other Thoughts

Ch. 3:

> It is a favorite thesis with him that a scientific theory is, not only never certain, but never even probable, in relation to the evidence for it [3].

Right, because logically there's no such thing as evidence *for* a theory. There's only evidence which does or doesn't contradict a theory. And any finite set of evidence is logically compatible with (does not contradict) infinitely many theories, and those theories reach basically *every* conclusion.

What does Stove think of this?

> These two theses [the one above and one other] will be acknowledged to be irrationalist enough; and they are ones upon which Popper repeatedly insists.

Stove doesn't present and discuss Popper's solution to the logical difficulties of positive support. Nor does Stove present his own solution. Instead he says it "will be acknowledged" that Popper's view is irrational, without argument. Stove treats it as if Popper only talked about this difficulty without also giving a solution. (The solution, in short, is that negative arguments don't face this difficulty.)

Ch. 3:

> Scepticism about induction is an irrationalist thesis itself

Rather than present and discuss Popper's solution to the problem of induction, Stove simply asserts that the only alternative to induction is irrationalism. He goes on to discuss Hume at length rather than Popper.

Ch. 5:

> One of these features, and one which is at first sight surprising in deductivists, is this: an extreme lack of rigor in matters of deductive logic.

Because Popper's main positions aren't about deduction. The technical reason that conjectures and refutations is able to create knowledge is that it's evolution, not deduction. The key to evolution is *error correction*, and that's also the key to Popper's philosophy, but Stove doesn't understand or discuss that. Stove only uses the word "evolution" once (in a Kuhn quote where it means *gradual development* rather than *replication with variation and selection*).

A core issue in Popper's philosophy is: "How can we hope to detect and eliminate error?" (as quoted earlier). Stove doesn't understand, present, or criticize Popper's answer to that question.

Note: My comments on Popperian thinking are summary material. There's more complexity. It's a big topic. There are **books of details**, and I can expand on particular points of interest if people ask questions.

Comments on "An Eye for An I: Philosophies of Personal Power"

Comments on **An Eye for An I: Philosophies of Personal Power**, primarily about Popper. "You" refers to the author, Robert Spillane, who I emailed.

I appreciated your comments on mistranslating Descartes on the "soul" as about "mind". I'd forgotten that idea. I learned that translation errors are a major issue from Popper. He found another major philosophical mistranslation:

The World of Parmenides, by Popper, in the Introduction:

> Plato says explicitly (in the Timaeus, for example, but also in other places) that all he can tell us is at best only ‘truthlike’ and not the truth: it is, at best, like the truth. This term is usually translated by ‘probable’ ... I use the term ‘truthlikeness’, or ‘verisimilitude’, especially for theories. The word that Plato uses is really ‘similar’, and sometimes he says ‘similar to truth’; the word is also connected with ‘pictorial similarity or likeness’, and this seems, indeed, to be the root of the meaning.[1] According to Plato, humans can have only this kind of knowledge; he rarely calls it opinion, which is the usual term used, for example by his contemporary Isocrates, who says ‘We have only opinion.’

Back to your book:

> Popper’s philosophy of critical rationalism has attracted widespread criticism because, despite his intentions, it leads to a radical scepticism.

I think you're mistaken about Critical Rationalism and I can defend it from skepticism. The broad issue is that one has to form a new, evolutionary understanding of what knowledge is. Without that, Popper appears to be a skeptic because he did reject some standard concepts of knowledge (not as a matter of taste, but because they just plain don't work).

One of Popper's main achievements was to reconcile knowledge with fallibility. The proof/skepticism false dichotomy had dominated philosophy since Aristotle, and isn't improved by equivocations about probability (99% proven doesn't actually make sense).

> Those who embrace Popper's worldview are concerned, if not obsessed with, deduction (since induction is a myth).

That's true of some of them. But it's not true of David Deutsch, myself, and the other Popperians I typically discuss with. (And I've found the others basically unwilling to discuss philosophy, so I don't think they matter.) I don't think it's true of Popper himself, either.

The basic reason people are attracted to deduction is to prove things. But someone who really understands Popper and fallibilism won't be so interested in proof. Popper himself was more interested in deduction early in his career (the LOGIC of scientific discovery) and less so in his better, later works.

A deductive proof is just as fallible as a standard English argument. Everyone knows what regular, commonsense arguments are. For example this argument is neither induction nor deduction: "Socialism doesn't work because there's no way to do rational economic calculation without prices. Is it better or more efficient to use up two tons of iron or two tons of aluminum in your project, or something else? Without prices you can't figure that out."

Rather than seek to prove things (deduction) or try to sorta approximate proof (as induction does), we should seek to explain and criticize. Which is what informal arguments often already do. So it's informal arguments which should matter most to Popperians!

By finding some of our errors and making fixes -- which can be done with informal arguments -- we can improve. This improvement is knowledge accumulation. It's not inductive. Deduction and logic do play a role sometimes, but aren't a primary focus.

Technically, knowledge is created by evolution. How knowledge is created is a very hard problem, and there have only been a handful of proposed solutions. Induction (wrong). Creationism (knowledge is magically created out of nothing). Design (knowledge is "created" by a designer who already contains all the

complexity, which leads to regress). Abduction (inductivist equivocations). And conjectures and refutations (which is a form of evolution).

Evolution isn't deduction (or induction). It's a process of replication with variation and selection. Ideas, like genes, can replicate. The information can be copied, just like duplicating a file on a hard drive or downloading it from someone else's website. The information can also be varied and selected (which is what brainstorming and critical argument are about). This is Popper's position, clarified by Deutsch and myself (Popper didn't have a fully modern understanding of evolution, computation and way information flows in quantum physics).

For some indication of the physics, see Deutsch's books and his paper:

http://beginningofinfinity.com/books

https://graphene.limited/services--technologies/physics-of-triggering/Trigger-Physics/0104033v1.pdf

Abstract: The structure of the multiverse is determined by information flow.

Relating epistemology to physics is important because, contra a lot of nonsense about the "mind", thinking and knowledge creation are physical processes.

Why does evolution create knowledge? This question relies on correctly understanding what knowledge is. Not proof. Not justified ideas. Not infallible ideas. Not induced ideas. etc. But what?

Knowledge is information which solves problems. It's useful information. It's information with some purpose, some design, some adaptation, so that it actually works to do something.

From here, along with the appropriate background knowledge, it's straightforward to see that evolution creates knowledge. Evolution gradually generates information more and more in line with the selection criteria. That is, it creates information about how to meet the selection pressure. That is, it creates knowledge about how to solve the problem of meeting that selection pressure(s).

This leads to a further issue which is universal knowledge vs. knowledge limited to a particular purpose. Some problems are dumb and their solutions aren't

valuable. Which I can answer if you like. It gets even further afield from standard philosophy into uniquely Popperian ways of thinking.

> Deductivism, in Popper’s hands, leads to the conclusion that we should prefer the best-tested theories: theories which have survived repeated attempts to falsify them. These theories are not true, but they are to be preferred to theories which have been progressively falsified or theories which have not been subjected to attempts to disprove them.

"These theories are not true" is an error. What Popper meant, and what's true, is, "We don't know for certain that these theories are true". Some of our ideas may in fact be true, but we can't ever prove it with 100% infallible certainty.

Popper's fallibilism is easy to confuse with skepticism because he denies the possibility of proven knowledge, certain knowledge, and justified true belief.

> Critics are bothered by the deep scepticism that infects Popper’s philosophy.

Using a medical metaphor ("infects") was a mistake. It's, as Szasz would have put it, the medicalization of everyday life.

> Theories are bold guesses riddled with uncertainty and science is a game. Understandably, we want to know upon which theory we should rely on rational grounds for practical action.

That's pretty simple: you should act on an idea you don't know a refutation of.

Why? Because you're trying to avoid error, and refutations consist of pointing out errors.

Rather than complaining about uncertainty, it's crucial to think in terms of error-correcting processes. Popper applied this insight to Democracy (fixing bad rulers and policies without violence is a type of error correction). And it comes up with computer filesystems. The raw data on disk is riddled with uncertainty due to the unavoidable possibility of hardware error. But our use of computers is NOT riddled with uncertainty, because of the use of error-correcting software algorithms involving parity bits, checksums, etc.

Our lives don't have to be riddled with uncertainty, either. We can't prevent all error, but we can keep error under control by using the right thinking methods.

> As for practical action, we should rely on the best-tested theory. But why should we prefer any theory at all? Indeed, why should we even accept the results of falsified experiments, for such an acceptance involves us in an inductive inference (an experiment falsified today will achieve the same result tomorrow)?

Remembering and using the results of past tests does not rely on a "the future will resemble the past" style inductive principle.

It instead is based on explanations of physics which say what sorts of changes happen and don't happen. This gives us an understanding of what kinds of changes to expect, or not, on what timeframes. As a simple example, the speed of light limit means I shouldn't expect a person standing a light-second away to change their mind in under one second after I come up with a great new argument.

Our understanding of the world involves many layers of abstraction on top of physics. At a higher level, we understand things like what forces exist and what kinds of things could or could not split the Earth in two. It'd take a huge amount of force to do that, and we know what kinds of physical processes can and can't create that force. So we don't have to worry that our footsteps will break the Earth. Not because the future will resemble the past, but because we understand the material structure of the Earth, its density, the energy bonding the atoms and molecules together, the energy required to separate that much matter in that configuration, etc.

Our understanding of physics used experimental tests in a critical role. We criticize ideas which contradict experiment.

It's up to a theory to say whether it applies at all times, or not.

A theory is welcome to say e.g. "The following is how the physical world worked in the 1900s, and the following is how it will work in the 2000s". But a theory can also say "This is how the physical world works in the 1900s and the 2000s and all other centuries."

An experiment done in the 1900s can refute, or not refute, either of those theories. They also both make predictions telling us what to expect in the future. The difference is one of them predicts the same experiment, done in 2017, will have the same result it had in 1917, and the other says the rules have changed over time and now it will get a different result.

Rather than assuming the future will resemble the past, we have hypotheses which claim it in particular respects, or don't. We then criticize those hypotheses. And lots of that criticism is non-empirical. We ask critical questions like WHY the laws of physics would suddenly and discontinuously change when the millennium passes on our calendar. If there is no answer, we reject that hypothesis as a bad explanation.

> The empirical basis of objective science has nothing absolute about it. Science does not rest upon bedrock: it rises above a swamp.

Yes, foundations are highly overrated in philosophy. You can start anywhere and build up solutions to the problems layer by layer. Rather than seek an error-free starting place, we must accept we are fallible and errors are inevitable. Then we must recognize that errors are fixable, and start solving our problems. A swamp can be drained, or a platform can be built on top of it, etc. No matter where we start our inquiry, there will be problems in need of solving, rather than certainty that allows us to relax and retire with no more need for effort.

> Popper does not seem too distressed to admit that the acceptance or rejection of observation statements ultimately rests on a decision reached through a process much like trial by jury.

Yes, trial by jury is a reasonable metaphor. Arguments are presented and judgements are made. That's gotten us into space, built skyscrapers and iPhones, etc. It works. As opposed to the alternatives which, rather than considering how to deal with the human condition, yearn for a different world with different rules and lament, and encourage the skeptics by saying that human judgement isn't good enough and needs to be aided by something to give it more certainty. (And then the skeptics see, correctly, that the "something" offered doesn't actually work.)

> Popper tells us that science is neither a system of well-established statements, nor is it a system which steadily advances towards the truth.

That's unfair. Popper tells us science is a system which unsteadily advances towards the truth. Scientific breakthroughs don't come on a regular schedule, but they do happen.

Popper also says we never know how close to the truth we are, on an absolute scale. But that doesn't stop us from getting closer to it.

> Science, he says, can never claim to have attained truth, or even a substitute for it, such as probability.

We can claim to have attained knowledge, which is a substitute for truth.

That knowledge is fallible, tentative (could be reconsidered in the future) and conjectural (based on human guesses, rather than methodically built up from foundations offering certainty).

Comments on: "Personality or performance?: The case against personality testing in management"

Robert Spillane **wrote**:

> Szasz's argument can be supported empirically by the many Australian work organisations whose managers secure psychological profiles on their subordinates despite overwhelming evidence that psychological (especially personality) tests have consistently and strikingly failed to predict work performance (Spillane, 1994).

I was particularly interested in this evidence. Psychologist **Jordan Peterson** (whose videos I generally like) has claimed the research shows that personality tests *do* correlate with various life outcomes. For example, he said agreeableness correlates to doing well at university (teachers like people who agree with them and grading is biased). I'd like to know if he's wrong about the correlation research (which I know is very different than understanding what's actually going on).

Peterson specifically says the **big five** personality traits (openness, conscientiousness, extraversion, agreeableness and neuroticism), plus IQ, are the important ones. He says psychologists don't check if their constructs are accounted for by the big five plus IQ because then they'd find out they haven't invented anything, they've just found a proxy for something that's already been discovered.

Peterson says they discovered these traits by asking people a wide variety of questions and finding the answers to some groups of questions are correlated. That is, if you give some conscientious answers you're likely to give other conscientious answers too. The point is that different questions are related, and the questions about personality ending up statistically falling into five groups.

Note that psychologists cannot be trusted to make true statistical claims. For example, the big five wikipedia page says:

> Genetically informative research, including twin studies, suggest that heritability and environmental factors both influence all five factors to the same degree.[71] Among four recent twin studies, the mean percentage for heritability was calculated for each personality and it was concluded that heritability influenced the five factors broadly. The self-report measures were as follows: openness to experience was estimated to have a 57% genetic influence, extraversion 54%, conscientiousness 49%, neuroticism 48%, and agreeableness 42%.[72]

I know this is bullshit because I've researched heritability and twin studies before. (**Yet More on the Heritability and Malleability of IQ** is very good.) They define "heritability" to refer to a mathematical correlation which doesn't imply anything is passed down genetically from your parents. They do this to be misunderstood, on purpose, by people who think they're talking about the standard English meaning of "heritability". And their twin studies don't address gene-culture interactions, and they know that and dishonestly ignore it. They also look at the *variation* in traits, rather than the cause of the traits themselves (e.g. they would study why you're a little bit happier than some other people, then announce they found a gene controlling happiness.)

For example of a gene-culture interaction, a gene for being taller could correlate to basketball success. That doesn't actually mean that basketball success is genetically passed down. Becoming a good basketball player depends on cultural factors like whether basketball is popular in your society or even exists. Nevertheless they will correlate some gene to basketball success and announce they've discovered basketball skill is 60% hereditary. And they will imply this is *determined* by your genes and outside your control, and that it couldn't be changed by writing a popular blog post with new ideas. But that's completely false and the "heritability" they study says nothing about what interventions would be successful in changing the results. (In other words, when they say a trait is 60% genetically determined, that actually allows for the possibility that an essay would change 100% of the trait. The more educated psychologists know that and making misleading statements anyway because they believe these kinds of caveats don't really matter and the bulk of their conclusions are about right.)

So I read Spillane's paper: *Personality or performance?: The case against personality testing in management*[1]:

> The failure of psychologists to produce laws of behaviour or discoveries of importance has stimulated the study of behaviour called *reductionism*.

Reductionism is refuted in **The Fabric Of Reality**, ch. 1.

> To explain introverted behaviour by reference to an 'introvert trait' in a person betrays an insensitivity to logic since the 'explanation' is viciously circular.

Introvert is a loose *description* or *label*. It's a shortcut which condenses and summarizes many facts. E.g. I observed Joe reading a book instead of socializing three times, and I remember one word instead of three events.

I don't think "introverted" is very useful (it's too vague). But shortcut labels in general are OK, e.g. "Popperian", "Aristotelian", or "Kantian". These are more specific than "introverted" and I find them useful despite some ambiguity.

An *explanation* says *why, how, or because*. But calling someone introverted doesn't say *why* they're introverted. An explanation would say, "Joe is introverted *because* ..." It would then give a *reason*, e.g. because Joe found that many people are mean to him because he likes books. After you understand the reason for behavior, you can make better predictions. E.g. you won't be surprised if Joe is more outgoing at a book club meeting.

> insurmountable problems for those who explain, say, the behaviour of individuals who withdrew their labour by reference to the traits 'aggression' or 'apathy'

"He didn't do much yesterday because he's apathetic" isn't an explanation. It's just a restatement with a synonym. Apathetic means not doing much. But *why* doesn't he do much?

This reminds me of: people often say they do stuff because they *enjoy* or *like* it. They find it *fun* or *entertaining*. And they act like that is an explanation and settles the matter. But why do they like it? What's fun about it? Often they are bad at introspection, uninterested in self-understanding, and don't know.

> Maslow's hypotheses have been vigorously tested and the results, far from supporting his theory, have invalidated it This would not have surprised Maslow himself who was *bothered by the way his conjectures were so*

> *readily accepted* as true and paraded as the latest example of erudite knowledge in management [emphasis added]

Sad story.

> The results of personality tests, to which I now turn, are *communications*, not traits or needs, and they are particularly responsive to the demands of the social situation in which individuals are expected to perform. After decades of personality testing we can now say with confidence that the search for consistent personality and motivational traits has been strikingly unsuccessful (Mischel 1968). While self-descriptions on trait measures are reasonably consistent over short periods of time these measures change across social settings (Anastasi 1982). In other words, people answer questions about hypothetical situations in a reasonably consistent fashion, but when it comes to behaving in the world—the way the situation is perceived—the rewards and penalties obtained and the power one is able to exert influence the consistency of behaviour. It is not surprising, therefore, that efforts to predict performance from personality and motivational inferences have been consistently and spectacularly unsuccessful (Blinkhorn 1990; Fletcher, Blinkhorn & Johnson 1991; Guion & Gottier 1965).

The relevant part! It'd be a lot of work to check those cites though. Let's see what details Spillane provides.

> For more that 30 years researchers have stated unequivocally that they cannot advocate the use of personality tests as a basis for making employment decisions about people (Guion & Gottier 1965; Guion 1991). Where significant predictable findings are reported they are barely above chance occurrence and **explain** only a small proportion (less than 10%) of the variance in behaviour which ‘is incredibly small for any source which is considered to be the basis of behavioural variations’ (Hunt 1965, p 10). [emphasis added]

This use of the word "explain" is standard in these fields and really bad. They use "explain" to talk about *correlations*, contrary to standard English. In regular English, explanations tell you *why, how or because*. The implication when they say "explain" is it's telling you *why* – that is, it's telling you about *causes*. But correlations aren't causes, so this use of language is dishonest.

The rest looks good.

> In the face of low validity coefficients

This reminds me of Jordan Peterson who said psychologists used to underestimate their findings because the correlation coefficients they found were low. But then someone figured out to compare coefficients to other psychology research and call the top 25% of coefficients high no matter how low the actual numbers are! He thought this was a good idea. It reminds me of how poverty is now commonly defined to refer to relative poverty (being poorer than other people, no matter how wealthy you are).

> On comparing three respected and widely used personality tests, two researchers found ‘little evidence that even the best personality test predict job performance, and a good deal of evidence of poorly understood statistical methods being pressed into service to buttress shaky claims (Blinkhorn & Johnson 1990, p 672).

Doh.

> Poor validity is matched by poor internal consistency and test-retest reliability. In Cattell’s (1970) 16 personality factors, for example, only two out of 15 Alpha coefficients of internal reliability reach a statistically acceptable level, so testers cannot know what exactly the test has measured. This finding is not surprising given the vagueness of trait definitions and the fact that factor analysis ‘is a useful mathematical procedure for simplifying data but it does not automatically reveal basic traits. For example, the personality factors identified from ratings may partly reflect the rater’s conceptual categories’ (Mischel 1971).

Of course personality trait categorizations reflect the conceptual categories of the people who invented them. They chose what they thought was a question about personality to ask about in the first place.

It's like IQ tests, which all have a heavy cultural bias. So they don't accurately measure intelligence. But that doesn't necessarily make them worthless. Despite the bias, the results may still correlate to some types of success within the culture the tests are biased towards. In other words, an equally smart person who isn't as familiar with our culture will get a lower IQ score. But he may also, on

average, go on to have less success (at getting high university grades or getting a high income) since he doesn't fit in as well.

IQ tests also deal with outliers badly. Some people are too smart for the test and see ambiguities in the questions and have trouble guessing what the questioners meant. Here's an example from testing the child of a friend of mine. They were asked what a cow and a pig have in common. And they tried answers like "mammal" or "four legs" or "both are found on farms". Nope, wrong! The right answer was they were both "animals". The child was too smart for the test and was marked wrong. The child was only told the right answer after the test was over, so they got a bunch of similar questions wrong too... Similarly, I recall reading the Richard Feynman scored like 125 on an IQ test, which is ridiculously low for him. He's the kind of person you'd expect to easily break 175 if the tests were meaningful that far from 100, which they aren't.

> The technical deficiencies of most personality tests have been known for many years. Yet they are conveniently ignored by those with vested interests in their continued use. For example, the Edwards Personal Preference Scale is technically deficient in form and score interpretation and rests on poorly designed validation studies (Anastasi 1982). The limitations of the Myers-Briggs Temperament Indicator are well known: 'The original Jungian concepts are distorted, even contradicted; there is no bi-modal distribution of preference scores; studies using the MBTI have not always confirmed either the theory or the measure' (Fumham 1992, p 60).

Cool. I may look those papers up. I'd really like one for the big five, though!

> Testers rely on the validity of self-reports and assume that subjects have sufficient self-insight to report their feelings and behaviour accurately. However, evidence has shown that respondents frequency lack appropriate levels of self-awareness or are protected from exposing themselves by an army of defence mechanisms (Stone 1991).

Of course. So personality tests don't measure your *real personality* anymore than IQ tests measure your *real intelligence*. But, it could still be the case that people who claim to be agreeable on personality tests do better at university, on average (though without knowing *why* you can't understand what changes to our society would ruin the effect). One of the reasons I was interested by Peterson's

comments on personality tests is he said basically the correlations exist and therefore there's something going on there even if we don't know what it is, and he's admitted that some of the big five personality traits aren't really understood, they are just names tacked on to the correlation which is the real discovery.

Correlations are *worthless* without any explanation. But they do have some explanatory context to put these correlations in. We already knew that some of people's communications reveal information about their preferences and skills. And it's not just what people openly say that matters, sometimes subtle clues are revealing. In that context, it could theoretically be possible to correlate some communications to some outcomes. It's like reading between the lines but then statistically checking if you're right very often or not.

> Then there is the problem of faking which is so widespread that it is amazing that test scores obtained under conditions of duress or vested interest are taken seriously. The use of so called objective self-report tests requires the assumption that the subject’s score is free from artifacts that superficially raise or lower scores.

Yet many researchers list studies which show that personality tests are especially subject to faking (Anastasi 1982; Goldstein & Hersen 1990; Hogan 1991). So serious is this problem that one of the world’s best known personality psychologists, H J Eysenck (1976), will not endorse the use of his personality test where there is a vested interest in obtaining a particular result. Australian researchers have expressed similar reservations about the use of Cattell’s 16 personality factors in selection situations (Stone 1991; Spillane 1985). Yet the testing continues in the absence of countervailing evidence.

Right. I only had in mind only voluntary, confidential tests for personal use. If the test can affect getting a job offer, a raise, or college admissions, then of course people will lie. (People are really bad at introspection and personality tests could be a starting point for them to think about themselves. Yes a biased starting point, but still potentially useful for people who have no idea how to do better. I took some online personality tests in the past and found them interesting to think about. That's in the context of my belief that personality is changeable anyway. I never interpreted the tests as doing anything like authoritatively pronouncing what my life will be like, nor did I expect them to be unbiased or highly accurate.)

> The claim that lie scales built into the tests weed out fakers is an insult to the intelligence of those who are subjected to them. Whyte (1956) explained 38 years ago how to fake these tests by summarising the strategies employed by bright people to make fools of the testers.

That sounds interesting. At least the test faking strategies. I bet if I look it up, the "lie scales" will boringly naive.

> Then there is the question of cross-cultural applicability, fairness and discrimination. Most personality tests are derived from an Anglo-American environment and are therefore culturally biased. Such tests have been found to be sexually and racially discriminating (Anastasi 1982; Fumham 1992).

Of course they are. That doesn't make them worthless though. If your company is sexist and racist, then the white male who gets a higher test score may actually do better at your company... (Or have they updated the tests yet to promote "diversity" by biasing them in favor of brown females?)

Also, as far as hiring goes, I believe companies should use *work sample tests*. Typical interviews are extremely biased to find people who are socially-culturally similar to the interviewer, rather than people who would do a good job. It's also biased to outgoing people who are relaxed, rather than nervous, during interviews. Current hiring practices are so bad that many people are hired for programming positions who **can't write working code**. The trivial **FizzBuzz** work sample test actually improves hiring because the other hiring criteria being used, like interviews, are worthless.

> Test scores can be interpreted in many ways. The most logical interpretation is that they reflect strategies adopted by the subject for the testing game. To argue that these strategies will necessarily equate to strategies adopted in the world of business is dishonest or naive.

Right. If you take a test because of personal curiosity, then you can try to answer honestly and see if the results say anything you find interesting. If personality tests were used for college admissions, then they'd be a test just like the SAT where you can read books telling you how to give answers that will get you admitted. It'd be funny if people wanted to retake a personality test to try again to get a better score, as they do now with the SAT.

> Personality tests assess generalised attitudes and gloss over the rich subtleties of human behaviour.

Of course! Isn't that what they're supposed to do? They are trying to summarize a person – which is very complex – with e.g. scores on 5 continuums. Summary information like that necessarily loses a lots of detail. Does anyone deny it!?

> Nowadays it is commonplace to hear apologists for personality testing admit that the tests don’t predict performance, but should be used nonetheless to ensure an appropriate fit of individual with organisational culture.

Seeking *cultural fit* at a company is one of the main excuses for not basing hiring primarily on work sample tests.

If companies cared more about work performance, they would come up with objective measures allowing no managerial discretion and then hand out bonus pay accordingly. (Some do this, many don't.)

> ... foist their crude ideas about human nature on to people who frequently don’t have the opportunity to assess their claims or to refuse to participate in the testing game.

A friend of mine got bored while taking an IQ tests and skipped the rest of the questions.

I got bored while taking a physics test at school, so I left most of it blank. The teacher didn't want to try to explain to anyone why a smart student who knew the material got a bad grade. Why rock the boat? So he just ignored the test result and asked me to take it again later. Grade falsification like this is common, and the amount of grade falsification depends on the teacher's opinion of you. A friend of mine went through school making friends with his teachers and then turning in many of his assignments weeks late and getting A's anyway.

> No doubt one of the reasons for the continuing belief in personality traits and the instruments used to 'measure' them is the result of an outmoded inductivist view of science which emphasises confirming instances.

Yes. And induction is closely related to correlation. Induction involves picking out some pattern (correlation) from a data set and then extrapolating without

thinking about explanations of the causal mechanisms. We know the sun will rise tomorrow because we know what it's made out of and what forces (gravity and the Earth's spin) are involved, not because of the correlation between 24 hours passing and the sun rising again.

But induction doesn't work because, among other reasons, there are always infinitely many patterns (and also explanations) which fit any finite data set. So too are there infinitely many patterns to be found in personality test data, and infinitely many explanations compatible with the test results. It's only by critical thinking about explanations that we can understand what's going on. Data can't guide us (contrary to the common claim that *correlations hint at causations*), we have to guide ourselves using data as a tool.

Final Comments

Even without any tests, people often use their personality as an excuse. They say they can't do some task well (e.g. go to a party and do business networking) because they are "introverted". Or, rather than simply say they don't like networking (and perhaps even giving some reasons), they say it *makes* them nervous or anxious because of its incompatibility with their personality type.

*Many people would prefer to be **victims** of their personality, to have an **excuse** for their failures, rather than strive to better themselves.*

Footnotes

1: Spillane, R. (1994) 'Personality or Performance? The Case Against Personality Testing in Management.' In A.R. Nankervis & R.L. Compton (eds) Strategic Human Resource Management, Melbourne: Nelson, Ch 14.

Update

Spillane comments on the Big Five in his book **Psychomanagement: An Australian Affair**:

> In connection with any quantity which varies, such as job performance, the variation does not arise solely because of differences among personalities. So the correlation coefficient is used as an indicator of the proportion of the

variation in job performance, which is related to personality scores. The correlation thus provides an answer to the question: how much does the variation in personality contribute to the variation in job performance? This question may be answered in terms of variance. The square of the correlation coefficient indicates the proportion of variation in job performance, which is accounted for by differences in personality scores. If the correlation between job performance and personality scores is +.9 then the proportion of variance accounted for is 81% (and 19% is unaccounted for) and personality would be a very strong predictor of job performance. For 50% of the variance of job performance to be accounted for by personality, a correlation coefficient of just over +.7 is required. Since important employment decisions are based on the assumption that personality scores predict job performance, one would expect and hope that the correlation coefficients are greater than +.7 otherwise decision makers will make a large number of rejecting and accepting errors.

What have the meta-analyses found? Four meta-analytic studies of the relationship between job performance and personality scores yielded the following average correlation coefficients: conscientiousness .21; neuroticism .15; extraversion .13; agreeableness .13; openness to experience .12. These results are worrying enough since the much-quoted result of .21 for conscientiousness means that the proportion of variance unaccounted for is 95.6%. Responsible decisions about hiring, promotion or training cannot be made on the basis of these figures.

However, the actual situation is far worse since it makes an important difference to the results when personality scores are correlated with ‘hard’ or ‘soft’ performance criteria. Soft criteria include subjective ratings whereas hard criteria include productivity data, salary, turnover/tenure, change of status. Since personality scores are better predictors of subjective performance ratings than objective performance measures, it is reasonable to conclude that raters rely on personality when evaluating job performance, thereby raising the question whether the relationship between personality and performance is the result of the bias of the rater rather than actual performance. In the much-quoted study by Barrick and Mount, the correlation coefficient dropped from .26 for soft criteria to .14 for hard criteria. The average correlation between the Big Five and job performance (hard criteria) was .07.[2]

The footnote is:

> M.R. Barrick & M.K. Mount, ‘The Big Five Personality Dimensions and Job Performance: A Meta-Analysis’, Personnel Psychology, 1991, 44, pp. 1-26.

That article is freely **available online**. I read some and Spillane seems to be factually correct. It looks like Jordan Peterson is badly wrong.

25 Robert Spillane Replies

Robert Spillane (RS) is a philosopher who worked with Thomas Szasz for decades. He comments on Critical Rationalism (CR) in his books. I think he liked some parts of CR, but he disagrees with CR about induction and some other major issues. Attempting to clear up some disagreements, I sent him a summary of CR I wrote (not published yet).

Previously I **criticized a David Stove book he recommended**, responded to him about **RSI** (we agree), **replied positively** to his article on personality tests, **explained a Popper passage** RS didn't understand, and wrote some **comments about Popper** to him.

RS replied to my CR article with 25 points. Here are my replies:

> I am reluctant to comment on your article since it is written in a 'popular' style - as you say it is a summary article. Nonetheless, since you ask.....

I think writing in a popular (clear and readable) style is *good*. I put effort into it.

Speaking of style, I also think *heavy use of quoting is important to serious discussions*. It helps with responding more precisely to what people said, rather than to the gist of it. And it helps with engaging with people rather than talking past them.

(I've omitted the first point because it was a miscommunication issue where RS didn't receive my Stove reply.)

> 2. Your summary article is replete with tautologies which, while true, are trivial. The first paragraph is, therefore, trivial. And from trivial tautologies one can only deduce tautologies.

I'm not trying to approach philosophy by deduction (or induction or abduction), which I consider a mistaken approach.

Here's the paragraph RS refers to:

> Humans are *fallible*. That means we're capable of being mistaken. This possibility of making a mistake applies to everything. There's no way to get a *guarantee* that one of your ideas is true (has no mistakes). There's no guaranteed way to limit where a mistake could be (saying this part of my idea could be mistaken but not that part) or the size a mistake could be.

This makes claims which I believe most people disagree with or don't understand, so I disagree that it's trivial. I think it's an important position statement to differentiate CR's views from other views. I wish it was widely considered trivial!

I say, "There's no way to get a guarantee that one of your ideas is true". I don't see how that's a tautology. Maybe RS interprets it as being a priori deducible from word definitions? Something like that? That kind of perspective is not how I (or Popper) approach philosophy.

I wrote it as a statement about how reality actually is, not how reality logically must be. I consider it contingent on the laws of physics, not necessary or tautological. I didn't discover it by deduction, but by critical argument (and even some scientific observations were relevant). And I disagree with and deny the whole approach of a priori knowledge and the analytic/synthetic dichotomy.

> 3. Why are informal arguments OK? What is an example of an informal argument? It can't be an invalid one since that would not be OK philosophically, unless one is an irrationalist.

An example of an informal argument:

Socialism is a system of price controls. These cause shortages (when price ceilings are too low), waste (when price floors are too high), and inefficient production (when the controlled prices don't match what market prices would be). Price floors cruelly keep goods out of the hands of people who want to purchase the goods to improve their lives, while denying an income to sellers. Price ceilings prevent the people who most urgently need goods from outbidding others for those goods. This creates a system of first-come-first-serve (rather than allocating goods where they will provide the most benefit), a shadow market system of friendships and favors (to obtain the privilege of buying goods), and a black market. Socialism sacrifices the total amount wealth

produced (which is maximized by market prices), and what do we get in return for a reduction in total wealth? People are harmed!

Szasz's books are full of informal arguments of a broadly similar nature to this one. He doesn't write deductions, formal logic, and syllogisms.

Informal arguments are invalid in the sense that they don't conform to one of the templates for a valid deduction. I don't think that makes them false.

I don't think it's irrationalism to think there's value and knowledge in that price controls argument against socialism, even though it's not a set of syllogisms and doesn't reduce to a set of syllogisms.

The concept of formal logic means arguments which are correct based on their form, regardless of some of the specifics inserted. E.g. All X are Y. Z is X. Therefore Z is Y.

The socialism argument doesn't work that way. It depends on the specific terms chosen. If you replace them with other terms, it wouldn't make sense anymore. E.g. if you swapped each use of "floor" and "ceiling" then the argument would be wrong. Or if you replaced "socialism" with "capitalism" then it'd be wrong because capitalism doesn't include price controls.

The socialism argument is also informal in the sense that it's fairly imprecise. It omits many details. This could be improved by further elaborations and discussion. It could also be improved with footnotes, e.g. to George Reisman's book, **Capitalism: A Treatise on Economics**, which is where I got some of the arguments I used.

Offering finite precision, and not covering every detail, is also something I consider reasonable, not irrationalist. And I'd note Szasz did it in each of his books.

Informal arguments are OK because there's nothing wrong with them (no criticism refuting their use in general – though some are mistaken). And because informal arguments are useful and effective for human progress (e.g. science is full of them) and for solving problems and creating knowledge.

> 4. I wasn't aware that there was A key to philosophy of knowledge (metaphor?). And how is 'fixing' mistakes effective if we are condemned to

> fallibility?

It's not a metaphor, it’s a dictionary definition. E.g. **OED** for key (noun): "A means of understanding something unknown, mysterious, or obscure; a solution or explanation.”

What does RS mean “condemned” to fallibility? If one puts effort into detecting and correcting errors, then one can deal with errors effectively and have a nice life and modern science. There’s nothing miserable about the ongoing need for critical consideration of ideas.

In information theory, there are methods of communicating with arbitrarily high (though not 100%) reliability over channels with a permanent situation of random errors. The mathematical theory allows dealing with error rates up to but not including 50%! In practice, error correction techniques do not reach the mathematical limits, but are still highly effective for enabling e.g. the modern world with hard disks and internet communications. (Source: **Feynman Lectures On Computation**, ch. 4.3, p. 107)

The situation is similar in epistemology. Error correction methods like critical discussion don't offer any 100% guarantees, nor any quantifiable guarantees, but are still effective.

> 5. Critical rationalists leave themselves open to the charge of frivolity if they maintain that the 'sources of ideas aren't very important'. How is scientific progress possible without some 'knowledge' of ideas from the past?

Learning about and building on old ideas is fine.

The basic point here is to judge an idea by what it says, rather than by who said it or how he came up with it.

You may learn about people from the past because you find it interesting or inspiring, or in order to use contextual information to better understand their ideas. For example, I read biographies of William Godwin, his family, and Edmund Burke, in order to better understand Godwin’s philosophy ideas (and because it’s interesting and useful information).

> 6. Why must we be tolerant with, say, totalitarians? Do you really believe that Hitler could be defeated through argumentation?

I think Hitler could easily have been stopped without violence if various people had better ideas early enough in the process (e.g. starting at the beginning of WWI). And similarly the key to our current struggles with violent Islam is philosophical education –- proudly standing up for the right values. The mistaken ideas of our leaders (and most citizens) is what lets evil flourish.

> 7. One of the most tendentious propositions in philosophy is 'There is a real world.' Popper's 'realism' is Platonic.

So what if it's "tendentious"? What's the point of saying that? Is that intended to argue some point?

Popper isn't a Platonist and his position is that there is a real, objective reality and we can know about it. I was merely stating his position. Sample quote (*Objective Knowledge*, ch. 2.3, p. 36):

> And Reid, with whom I share adherence to realism and to common sense, thought that we had some very direct, immediate, and secure perception of external, objective reality.

Popper's view is that there is an external, objective reality, and we can know about it. However, all our observations are theory-laden – we have to think and interpret in order to figure out what exists.

> 8. How can an idea be a mistake if its source is irrelevant?

Its content can be mistaken. E.g. "2+3=6" is false regardless of who writes it.

RS may be thinking of a statement like, "It is noon now." Whether that's true depends on the context of the statement, such as what time it is and what language it's written in. Using context to understand the meaning/content of a statement, and then judging by the meaning/content, is totally different than judging an idea by its source (such as judging an idea to be true or probably true because an authority said it, or because the idea was created by attempting to follow the scientific method).

> 9. One of the many stupid things Popper said was 'All Life is Problem Solving'. Is having sexual intercourse problem-solving? Is listening to Mozart problem-solving?

Yes.

RS calls it stupid because he don't understand it. He doesn't know what Popper means by the phrase "problem solving". Instead of finding out Popper's meaning, RS interpreted that phrase in his own terminology, found it didn't work, and stopped there. That's a serious methodological error.

Having sex helps people solve problems related to social status and social role, as well as problems related to the pursuit of happiness.

Listening to Mozart helps people solve the problem of enjoying their life.

The terminology issue is why I included multiple paragraphs explaining what CR means in my article. For example, I wrote, "[A problem] can be answering a question, pursuing a goal, or fixing something broken. Any kind of learning, doing, accomplishing or improving. Problems are opportunities for something to be better."

Despite this, RS still interpreted according to his own standard terminology. Understanding other perspectives, frameworks and terminology requires effort but is worthwhile.

The comment RS is replying to comes later and reads:

> Solving problems always leads to a new situation where there’s new problems you can work on to make things even better. Life is an infinite journey. There’s no end point with nothing left to do or learn. As Popper titled a book, *All Life is Problem Solving*.

I brought up *All Life is Problem Solving* because part of its meaning is that we don't run out of problems.

> 10. 'All problems can be solved if you know how' is a tautology and has no contingent consequences.

It's not a tautology because there's an alternative view (which is actually far more popular than the CR view). The alternative is that there exist insoluble problems (they couldn't be solved no matter what knowledge you had). If you think that alternative view is wrong on a priori logical grounds, I disagree, I think it depends on the laws of physics.

> 11. 'Knowledge is power' entails 'power is knowledge' which is clearly false as an empirical generalisation.

"Knowledge is power" is a well known phrase associated with the Enlightenment. It has a non-literal meaning which RS isn't engaging with. See e.g. Wikipedia: **Scientia potentia est**.

I would be very surprised if RS is unfamiliar with this phrase. I don't know why he chose to split hairs about it instead of responding to what I meant.

> 12. 'If you have a correct solution, then your actions will work' is a tautology.

It's useful to point out because some people wouldn't think of it. If I omitted that sentence, some readers would be confused.

> 13. 'Observations play no formal role in creating ideas' is clearly false. Semmelweis based his idea about childbirth fever on observations and inductive inferences therefrom.

RS states the CR view is "clearly false". That's the fallacy of begging the question. Whether it's false is one of the things being debated.

Rather than assume CR is wrong, RS should learn or ask what CR's interpretation of that example is (and more broadly CR's take on scientific discovery). Popper explained this in his books, at length, including going through a variety of examples from the history of science, so there shouldn't be any mystery here about CR's position.

I don't think discussing this example is a good idea because it's full of historical details which distract from explaining issues like why induction is a myth and what can be done instead. If RS understood CR's position on those issues, then he could easily answer the Semmelweis example himself. It poses no particular challenge for CR.

Anyone who can't explain the Semmelweis example in CR terms is not adequately familiar with CR to reject CR. You have to know what CR would say about a scientific discovery like that *before* you decide CR is "clearly false".

> 14. 'Knowledge cannot exist outside human minds'. Of course it can if there are no human minds. I agree with Thomas Szasz who, in 'The Meaning of Mind' argued that while we are minded (mind the step) we do not have minds. 'Mind' should only be used as a verb, never as a noun. Popper's mind-body dualism is bad enough, but his pluralism is embarrassing.

I wrote "Knowledge can exist outside human minds." and this changes "can" to "cannot". RS, please use copy/paste for quotes to avoid misquotes.

I'm not a dualist.

It's fine to read my statement as "Knowledge can exist outside human brains" or outside people entirely. The point is knowledge can exist separate from an intelligent or knowing entity.

> 15. 'A dog's eyes contain knowledge'. I don't understand this since to know x is to know that x is true. Since truth is propositional, dogs don't have to deal with issues of truth. Lucky dogs!

CR disagrees with RS about what knowledge is, and claims e.g. that there is knowledge in books and in genes. Knowledge in genes has nothing to do with a dog knowing anything.

RS, what is your answer to Paley's problem? And what do you think genetic evolution creates?

> 16. Your use of 'knowledge' is somewhat eccentric if you claim that trees know that x.

I don't claim trees know anything, I claim that the genes in trees have knowledge of how to construct tree cells.

CR acknowledges its view of knowledge is non-standard, but nevertheless considers it correct and important.

17. 'Knowledge is created by evolution' is a tautology if we accept a liberal interpretation of 'created'. If we do not and we assume strict causation, it is false.

That knowledge can be created by evolution is contingent on the laws of physics, not tautological. RS does not state what the "liberal interpretation" he refers to is, nor what "strict causation" refers to, so I don't know how to answer further besides to request that he provide arguments on the matter (preferably arguments that would persuade me that RS understands evolution).

18. Ideas cannot literally replicate *themselves*.

This is an unargued assertion. Literally, they can. I think RS is simply concluding something is wrong because he doesn't understand it, which is a methodological error.

David Deutsch has explained this matter in *The Fabric of Reality*, ch. 8:

> a replicator is any entity that causes certain environments to copy it.
>
> ...
>
> I shall also use the term *niche* for the set of all possible environments which a given replicator would cause to make copies of it....
>
> Not everything that can be copied is a replicator. A replicator *causes* its environment to copy it: that is, it contributes causally to its own copying. (My terminology differs slightly from that used by Dawkins. Anything that is copied, for whatever reason, he calls a replicator. What I call a replicator he would call an *active* replicator.) What it means in general to contribute causally to something is an issue to which I shall return, but what I mean here is that the presence and specific physical form of the replicator *makes a difference* to whether copying takes place or not. In other words, the replicator is copied if it is present, but if it were replaced by almost any other object, even a rather similar one, that object would not be copied.
>
> ...
>
> *Genes embody knowledge about their niches.*

> ...
>
> It is the *survival of knowledge*, and not necessarily of the gene or any other physical object, that is the common factor between replicating and non-replicating genes. So, strictly speaking, it is a piece of knowledge rather than a physical object that is or is not adapted to a certain niche. If it is adapted, then it has the property that once it is embodied in that niche, it will tend to remain so.
>
> ...
>
> But now we have come almost full circle. We can see that the ancient idea that living matter has special physical properties was almost true: it is not living matter but *knowledge-bearing* matter that is physically special. Within one universe it looks irregular; across universes it has a regular structure, like a crystal in the multiverse.

Add to this that ideas exist physically in brain matter, (in the same way data can be stored on computer disks), and they do cause their own replication.

Understanding evolution in a precise, modern way was Deutsch's largest contribution to CR.

I don't expect RS to understand this material from these brief quotes. It's complicated. I'm trying to give an indication that there's substance here that could be learned. If he wants to understand it, he'll have to read Deutsch's books (there's even more material about memes in *The Beginning of Infinity*) or ask a lot of questions. I do hope he'll stop saying this is false while he doesn't understand it.

> 19. You claim that CR 'works'. According to what criteria - logical? empirical? pragmatic? If it is pragmatism - or what Stove calls the American philosophy of self-indulgence' - then all philosophies, religions and superstitions 'work' (for their believers).

CR works logically, empirically, and practically. That is, there's no logical, empirical or practical refutation of its effectiveness. (I'm staying away from the word "pragmatic" on purpose. No thanks!)

What CR works to do, primarily, is create knowledge. The way I judge that CR works is by looking at the problems it claims to solve, how it claims to solves them, and critically considering whether its methods would work (meaning succeed at solving those problems).

CR offers a conception of what knowledge is and what methods create it (guesses and criticism – evolution). CR offers substantial detail on the matter. I know of no non-refuted criticism of the ability of CR's methods to create knowledge as CR defines knowledge.

There's a further issue of whether CR has the right goals. We can all agree we want "knowledge" in some sense, but is CR's conception of knowledge actually the thing we want? Not for everyone, e.g. infallibilists. But CR explains why conjectural knowledge is the right conception of knowledge to pursue, which I don't know any non-refuted criticism of. Further, there are no viable rival conceptions of knowledge that anyone knows how to pursue. Basically, all other conceptions of knowledge are either vague or wrong (e.g. infallibilist). This claim depends on a bunch of arguments – RS if you state your conception of knowledge then I'll comment on it.

> 20. You are right to say that '90% certain' is an oxymoron. But so is 'conjectural knowledge'.

Here RS interprets "knowledge" and perhaps also "conjectural" in his own terminology, rather than learning what CR means.

The most important part of CR's conception of knowledge is that fallible ideas can be knowledge. Conjectures are fallible.

"Conjectural knowledge" is also an anti-authoritarian concept. Popper is saying that mere guesses (even myths) can be knowledge (if they solve a problem and are subjected to critical scrutiny). An idea doesn't have to be created by an authority-granting method (e.g. deduction, induction, abduction, "the scientific method", etc) or come from an authority-granting source (e.g. a famous scientist) in order to be knowledge.

> 21. 'Actually, the possibility for further progress is a good thing' is a value judgement. But how can progress be a feature of CR? Was not Thomas Kuhn right to claim that Popper's position leads to rampant relativism (as Kuhn's does).

No, Popper isn't a relativist about anything. Popper wrote a ton about progress and took the position that progress is possible, objective and desirable. (E.g. "Equating rationality with the critical attitude, we look for theories which, however fallible, progress beyond their predecessors" from *C&R*.) And Popper thought we have objective knowledge, including about value judgements and morality. Some of Popper's comments on the matter in *The World of Parmenides*:

> Every rational discussion, that is, every discussion devoted to the search for truth, is based on principles, which in actual fact are *ethical principles*.
>
> ...
>
> All this shows that ethical principles form the basis of science. The most important of all such ethical principles is the principle that objective truth is the fundamental regulative idea of all rational discussion. Further ethical principles embody our commitment to the search for truth and the idea of approximation to truth; and the importance of intellectual integrity and of fallibility, which lead us to a self-critical attitude and to toleration. It is also very important that we can *learn* in the field of ethics.
>
> ...
>
> Should this new ethics [that Popper proposes] turn out to be a better guide for human conduct than the traditional ethics of the intellectual professions ... then I may be allowed to claim that new things can be learnt even in the field of ethics.
>
> ...
>
> in the field of ethics too, one can put forward suggestions which may be discussed and improved by critical discussion

In CR's view, the ability to learn in a field requires that there's objective knowledge in that field. Under relativism, you can't learn since there's no mistakes to correct and no objective truth to seek. So Popper thinks there is objective ethical knowledge.

> 22. Your claim that 'induction works by inducing' applies also to 'deduction works by deducing'.

The statement "deduction works by deducing" would be a bad argument for deduction or explanation of how deduction works.

Inductivists routinely state that induction works by generalizing or extrapolating from observation and think they've explained how to do induction (rather than recognizing the relation of their statement to "induction works by inducing").

23. Inductivists do have an answer for you. Stove has argued, correctly in my view, that there are good reasons to believe inductively-derived propositions. I paraphrase from my book 'An Eye for an I' (pp.183-4) for your readers who have no knowledge of my book.

'Hume's scepticism about induction - that it is illogical and hence irrational and unreasonable - is the basis for his scepticism about science. His two main propositions are: inference from experience is not deductive; it is therefore a purely irrational process. The first proposition is irrefutable. 'Some observed ravens are black, therefore all ravens are black' is an invalid argument: this is the 'fallibility of induction.' But the second proposition is untenable since it assumes that all rational inference is deductive. Since 'rational' means 'agreeable to reason', it is obvious that our use of reason often ignores deduction and emphasises the facts of experience and inferences therefrom.

Stove defends induction from Hume's scepticism by arguing that scepticism about induction is the result of the 'fallibility of induction' and the assumption that deduction is the only form of rational argument. The result is inductive scepticism, which is that no proposition about the observed is a reason to believe a contingent proposition about the unobserved. The fallibility of induction, on its own, does not produce inductive scepticism because from the fact that inductive arguments are invalid it does not follow that something we observe gives us no reason to believe something we have not yet observed. If all our experience of flames is that they burn, this does give us a reason for assuming that we will get burned if we put our hand into some as yet unobserved flame. This is not a logically deducible reason but it is still a good reason. But once the fallibility of induction is joined with the deductivist assumption that the only acceptable reasons are deductive ones, inductive scepticism does indeed follow.

> Hume's scepticism about science is the result of his general inductive scepticism combined with his commitment to empiricism, which holds that any reason to believe a contingent proposition about the unobserved is a proposition about the observed. So the general proposition about empiricism needs to be joined with inductive scepticism to produce Hume's conclusion because some people believe that we can know the unobserved by non-empirical means, such as faith or revelation. As an empiricist Hume rules these means out as proper grounds for belief. So to assert the deductivist viewpoint is to assert a necessary truth, that is, something that is trivially true not because of any way the world is organised but because of nothing more than the meanings of the terms used in it. When sceptics claim that a flame found tomorrow might not be hot like those of the past, they have no genuine reason for this doubt, only a trivial necessary truth.'
>
> What, then, is the bearing of 'all observed ravens have been black' on the theory 'all ravens are black'? Stove's answer is based on an idea of American philosopher Donald Cary Williams, which is to reduce inductive inference to the inference from proportions in a population. It is a mathematical fact that the great majority of large samples of a population are close to the population in composition. In the case of the ravens, the observations are probably a fair sample of the unobserved ravens. This applies equally in the case where the sample is of past observations and the population includes future ones. Thus, probable inferences are always relative to the available evidence.

The claim "there are good reasons to believe inductively-derived propositions" doesn't address Popper's arguments that inductively-derived propositions *don't exist*.

Any finite set of facts or observations is compatible with infinitely many different ideas. So which idea(s) does one induce?

Note that this argument is not about the "fallibility of induction". So Stove is mistaken when he says that's the source of skepticism of induction. (No doubt it's a source of *some* skepticism of induction, but not of CR's.) The claim that deduction is the only form of rational argument is also not CR's position. So Stove isn't answering CR. Yet RS said this was an inductivist answer to me.

This is typical. I had an objection to the first sentence following "Inductivists do have an answer for you." It made an assumption I consider false. It then proceeded to build on that assumption rather than answer me.

Where RS writes, "it is still a good reason", no statement of *why* it's a good reason or *in what sense it's "good"* or *why being good in that sense matters* is given. Avoiding some technical details, CR says *approximately* that it's a good reason because we don't have a criticism of it, rather than for an inductive reason. Why does no criticism matter? What's good about that? Better an idea you don't see anything wrong with than one you do see something wrong with.

Nothing in the paragraphs answers CR. They just demonstrate unfamiliarity with CR's standard arguments. Consider:

> When sceptics claim that a flame found tomorrow might not be hot like those of the past, they have no genuine reason for this doubt, only a trivial necessary truth.

Many things in the future are different than the past. So one has to understand explanations of in what ways the future will resemble the past, and in what ways it won't. Induction offers no help with this project. Induction doesn't tell us in which ways the future will resemble the past and in which ways it won't (or in which ways the unobserved resembles the observed and in which ways it doesn't). But explanations (which can be improved with critical discussion) do tell us this.

For example, modern science has an explanation of what the sun is made of (mostly hydrogen and helium), its mass (4.385e30 lbs), why it burns (nuclear fusion), etc. These explanations let us understand in what respects the sun will be similar and different tomorrow, when it will burn out, what physical processes would change the date it burns out, what will happen when it burns out, and so on. Explanations simply aren't inferences from observations using some kind of inductive principle about the future probably resembling the past while ignoring the "in which respects?" question. And the sort of skeptic being argued with in the quote has nothing to do with CR.

I won't get into probability math here (we could do that in the future if desired), but I will mention that Popper already addressed that stuff. And the object of this exercise was to *answer* CR, but that would take something like going over

Popper's arguments about probability (with quotes) and saying why they are mistaken or how to get around them.

> 24. You state that Popper invented critical rationalism around 1950. I would have thought it was around the mid-1930s.

Inventing CR was an ongoing process so this is approximate. But here are some of the book publication dates:

Objective Knowledge, 1972. *Conjectures and Refutations*, 1963. Realism and the Aim of Science, 1983 (circulated privately in 1956). *The Logic of Scientific Discovery*, 1934 (1959 in English). Since I don't consider LScD to be anything like the whole of CR, I chose a later date.

> [25.] Your last paragraph is especially unfortunate because you accuse those philosophers who are not critical rationalists (which is most of them) of not understanding 'it enough to argue with it.' With respect Elliot, this is arrogant and ill-informed. Many philosophers understand it only too well and have written learned books on it. Some are broadly sympathetic but critical (David Miller, Anthony O'Hear) while others (Stove, James Franklin) are critical and dismissive. To acknowledge that CR 'isn't very popular, but it can win any debate' is nonsensical and carries the whiff of the 'true believer', which would seem to be self-contradictory for a critical rationalist.

It may be arrogant, but I don't think it's ill-informed. I've researched the matter and don't believe the names you list are counter-examples.

What's nonsensical about an idea which can win in debate, but which most people don't believe? Many scientific ideas have had that status at some time in their history. Ideas commonly start off misunderstood and unpopular, even if there's an advocate who provides arguments which most people later acknowledge were correct.

I think I'm right about CR. I'm fallible, but I know of no flaws or outstanding criticisms of any of my take on CR, so I (tentatively) accept it. I have debated the matter with all critics willing to discuss for a long time. I have sought out criticism from people, books, papers, etc. I've made an energetic effort to find out my mistakes. I haven't found that CR is mistaken. Instead, I've found the critics consistently misunderstand CR, do not provide relevant arguments which

address my views, do not address key questions CR raises, and also have nothing to say about Deutsch's books.

I run a public philosophy discussion forum. I have visited every online philosophy discussion forum I could find which might offer relevant discussion and criticism. The results were pathetic. I also routinely contact people who have written relevant material or who just seem smart and potentially willing to discuss. For example, I contacted David Miller and invited him to discuss, but he declined.

Calling this arrogant (Because I think I know something important? Because I think many other people are mistaken?), doesn't refute my interpretation of these life experiences. RS, if you have a proposal for what I should do differently (or a different perspective I should use), I'll be happy to consider it. And if you know of any serious critics of CR who will discuss the matter, please tell me who they are.

None of RS's 25 points were difficult for me to answer. If RS knew of any refutation of CR by any author which I couldn't answer, I would have expected him to be able to pose a difficult challenge for me within 25 comments. But, as usual with everyone, so far nothing RS has said gives even a hint of raising an anti-CR argument which I don't have a pre-existing answer for.

Reply to Robert Spillane

I'm not trying to make ad hominem remarks. I put effort into avoiding them. It is nevertheless possible that an argument targets idea X, but CR was saying Y, not X. It's also possible that CR makes a statement in its own terminology which is misread by substituting some word meanings with those favored by a rival philosophy. I don't see anything against-the-person about bringing up these issues.

I reject Popper's three worlds. I think there's one world, the physical world. I think minds and ideas have physical existence in that one world, just like running computer software and computer data physically exist. More broadly, the laws of physics say that information exists and specify rules for it (the rules of computation); ideas are a type of information.

I've never selected philosophy ideas by nationality, and never found pragmatism appealing. Nor am I getting material from Quine. And I don't accept the blame for Feyerabend, who made his own bad choices. Here's a list of philosophers I consider especially important: Karl Popper, David Deutsch, Ayn Rand, Ludwig von Mises, William Godwin, Edmund Burke, Thomas Szasz, and some ancient Greeks.

All propositions are synthetic because the laws of logic and math depend on the laws of computation (including information processing) which depend on the laws of physics. Our understanding of physics involves observation, and the particular laws of physics we have are contingent. Epistemology and evolution depend on physics too, via logic and computation, and also because thinking and evolving are physical processes.

Of course I agree with you that the goal is to find truth, not power or bullying or popularity.

Stove on Paley didn't answer my questions, but gave me some indication of some of your concerns, so:

I do not accept any kind of genetic or biological determinism, nor Darwinian "survival of the fittest" morality. Men have free will and are not controlled by a

mixture of "influences" like genes, memes, culture, etc. By "influences" I include claims like "that personality trait is under 60% genetic control" – in that way genes are claimed to partially influence, but not fully control, some human behavior.

I have read some of the studies in this field and their quality is terrible. I could tell you how to refute some of their twin studies, heritability claims, etc, but I'm guessing you already know it.

I think "influences" may play a significant role in two ways:

1) A man may like and agree with an "influence", and pursue it intentionally. E.g. his culture praises soldiers, and he finds the profession appealing and chooses to become a soldier. Here the "influence" is actually just an option or piece of information which the man judges.

or

2) "Influences" matter more when a man is irresponsible and passive. If you don't take responsibility for your life, someone or something else may partially fill the void. If you don't actively control your life, then there's room for external control. A man who chooses to play the role of a puppet, and lets "influences" control him, may partially succeed.

Regarding Miller: by your terminology, I'm also a critic of Popper.

> When two philosophers cannot agree on basic definitions,

could you give definitions of knowledge and induction? for clarity, i'll be happy to call my different concepts by other words such as CR-knowledge.

> You state that 'I disagree with and deny the whole approach of a priori knowledge and the analytic/synthetic dichotomy.' But Popper, as a rationalist, relies on a priori knowledge, i.e. primitive theories which are progressively modified by trial and error elimination.

Inborn theories aren't a priori, they were created by genetic evolution. (They provide a starting point but DO NOT determine people's fate.)

> when I try to argue with you, and you disagree with my mode of arguing, which is widely accepted in philosophical circles, it is difficult to know how to respond to your questions.

i think this is important. I have views which disagree with what is, i agree with you, "widely accepted in philosophical circles". it is difficult to understand different frameworks than the standard one, but necessary if you want to e.g. evaluate CR.

> For example, with respect to Szasz you write that he 'doesn't write deductions, formal logic and syllogisms'. True, he doesn't use symbolic logic but his life's work was based on the following logic (see Szasz Under Fire, pp.321-2 where he relies on the analytic-synthetic distinction):
>
> "When I [Szasz] assert that (mis)behaviors are not diseases I assert an analytic truth, similar to asserting that bachelors are not married...InThe Myth of Mental Illness, I argued that mental illness does not exist not because no one has yet found such a disease, but because no one can find such a disease: the only kind of disease medical researchers can find is literal, bodily disease."

I acknowledge that I disagree with Szasz about analytic/synthetic. Unfortunately he died before we got to resolve the matter.

However, I think Szasz's main point is that no observations of "patients" could refute him. I agree. Facts about "patients" can't challenge logical arguments.

However, as I explained above, I don't think logic itself is analytic. I think observations which led to a new understanding of physics could theoretically (I don't expect it) play a role in challenging Szasz's logical arguments.

> Here is Szasz's logic:
>
> - Illness affects the human body (by definition);
> - The 'mind' is not a bodily organ;
> - Therefore, the mind cannot be or become ill;
> - Therefore mental illness is a myth.
> - If 'mind' is really the brain or a brain process;
> - Then mental illnesses are brain illnesses.
> - Since brain illnesses are diagnosed by objective medical signs,

> - And mental illnesses are diagnosed by subjective moral criteria;
> - Mental illnesses are not literal illnesses
> - And mental illness is still a myth.
>
> If this is not deductive reasoning, then what is?

That isn't even close to a deductive argument. For example, look how "myth" is used in a conclusion statement (begins with "therefore"), without being introduced previously. You couldn't translate this into symbolic logic and make it work. Deduction has very strict rules, which you haven't followed.

As to what is deductive reasoning: no one does complex, interesting philosophy arguments using only deduction. Deduction is fine but limited.

I do appreciate the argument you present. I think it's well done, valuable, and rational. It's just not pure deduction (nor a combination of deduction and induction).

I would normally just call it an "argument". CR doesn't have some special name for what type of reasoning it is. We could call it a CR-argument or CR-reasoning if you like. You ask what's left for reasoning besides induction and deduction, and I'd point to your example and say that's just the kind of thing I think is a typical argument. (Your argument is written to appear to resemble deduction more than is typical, so the style is a bit uncommon, but the actual way it works is typical.)

> 'The basic point here is to judge an idea by what it says..." Quite so. But how do you do that?

By arguments like the "mental illness" example you provided, and the socialism and price controls example I provided previously. By using arguments to criticize mistakes in ideas. etc.

> You write: 'The claim [Stove's and mine] that "there are good reasons to believe inductively-derived propositions" doesn't address Popper's arguments that inductively-derived propositions don't exist.' This follows more than half a page of reasons why they do exist. And, contrary to your claim, I gave you an example of a good (i.e reasonable, practical, useful) reason to believe an inductively-derived proposition. What more can I say?

> You write: 'This is typical. I had an objection to the first sentence following "Inductivists do have answer for you." It made an assumption I consider false. It then proceeds to build on that assumption rather than answer me.' That obnoxious sentence is 'Stove has argued, correctly in my view, that there are good reasons to believe inductively-derived propositions.' What is the assumption you consider false? I then proceed to provide Stove's arguments. Is not this what critical rationalists encourage us to do with their platitudes about fallibility, willingness to argue a point of view? Those arguments, whether valid or invalid, do provide reasons why one might reject Popper's authoritarian pronouncement that inductively-derived propositions don't exist. Of course, they exist, even if Popper does not grant them legitimacy.

We're talking about too many things at once. If you think this is particularly important, I could answer it. I do attempt to continue the discussion of induction below.

> You write: 'But as usual with everyone, so far nothing RS has said gives even a hint of raising an anti-CR argument which I don't have a pre-existing answer for.' Well, then future argument is pointless because your 'fallibilism' is specious. If you have already decided in favour of CR, I doubt there are any critical arguments which you will consider. You appear to have developed your personal version of CR and immunised yourself against criticism, a vice which Popper in theory, if not in practice, warned against.

I'm open to changing my mind.

I have discussed these issues in the past and made judgements about some ideas. To change my mind, you'll have to say something new to me. I expect the same the other way around: if I don't have anything to say that you haven't heard before, then I won't change your mind.

I have a lot of previous familiarity with these issues. So far you haven't come near the edges of my pre-existing knowledge. You haven't said something about epistemology which is surprising or new for me (nor has Stove in what I read). Minor details differ, but not main points.

That's OK, I would expect it to take more discussion than we've done so far to get to get beyond people's already-known arguments.

It's right and proper that we each have initial (level 1) responses ready which cover many issues a critic could raise. And when he responds to one of them, we again have level 2 responses ready for many things he may say next. Very educated, experienced persons may have a dozen levels of responses that they already know. So to change their minds, one has to either say something surprising early on (that they didn't hear or think of previously, so they don't have a pre-existing answer) or else go through the levels and then argue with some of their ideas near the limits of their knowledge.

So far your comments regarding induction have been typical of other inductivists I've spoken with.

> A reviewer of Popper's work was published in 1982 in The New York Review (Nov.18 (pp.67-68) and Dec.2 (pp.51-56). I could not express my reservations better than this:
>
> 'Popper's philosophy of science is profoundly ambiguous: it is, he says, "empirical", but it is left unclear why scientists should consult experience.

The reason for consulting experience is to criticize ideas which contradict experience (because we want ideas which match reality). That is not "left unclear", it's stated clearly by Popper.

> It is called "fallibilism", in which we learn from our mistakes", but it is really an ill-concealed form of skepticism.

The skepticism accusation is an assertion, not an argument.

> It claims to surrender the quest for certainty, but it is precisely the standards of this quest - that if one is not certain of a proposition, one can never be rationally justified in claiming it to be true - that underlie Popper's rejection of induction (and the numerous doctrines that stem from this rejection).

Popper did NOT reject induction for being fallible or imperfect, he rejected it for reasons like:

1) Any finite set of data is compatible with infinitely many generalizations, so by what method does induction select *which generalizations to induce* from those infinite possibilities?

2) How much support does X give Y, in general? And what difference does that make?

Induction fails to meet these challenges, and without answers to those issues induction can't be used at all. These aren't "it's imperfect" type issues, they are things that must be addressed to use induction at all for anything.

There have been some attempts to meet these challenges, but I don't think any succeeded, and Popper pointed out flaws in some of them. If you can answer the questions, or give page numbers where Stove does, I will comment.

If you wish to address (2), note that "in general" includes non-mathematical issues, e.g. the beauty of a piece of music or flower. (And if you think induction can't address those beauty issues, then I'm curious what you propose instead. Deduction? Some third thing which will, on examination, turn out to have a lot in common with CR?)

More Robert Spillane Discussion

This reply to Robert Spillane follows up on **this previous discussion**. Here's a **full list of posts** related to Spillane.

> Thank you for your respectful reply. I think we are making progress.
>
> It has been helpful to have you clarify which parts of Popper you accept.

Great.

> I am reminded of an interesting chapter in Ernest Gellner's bookRelativism and the Social Sciences, (1985, Ch. 1: 'Positivism and Hegelianism), where he discusses early versus late Popper, supports the former against the latter, and concludes that Popper is (a sort of) positivist. It is an interesting chapter and one I would happily discuss with you.
>
> Like Gellner, I am sympathetic to Popper's 'positivism' but cannot accept his rejection of inductive reasoning. Like you (and Szasz), I reject his 3 Worlds model.

Popper was an opponent of the standard meaning of positivism. I mean something like this dictionary definition: "a philosophical system that holds that every rationally justifiable assertion can be scientifically verified or is capable of logical or mathematical proof, and that therefore rejects metaphysics and theism."

So what sort of "positivism" are you attributing to Popper?

I've ordered the book.

> Re your favourite philosophers: you might read Szasz's critical comments on Rand, Branden, Mises, Hayek, Rothbard and Nozick in Faith in Freedom: Libertarian Principles and Psychiatric Practices, (Transaction Publishers, 2004). Even though I received the Thomas Szasz Award in 2006, I told Tom that I could not commit myself to (economic) libertarianism in the way that he did and you appear to do. I accept the

> primacy of personal freedom but do not accept the economic freedom favoured by libertarians. Indeed, I would have thought that by now, in the age of huge corporations, neo-liberalism is on its last legs. I respect your position, however.

Yes, I'm fully in favor of capitalism.

Yeah, I discussed *Faith in Freedom* with Szasz, but I don't have permission to share the discussion. One thing Szasz did in the book was use some criticism of Rand from Rothbard. I could tell you criticism of Rothbard's arguments if you wanted, though I think he's best ignored. I do not consider Rothbard or Justin Raimondo to be decent human beings, let alone reliable narrators regarding Rand. I was also unimpressed by Szasz's criticisms of Rand's personal life in the book, and would prefer to focus on her ideas. And I think Szasz made a mistake by quoting Whittaker Chambers' ridiculous slanders.

FYI I only like Rand and Mises from the list of people you mention, and I agree with Szasz that they were mistaken regarding psychiatry. (Rand didn't say much on psychiatry, and some of it good, as Szasz discusses. But e.g. she got civil commitment partly wrong.)

You may be interested to know that Rand spoke very critically of libertarians, especially Hayek and Friedman (who both sympathized with socialism, as did Popper). She thought libertarians were harming the causes of liberty and capitalism with their unprincipled, bad philosophy. I agree with her.

Rand did appreciate Mises because he was substantially different than the others: he was an anti-anarchy classical liberal, a consistent opponent of socialism, and he was very good at economics.

We have criticisms of many libertarian ideas *from the right*.

Let me mention that I'm not an orthodox Objectivist. I do not like the current Objectivist leadership like Peikoff, Binswanger, and the Ayn Rand Institute. I am banned from the main Objectivist forum for dissenting regarding epistemology (especially induction, fallibilism and perception). I also dissented regarding psychiatry, but discussion of psychiatry was banned before much was said.

If you're interested, I wrote about what the disagreements were and the decision to ban me. I pointed out various ways my views and actions are in line with Ayn Rand's philosophy and theirs aren't. It clarifies some of my philosophy positions:

http://curi.us/1930-harry-binswanger-refuses-to-think

There was no reply, no counter-argument. I am aware that they will hold a grudge for life because I wrote that.

I also made a public record of what I said in my discussions with them:

http://curi.us/1921-the-harry-binswanger-letter-posts

Warning: my comments are book length.

> I have spent my career in the space between neo-positivism (Hume, Stove) and a critical existentialism (Sartre, Szasz). You might see inconsistencies here but I have always agreed with Kolakowski who wrote in his excellent book Positivist Philosophy, (pp. 242-3):
>
> 'The majority of positivists tend to follow Wittgenstein's more radical rule: they do not simply reject the claims of metaphysics to knowledge, they refuse it any recognition whatever. The second, more moderate version is also represented, however, and according to it a metaphysics that makes no scientific claims is legitimate. Philosophers who, like Jaspers, do not look upon philosophy as a type of knowledge but only as an attempt to elucidate Existenz, or even as an appeal to others to make such an attempt, do not violate the positivist code. This attitude is nearly universal in present-day existential phenomenology. Awareness of fundamental differences between 'investigation' and 'meditation', between scientific 'accuracy' and philosophic 'precision', between 'problems' and 'questioning' or 'mystery' is expressed by all existential philosophers...'

I broadly disagree with attempts to separate some thinking or knowledge from reality.

> As an aside: I asked Tom Szasz that since he has been appropriated by some existentialists, whether he accepted that label. He thought about it for an hour and said: 'Yes, I'm happy to be included among the existentialists. However, if Victor Frankl is an existentialist, I'm not!' Frankl, despite his

> reputation as a humanist/existentialist boasted of having authorised many and conducted a few lobotomies on people without their consent.
>
> Your criticism of the analytic/synthetic dichotomy reminds me of Quine but expressed differently. I disagree with you (and Quine) and agree with Hume, Stove and Szasz (and many others) on this issue. I am confident that had Szasz lived for another 50 years, you would not have convinced him that all propositions are synthetic and therefore are either true or false. He and I believe that the only necessities (i.e necessary truths) in the world are those expressed as analytic propositions and these tell us nothing about the world of (empirical) facts.

I don't believe necessary truths like that exist. I think people mistake features of reality (the actual reality they live in) for necessary truths. In our world, logic works a particular way, but it didn't necessarily have to. People fail to imagine how some things could be otherwise because they are used to the laws of physics we live with.

If you have a specific criticism of my view, I'll be happy to consider it.

I think I would have persuaded Szasz in much less than 50 years, *if I'm right*. Or else Szasz would have persuaded me. I don't think it would have stayed unresolved.

I found Szasz extraordinarily rational and open to criticism, more so than anyone else I've ever discussed with.

> I'm delighted that you do not buy into Dawkins' nonsense about 'memes' even if you use 'ideas' as if they are things. Stove on Dawkins hits the mark.

There may be a misunderstanding here. I do buy into David Deutsch's views about memes! I accept memes exist and matter. But I think memes are popularly misunderstood and don't lead to the conclusions others have said they do.

I know that Szasz disagreed with me about memes. He did not, however, provide detailed arguments regarding evolution.

> 'Knowledge' and 'idea' are abstract nouns and therefore, as a nominalist, I'm bound to say they don't exist, except as names.

I consider them the names of either physical objects (like chairs) or attributes of physical objects (like the color red). As a computer hard drive can contain a file, a brain can contain an idea.

> I encourage my students to rely less on nouns and more on verbs (from which most nouns originated). You asked for two definitions:
>
> To 'know' means 'to perceive or understand as fact or truth' (Macquarie Dictionary, p.978). Therefore 'conjectural knowledge' is oxymoronic.

This is ambiguous about whether the understanding may be fallible or not.

Do you need a guarantee of truth to have knowledge, or just an educated guess which is correct according to your current best-efforts at understanding?

Why can't one conjecturally (fallibly) understand something to be a fact?

> Induction: 'the process of discovering explanations for a set of particular facts, by estimating the weight of observational evidence in favour of a proposition which asserts something about the entire class of facts (MD, p.904).
>
> Induction: 'a method of reasoning by which a general law or principle is inferred from observed particular instances...The term is employed to cover all arguments in which the truth of the premise, or premises, while not entailing the truth of the conclusion, or conclusions, nevertheless purports to constitute good reasons for accepting it, or them... With the growth of natural science philosophers became increasingly aware that a deductive argument can only bring out what is already implicit in its premises, and hence inclined to insist that all new knowledge must come from some form of induction. (A Dictionary of Philosophy, Pan Books, 1979, pp.171-2).

I agree that those are typical statements of induction. How do you address questions like:

Which general laws, propositions, or explanations should one consider? How are they chosen or found? (And whatever method you answer, how does it differ from CR's brainstorming and conjecturing?)

When and why is one idea estimated to have a higher weight of observational evidence in favor of it than another idea? Given the situation that neither idea is contradicted by any of the evidence.

I think these issues are very important to our disagreement, and to CR's criticism of induction.

> You say that 'inborn theories are not a priori'. But a priori means prior to sense experience and so anything 'inborn'must be a priori be definition.

A priori means "relating to or denoting reasoning or knowledge that proceeds from theoretical deduction rather than from observation or experience" (New Oxford American Dictionary)

Inborn theories, which come from genes, don't come from theoretical deduction, nor from observation. Their source is evolution. This definition offers a false dichotomy.

Another definition (OED):

"A phrase used to characterize reasoning or arguing from causes to effects, from abstract notions to their conditions or consequences, from propositions or assumed axioms (and not from experience); deductive; deductively."

that doesn't describe inborn theories from genes.

inborn theories are like the software which comes pre-installed on your computer, which you can replace with other software if you prefer.

inborn theories don't control your life, it's just that thinking needs a starting point. similar to how your life has a starting time and place, which does matter, but doesn't control your fate.

these inborn theories are nothing like analytical ideas or necessary truths. they're just regular ideas, e.g. we might have inborn ideas about the danger of snakes (the details of which ideas are inborn is largely unknown) which were created because of actual encounters with snakes before we were born. but that's still not created by observation or experience, because genes and evolution can neither observe nor experience.

Spillane wrote previously:

> Here is Szasz's logic:
>
> - Illness affects the human body (by definition);
> - The 'mind' is not a bodily organ;
> - Therefore, the mind cannot be or become ill;
> - Therefore mental illness is a myth.
> - If 'mind' is really the brain or a brain process;
> - Then mental illnesses are brain illnesses.
> - Since brain illnesses are diagnosed by objective medical signs,
> - And mental illnesses are diagnosed by subjective moral criteria;
> - Mental illnesses are not literal illnesses
> - And mental illness is still a myth.
>
> If this is not deductive reasoning, then what is?

I denied that this is deduction, and I pointed out that "myth" is introduced for the first time in a conclusion statement, so it doesn't follow the rules of deduction. Spillane now says:

> If the example of Szasz's logic is not deductive - the truth of the conclusion is implicit in the premise - what sort of argument is it? If you remove #4, would you accept it as a deductive argument?

I think it deviates from deduction in dozens of ways, so removing #4 won't help. For example, the terms "objective", "subjective" and "literal" are introduced towards the end without using previous premises and syllogisms to establish anything about them. I also consider it incomplete in dozens of ways (as all complex arguments always are). You could try to write it as formal (deductive) logic, but I think you'd either omit most of the content or fail.

I don't think the truth of the conclusion is implicit in the premises. I think many philosophers have massively overestimated what they could translate to equivalent formal deductions. So I regard it simply as an "argument", just like most other arguments which don't fall into the categories non-Popperian philosophers are so concerned with.

And even if some arguments could be rewritten as strict deductions, people usually don't do that, and they can still learn and make progress anyway.

Rather than worrying about what category an argument falls into, CR is concerned with whether you have a criticism of it – that is, an argument for why it's false.

I don't think pointing out "that isn't deduction" is a criticism, because being non-deductive is compatible with being true. (The same comment applies to induction.)

I also don't think that pointing out an idea is incomplete is a criticism without further elaboration. What matters is if the idea can succeed at it's purpose, e.g. solve a problem, answer a question, explain an issue. An idea may do that despite being incomplete in some way because the incompleteness may be

irrelevant.

> My epistemological position should be clear from what I have said above - it is consistent with a moderate form of neo-positivism.
>
> That Popper's fallibilism is ill-concealed skepticism has been argued at length, by many Popper scholars, e.g. Anthony O'Hear. It was even argued in the book review mentioned.

I don't care how many people argued something at what length. I only care if there are specific arguments which are correct.

Are you denying that you are fallible (capable of making mistakes)? Do you think you sometimes have 100% guarantees against error?

Or do you just deny the second part of Popper's fallibilism? His claim that, in the world today, mistakes are common even when people feel certain they're right.

If it's neither of those, then I don't know what your issue with fallibilism is.

> I have already given you (in a long quote) examples of inductively-derived propositions that are 'reasonable'. Now they may not be reasonable to a deductivist, but that only shows that deductivists have a rigid definition of 'rational', 'reasonable' and 'logical'. Given that a very large number of observations of ravens has found that they are black without exception, I have no good reason to believe the next one will be yellow, even though it is possible. That the next raven may be yellow is a trivial truth since it is a

> tautology. Accordingly, I have a good reason to believe that the raven in the next room is black.

OK I'll address this topic after you answer my two questions about induction above.

Discussing Necessary Truths and Induction with Spillane

You often ask me for information/arguments that I have already given you

We're partially misunderstanding each other because communication is hard and we have different ways of thinking. I'm trying to be patient, and I hope you will too.

Please address these two questions about induction. Answering with page numbers from a book would be fine if they directly address it.

I've read lots of inductivist explanations and found they consistently don't address these questions in a clear, specific way, with actual instructions one could follow to do induction if one didn't already know how. I've found that sometimes accounts of induction give vague answers, but not actionable details, and sometimes they give specifics unconnected to philosophy. Neither of those are adequate.

1) *Which* general laws, propositions, or explanations should one consider? How are they chosen or found? (And whatever method you answer, how does it differ from CR's brainstorming and conjecturing?)

2) *When* and *why* is one idea estimated to have a higher weight of observational evidence in favor of it than another idea? Given the situation that neither idea is contradicted by any of the evidence.

These are crucial questions to what your theory of induction says. The claimed specifics of induction vary substantially even among people who would agree with the same dictionary definition of "induction".

I've read everything you wrote to me, and a lot more in references, and I don't yet know what your answers are. I don't mind that. Discussion is hard. I think they are key questions for making progress on the issue, so I'm trying again.

> As a fallibilist, you acknowledge that the 'real world' is a contingent one and there are no necessary truths. But is not 1+1=2 a necessary truth? Is not 'All tall men are men' a necessary truth since its negation is self-contradictory?

I'll focus on the math question because it's the easier case to discuss first. If we agree on it, then I'll address the A is A issue.

I take it you also think the solution to 237489 * 879234 + 8920343 is a necessary truth, as well as much more complex math. If instead you think that's actually a different case than 1+1, please let me know.

OK, so, how do you know 1+1=2? You have to figure out what 1+1 sums to. You have to calculate it. You have to perform addition.

The only means you have to calculate sums involve physical objects which obey the laws of physics.

You can count on your fingers, with an abacus, or with marbles. You can use a Mac or iPhone calculator. Or you can use your brain to do the calculation.

Your knowledge of arithmetic sums depends on the properties of the objects involved in doing the addition. You believe those objects, when used in certain ways, perform addition correctly. I agree. If the objects had different properties, then they'd have to be used in different ways to perform addition, or might be incapable of it. (For example, imagine an iPhone had the same physical properties as an iPhone-shaped rock. Then the sequences of touches the currently sum 1 and 1 on an iPhone would no longer work.)

Your brain, your fingers, computers, marbles, etc, are all physical objects. The properties of those objects are specified by the laws of physics. The objects have to be used in certain ways, and not other ways, to add 1+1 successfully. What ways work depends on the laws of physics which say that, e.g., marbles don't duplicate themselves or disappear when arranged in piles.

So I don’t think 1+1=2 is a truth independent of the laws of physics. If there's a major, surprising breakthrough in physics and it turns out we're mistaken about the properties of the physical objects used to perform addition, then 1+1=2 might have to be reconsidered because all our ways of knowing it depended on the old

physics, and we have to reconsider it using the new physics. So observations which are relevant to physics are also relevant to determining that 1+1=2.

This is explained in "The Nature of Mathematics", which is chapter 10 of *The Fabric of Reality* by David Deutsch. If you know of any refutation of Deutsch's explanation, by yourself or others, please let me know. Or if you know of a view on this topic which contradicts Deutsch's, but which his critical arguments don't apply to, then please let me know.

> I believe that Einstein is closer to the truth of what you call the real world than was Aristotle. So when I'm told by this type of fallibilist that we don't know anymore today than we did 400 years ago, I demur.

Neither Popper nor I believe that "we don't know anymore today than we did 400 years ago".

> Given your comments on LSD and the a-s dichotomy, after reading this I conclude that you are a fan of late Popper (LP) and I prefer early Popper (EP).

Yes.

You think EP is wrong, and I think LP is right, so I don't see the point of talking about EP.

(I disagree with your interpretation of EP, but that's just a historical issue with no bearing on which philosophy of knowledge ideas are correct. So I'm willing to concede the point for the purpose of discussion.)

> Gellner argued that Popper is a positivist in the logical positivist rather than the Comtean positivist sense. His discussion proceeded from the contrasting of positivists and Hegelians and so he put (early) Popper in the positivist camp - Popper was certainly no Hegelian. Of course, Popper never tired of reminding us that he destroyed the positivism of the Vienna Circle and went to great pains to declare himself opposed to neo-positivism. For example, he says that he warmly embraces various metaphysical views which hard positivists would dismiss as meaningless. Moderate positivists, however, accept metaphysical views but deny them scientific status. Does not Popper do this too, even if some of these views may one day achieve scientific status?

Yes: (Late) Popper accepts metaphysical and philosophical views, but doesn't consider them part of science.

CR (late-CR) says non-science has to be addressed with non-observational criticisms, instead of what we do in science, which is a mix of observational and non-observational criticism.

> If by fallibilism you mean searching for evidence to support or falsify a theory, I'm a fallibilist. If, however, you mean embracing Popper's view of 'conjectural knowledge' and the inability, even in principle, or arriving at the truth, then I'm not. I believe, against Popper, Kuhn and Feyerabend, that the history of science is cumulative.

No, fallibilism means that (A) there are no guarantees against error. People are capable of making mistakes and there's no way around that. There's no way to know for 100% sure that a proposition is true.

CR adds that (B) errors are common.

Many philosophers accept (A) as technically true on logical grounds they can't refute, but they don't like it, and they deny (B) and largely ignore fallibilism.

I bring this up because, like many definitions of knowing, yours was ambiguous about whether infallibility is a requirement of knowing. So I'm looking for a clear answer about your conception of knowing.

Anthony O'Hear on Popper

Quotes are from the book *Karl Popper*, by Anthony O'Hear (AOH). It's in "The Arguments of the Philosophers" series edited by Ted Honderich. (Be careful, AOH has two other books with titles beginning with "Karl Popper".)

AOH says:

> Popper's attempt to dispense with induction is unsuccessful. [ch. 4, p. 57]

AOH says his reason, which he'll attempt to show, is:

> any coherent conceptualization of the experience requires the assumption of a **stable** order in the world. [ch. 4, p. 58, emphasis added]

Previously, AOH wrote:

> But, argues Popper, we can see on logical grounds that there is no such thing as a perfect repetition of any event. Similarity in all respects would mean that the two events were really identical, and so there would actually be only one event. So the repetitions we experience are only approximate. But this means that some features of repetition B of event A will be different from some features of A. Thus B is to be seen as a repetition of A only to the extent that we discount those features in which B differs from A. [ch. 2, p. 13]

So AOH ought to address the question: "Stable in which respects?" He ought to know that the world is stable in some respects and not others, just as the future resembles the past in some ways and not others, and any two observations are similar to each other in some ways and not others.

Saying the world is "stable" means just as little as saying two observations are "similar". Claiming a stable world means claiming *some* things stay the same over time (or at least only change a small amount, according to some suitable measure). Of course not *all* things stay the same over time.

So AOH needs to say what *type* of stability he's talking about for his claim to mean anything.

One of the standard problems with inductivists is their routine failure to understand this general problem (that when we compare non-identical things they're always both similar and different, and you have to specify what sort of comparison you're doing). What does AOH do about this issue? Nothing. After the "stable" claim I quoted, he immediately changes the subject to solipsism. He's apparently unaware of this issue, even though he discussed it earlier in the book.

AOH proceeds (p. 59) to talk about regularities and patterns of experience without talking about *which ones*. Of course there are some regularities and some non-regularities in the world. AOH's approach to epistemology is basically "We live in a stable world, so recognize regularities and project them into the future." This is standard inductivist, and misses the point in the standard ways, such as the issue of *which* regularities to project into the future and *how to find them* (how does thinking work? AOH just takes for granted that we find these regularities somehow – that is, his epistemology *presupposes* intelligent thought and fails to explain how thinking actually works. He starts in the middle.) Then:

> Our notion of an objective world, then, is reflected by the degree of continuing order and regularity that is to be found within our perceptions. [ch. 4, p. 59]

But Popper already explained the problem with this, and AOH already included that in this book. There is no such thing as "order" or "regularity" out of context. You have to first say which things you want to be the same which you'll count as being orderly or regular. Different aspects of the world are always similar (orderly, regular) in some ways and different in other ways. AOH doesn't address this.

I also found this bizarre statement:

> That a belief in induction is not something which can be dropped without substantial alterations elsewhere in our conceptual scheme is why the failure of Popper to develop a truly non-inductive science is not a chance result, but one with deep roots. [ch 4, p. 60]

But Popper was aware of this issue, and wrote about it, and did develop substantial alterations in our conceptual scheme. I would understand if someone thought Popper's substantial alterations were mistaken, or if someone was unfamiliar with Popper's writing. But AOH has studied Popper a lot, and then is apparently unaware this substantial alterations even *exist*. AOH even quotes and discusses some of them, but apparently(?) doesn't recognize their meaning and importance. This is just like the *similar in which respects* issue, where AOH quoted Popper about it and discussed it – but then later on he writes as if he was unaware of it (which I take to mean he doesn't fully understand it).

> the assumption that the world is not going to [suddenly become chaotic] [ch 4. p. 61]

The world is already chaotic in some ways and not others. So what does this mean? AOH doesn't say.

Does it mean the world won't suddenly become chaotic in *all* respects? But what would a world that is chaotic in *all* respects even mean? AOH doesn't address the issue and it's highly problematic.

One fairly technical way to approach the matter is via the theory of computation: consider whether there exist long bitstrings which can't be compressed by *any* compression algorithm (or, equivalently, can't be the output of any computer program, in any language, which is much shorter than the bitstring). Such a bitstring would be chaotic in *all* respects. But the answer is *no*, such a bitstring doesn't exist.

AOH might imagine that, all of a sudden, all the ways the world is stable stop working, and some new ones take their place. But that doesn't make sense, because no matter what happens, you can always retrospectively find regularities in the bigger picture including both before and after the so-called descent into chaos. All that's happened is this: from the infinitely many regularities compatible with the data you have, you favored some (why those? how were they chosen?), and found out those favored regularities were mistaken. (Meanwhile this so-called descent into chaos is fully compatible with some of the other data-compatible claims about regularities you could have made before it happened.)

So the assumption of the world's stability really means assuming your favored theories are correct. Why did you favor them over other theories, compatible with the same data, which make different predictions about the future? From the perspective of those rival theories, the future you predict is a descent into chaos. So when you say the world won't descend into chaos, you just mean the future will happen as you expect and not as your rivals expect – you mean the world *will* descend into chaos for the people who disagree with you, just not for yourself.

> Thus, I am not simply saying that our ability to distinguish between true experience and illusions depends on our once having experienced an orderly world, but that it depends on the continuance of whatever order we had previously recognized. But to assume this is just what, according to Popper, is deeply irrational, and which should be eliminated from our conceptual scheme. [ch 4. p. 61]

Yes, it is irrational. Because it consists of assuming you're right.

What does "whatever order we had previously recognized" refer to? There are infinitely many theories compatible with the data you've observed previously. To recognize some order means to choose some of those of those theories (why those? why not others?) to provide order to your thinking. Then to assume the continuance of that order means to assume that your choice of *which* theories to prefer won't turn out to be mistaken in the future.

The solution to all this is what Popper said: critical and explanatory thinking (which is literally evolution). We can only *conjecture* which of the infinite regularities (or, preferably, explanatory theories) compatible with our data are correct. And we can correct errors with criticism, which is how progress is made. (Part of this is explained by AOH, pp. 171-177)

AOH also objects to Popper's corroboration, and I agree that corroboration is a mistake. I have fixed that aspect of *Critical Rationalism*. You can **find my solution here**. For a quick overview, I also offer a *free* **short argument**.

Analysis of Robert Spillane Quoting Roger Kimball

> Rather than spend time constructing new sentences to respond to your questions and comments, I quote Roger Kimball (who made Americans aware of David Stove's essays).
>
> 'At the center of Popper's thinking about the philosophy of science is a profound skepticism, derived from David Hume, about the rationality of inductive reasoning...

This is incorrect. With Popper, I hold there is no inductive reasoning. Induction is a myth. No one has ever induced a conclusion. Since inductive reasoning doesn't exist, judging it as rational or irrational is beside the point. (This is why I try to ask questions about instructions for doing induction, or about which ideas to induce in a given situation. Without answers to these questions, then induction can't be done and can't reach any conclusion at all, rational or not.)

So the quote you're giving doesn't engage with the position I'm advocating.

> Like the young Hume, Popper concluded from the fact that inductive reasoning was not logically valid - that inductive evidence does not yield absolute certainty - that it was therefore incapable of furnishing compelling reasons for belief.

This assumes one has induced some conclusion(s) and the issue is to debate whether we should accept those conclusions are rational, valid, practical, partially certain, etc.

But, as above, that isn't the issue. So this isn't engaging with the position I'm advocating.

> Popper was a deductivist. He dreamt of constructing a philosophy of science based solely on the resources of logic.

No he didn't. Quote? Source? Conjecture – which played a huge role in Popper's epistemology – isn't deduction. Popper also emphasized explanation and problem solving, which aren't deduction.

> He was also an empiricist: he admitted no source of knowledge beyond experience. As Stove shows, the combination of empiricism and deductivism - in Hume as well as in Popper - is a prescription for irrationalism and cognitive impotence. An empiricist says that no propositions other than propositions about the observed can be a reason to believe a contingent proposition about the unobserved; an empiricist who is also a deductivist is forced to conclude that there can be no reasons at all to believe any contingent proposition about the unobserved.

Popper explained what we can do instead of having positive reasons: we can make unjustified conjectures. We can then use criticism to improve our ideas and make progress. Error elimination, not justification, is the key to epistemology.

Whether Popper (and I) are correct or incorrect about this view, the quote isn't discussing it. The quote isn't a reply to us.

> Hume himself, in his posthumously publishedDialogues Concerning Natural Religion, ridiculed this "pretended skepticism" as a juvenile affectation...
>
> 'Popper resuscitated Hume's brand of skepticism, dressing it up in a new vocabulary. In Popper's philosophy of science, we find the curious thought that falsifiability, not verifiability, is the distinguishing mark of scientific theories; this means that, for Popper, only theories that are disprovable are genuinely scientific...

What's wrong with that? And how can theories be verified? The quote doesn't say.

> Popper denied that we can ever legitimately infer the unknown from the known;

Yes, but Popper says we can know about the unknown by methods other than inference. This quote doesn't discuss that.

> audacity, not caution, was for him the essence in science; far from being certain, the conclusions of science, he said, were never more than guesswork...;

Right. What's wrong with that? Do you claim we can be certain – meaning we can have infallible knowledge? How?

> and since for Popper "there are no such things as good positive reasons" to believe a scientific theory, no theory can ever be more probable than another; indeed, he says that the truth of any scientific proposition is exactly as improbable as the truth of a self-contradictory proposition - or, "in plain English", as Stove puts it, "it is impossible".

The quote isn't providing a criticism of Popper's view. It also, so far, hasn't said anything about the two questions I asked.

> '...What was novel [about Popper's doctrine] was the amazing thought that positive instances do not - in principle cannot - act to confirm a proposition or theory. For Popper, if every raven anyone has ever seen is black, that fact gives no rational support for the belief that all ravens, in fact, are black.

Correct: if you want to argue that all ravens are black, you'll need a different argument that doesn't have a logical hole in it. This quote isn't stating what the logical problems with positive support are that Popper explained, nor answering Popper's arguments.

> Scientific laws, he says, "can never be supported, or corroborated, or confirmed by empirical evidence". He goes even further: of two hypotheses "the one which can be better corroborated, is always less probable." Whatever else these statements may be, they are breathtakingly irrationalist...

Rather than provide a criticism of Popper's view, the author states the view then calls Popper irrationalist (without defining it).

What am I supposed to learn from this?

> 'It would be difficult to overstate the radical implications of Popper's irrationalist view of science. Popper was apparently found of referring to

> "the soaring edifice of science". But in fact his philosophy of science robbed that edifice of its foundation. Refracted through the lens of Popper's theories, the history of modern science is transformed from a dazzling string of successes into a series of "problems" or ... "conjectures and refutations". On the traditional view, scientific knowledge can be said to be cumulative: we know more now than we did in 1899, more then than in 1699. Popper's theory, which demotes scientific laws to mere guesses, denies this: in one of his most famous phrases, he speaks of science as "conjectural knowledge", an oxymoronic gem that, as Stove remarks, makes as much sense as "a drawn game that was won." (This paragraph contradicts your statement that Popper supports a cumulative view of science).

Overall, the quote is full of conclusion claims instead of arguments. It also doesn't speak to the two questions I asked.

> (Roger Kimball, Against the Idols of the Age, Transaction, 1999, pp. xxi-xxiii).
>
> I have answered your questions #1 and #2 directly and indirectly.
>
> To repeat: in The Rationality of Induction, Stove has answered your questions.

I have the book but you didn't tell me which pages you believe answer the question. When you provide the page numbers which you claim answer me, then I'll read them.

> Needless to say, I agree with him.
>
> Specifically, Stove reduces inductive inferences to the inference from proportions in a population. As mathematician, James Franklin, writes: 'It is a purely mathematical fact that the great majority of large samples of a population are close to the population in composition'. In cases such as political polling the observed, if based on a large enough sample, is probably a fair sample of the unobserved. 'This applies equally in the case where the sample is of past observation, and the population includes future ones. The sample is probably still a fair one, and one can make a probable inference (unless, of course, one has further reason not to: probable

> inferences are always relative to the evidence at hand).' (J. Franklin, Corrupting the Youth: A History of Philosophy in Australia, 2003, p. 338).

For any finite set of data, there are multiple ways to infer from proportions in the population which contradict each other. So which inferences from which proportions is one to find (by what means?) and then accept?

Note that this is the same two questions I asked in my previous email. The questions were about (1) *which* ideas do you induce and (2) *how much inductive support* do they have (so, if there's more than one, which is accepted over the others for having more support?)?

Also, related, the future always resembles the past in some ways and not other ways. So how do you approach the issue of *which* proportions of populations will hold in the future and which won't?

If Stove answers this, simply provide a reference (page numbers) where I can find the answer.

> A quick look at 'The Analytic-Synthetic Distinction' on Wikipedia gives a list of those philosophers who have, quite rightly, rejected Quine's criticism. Quine is routinely quoted by philosophers and psychologists who, I suspect, have never read Ayer, Quine or Strawson. I say this because they rarely, if ever, make clear exactly what Quine's arguments were.

I don't care about lists of people who took some position, I care about arguments.

> Admittedly, your short rejection is not Quine's but the criticisms of Quine can be applied to you.

Which criticisms?

> You will never convince me that the following two propositions are logically and empirically the same: 'All tall men are tall' and 'All tall men are blond'. By rejecting the a-s dichotomy, you deny the possibility of necessary truths. Do you accept, then, necessary falsity?

No I don't accept necessary falsity. It's the same issue. To judge if 1+1=3 you still have to sum 1 and 1 and compare the sum to 3. The arguments I gave about

1+1=2 apply to this too.

10 Robert Spillane Replies

Robert Spillane's latest email didn't directly reply to **what I said previously**. Here it is with my new comments which attempt to get discussion back on track:

> 1. '3 am in the morning' is a pleonasm and thus necessarily true.
>
> 2. '3 am in the afternoon' is an oxymoron and thus necessarily false.

We need to conclude our discussion of whether 1+1=2 is a necessary truth before opening a new, similar topic. My answer to the 3am issue is similar to my answer to 1+1=2, which is the easier case to discuss and which I already wrote an explanation of. I await your next reply about that.

If I end up conceding the point about 1+1=2, I expect I'll also concede about the 3am issue without any additional arguments. And if you concede about 1+1=2, then I think your reasoning will be relevant to the 3am case and make it easier.

> 3. 'Induction exists' cannot be falsified.

Why? My position (which is also Popper's) is that induction has never had any set of followable instructions (steps) with the properties claimed by inductivists. So no one has ever done induction since inductivists have never defined any set of possible steps someone could do that would constitute doing induction. There are also arguments for why no such set of steps could be invented in the future. This is why I've asked questions about how to do induction (what the steps are).

> 4. 'Inductive logic' can be rejected if one argues that 'inductive logic' is an oxymoron. But since you don't accept oxymora, you have to argue that you reject 'inductive logic' on empirical grounds. How do you do that without distorting the meaning of 'empirical'?

I can use logical arguments. There's nothing wrong with logic. I just said the laws of logic are based on the laws of computation which are based on the laws of physics, and physics is an empirical science.

> 5. If you can't reject it on empirical grounds, all that is left to you are your feelings - and they are irrelevant since one cannot argue with feelings.

I agree that feelings are irrelevant. I haven't brought them up.

> 6. It is a truism that inference from experience is not deductive. A proposition may imply another proposition, but an experience cannot imply another experience. But you deny that there can ever be an inference from experience? That is untenable. What do you think 'inference' means?

Inference means "a conclusion reached on the basis of evidence and reasoning."

Induction refers to some specific ways of learning using experience. CR says those are poorly defined and actually impossible to do, and there are other ways to learn from experience which work instead (conjectures and refutations – evolution).

> 7. If Popper rejected induction, he has to be a deductivist - what else could a philosopher who calls himself a (critical) rationalist be?

A person who thinks most arguments are neither inductive nor deductive. Both induction and deduction are pretty specific categories which most arguments don't fit into. More on this below. BTW this has been noticed by a lot of people – e.g. it's the issue "abduction" is intended to address.

> 8. In his Unended Quest (Fontana, 1977, p.79) Popper writes: '...I could apply my results concerning the method of trial and error in such a way as to replace the whole inductive methodology by a deductive one. The falsification or refutation of theories through the falsification or refutation of their deductive consequences was, clearly, a deductive inference (modus tollens)...

That doesn't say Popper could or did replace the whole of thinking or arguing with deduction. Popper is just saying that if you accept basic (observation) statements then you can deduce to reject theories which they contradict.

> 9. You repeatedly claim that I do not engage with your position. But what exactly is the position of a person who rejects necessary truths and falsehoods, rejects induction and yet claims not to be a deductivist?

Why don't you quote what I write and reply to quotes more? I have asked you direct questions – e.g. the two about induction – and you haven't replied in this email. I also asked, again, for criticism of my position regarding 1+1=2 not being a necessary truth, and you didn't reply to that.

I take specific things you say and reply directly to them. But you mostly don't use that method when you respond to me.

I attempted to explain my position about non-deductive, non-inductive arguments with the price controls and socialism example. You didn't discuss it. I tried again by commenting on your argument about "mental illness" which you claimed was deductive, and you stopped discussing that too. If you will continue discussing one of the issues – especially if you quote what I say and reply directly to it – then I think we could make progress. I don't think it's a good idea to open another, new attempt to discuss the matter instead of continuing one of the discussions we were already having.

> 10. Where do your conjectures come from, since you deny they come from experience? And how do you refute them if not by deduction?

Brainstorming involves generating random variants of existing ideas. This is like genetic evolution which generates random variants of existing genes.

Many ideas are interpretations of experience. Interpreting experience is different than being guided by experience. Observations are passive data which can't tell us what to think. Instead we think for ourselves and some of our reasoning references observations, e.g. by critically pointing out that an idea contradicts an observation, or more mundanely e.g. by saying "I'm not going to go that way because I saw a cliff over there and I don't want to fall."

Ideas are refuted (in the context of a particular CR-problem) by criticism. A criticism is an explanation of why an idea doesn't solve a CR-problem(s). A "CR-problem" is very broad and refers to any type of achieving a goal or purpose, answering a question, etc – accomplishing anything you'd want an idea to succeed at. (I prefixed the word "problem" because it's Popper's terminology, I don't know a better word, but you objected to it previously so I don't want CR-problems to be mixed up with "problems" in your terminology.)

Explanation is a key part of thinking and arguing which is covered by neither deduction nor induction. Explanations discuss *why* and *how*. Statements

following a "because" are generally explanations.

If you carefully analyze the arguments from most thinkers, including Szasz and your own books, you'll find many of them don't follow the rules of deduction or induction, and involve explaining why some idea fails to solve a CR-problem(s).

This would involve carefully defining what qualifies as both induction and deduction. I've asked you questions about this regarding induction.

Regarding deduction, it's CR-problematic too. Deutsch discusses that some in FoR ch. 10, the chapter I referred you to previously. In short, people don't actually agree about what the rules of deduction are, and it's a very hard CR-problem to address. You may define "deduction" as only Aristotle's syllogisms, but then you'll find you can't prove much and you won't be able to classify very many arguments as deductive. If you want a broader deductive system, you'll have to specify it and address issues like Godel's incompleteness theorem.

You'll also have to face the CR-problem that you won't be able to rely on deduction to argue for your deductive system against rival deductive systems, or criticisms of why it's a poor system, or that'd be circular. My solution to that issue is that arguments about which deductive system is correct are regular critical arguments, just as people usually use. But since deduction and induction are your only tools, you will have a harder time figuring out how to make arguments regarding deduction itself without circularity.

More Induction Discussion With Robert Spillane

Robert Spillane thought this was particularly important and requested a direct answer. Here it is:

> 1. Two simple answers to #1 and #2 will suffice - yes or no.
>
> 2. 1+1=2 is a necessary truth; '1 pint of water + 1 pint of alcohol = 2 pints of the mixture' is not. Can you not see the difference between the two?

They have many differences and many similarities.

By "the" difference, I guess you mean: that "1+1=2" is a "necessary truth", while the other statement isn't. I don't agree with that because I don't think anything is a necessary truth.

Regarding induction, I've asked several times about a set of instructions someone could follow to do induction. I've been unable to get answers which address basic issues like telling you *which* ideas to induce and *how much* inductive support they have. Here's another failure to address the issue, and my comments. This is extremely typical of inductivists. They don't have answers to these questions and wouldn't be inductivists if they understood the questions.

> You asked me for details about Stove's Rationality of Induction. Here is a very brief summary (pp. 3-5, 22) which addresses your concerns:
>
> (1) 'That all the many observed ravens have been black is not a completely conclusive reason to believe that all ravens are black' is true and not contingent, even though it mentions two propositions which are contingent:
>
> (2) 'All the many observed ravens have been black.'
>
> and

> (3) 'All ravens are black.'
>
> But (1) is not contingent since it is enough to entail the truth of (1) that it is logically possible that (2) be true and (3) false, whereas something's being logically possible is not enough to entail the truth of any contingent proposition. Therefore, (1), being true and not contingent, is a necessary truth.
>
> Another way of saying (1) is:
>
> (4) 'The inference from (2) to (3) is fallible' and this is also a necessary truth.
>
> The inference from (2) to (3) is an inductive one. So there is at least one inductive inference of which it is necessarily true that it is fallible.

This doesn't answer my question about how (2) and (3) were selected from the infinity of propositions which do not contradict the observation data under consideration. Why those statements instead of some other statements?

I asked about *which* statements to induce and for instructions someone could follow to do induction, but this description doesn't provide instructions for how to select or create statements (2) and (3) in the first place.

What are the *rules* of induction? Could one write any statements at all in place of (2) and (3), or what? (I'm familiar with many proposed rules of induction, but none of them work. You apparently think you know of some rules of induction that do work, so I'm asking what they are.)

> (5) 'That all the many observed ravens have been black is a reason to believe that all ravens are black' is like (1) in that it is true but not contingent. Like (1) it mentions two contingent propositions, but it does not assert either of them. Its truth, therefore, does not depend on what their truth values happen to be.
>
> Another way of saying (5) is:
>
> (6) 'The inference from (2) to (3) is rational' and this, also, is a necessary truth (pp. 3-5).

> Since induction is necessarily fallible, the validity of induction is a subject easily exhausted. 'And as to the truth of the conclusion of an induction, or whether the conclusion of an induction with true premises is true, or whether more of such conclusions are true than are false: well, these of course are all contingent matters, with which philosophers have nothing to do. The success rate among inductions is as little the concern of philosophers as the blackness rate among ravens. Hume, in particular, was as little concerned as the next philosopher with what the long-run success rate of induction might be, and of course he said nothing about this subject; and a fortiori, he said nothing discouraging about it. Yet there are philosophers who do not shrink from the absurdity of implying that in order to 'answer' what Hume said about induction, we would need to establish something encouraging about the long-run success rate of induction. Some people just like to make rope neckties for themselves. But, in general, it is scarcely possible to exaggerate the harm that has been done to the philosophy of induction by philosophers who drift from the success of induction to the rationality of induction, and back again, and all over the place. Squalor rules, OK?' (p. 22).
>
> Now, you will probably reply that this is irrelevant to your concerns since it assumes induction and engages in arguments for and against its rationality. You, on the other hand, insist that induction is a myth. If by 'myth' you mean 'the presentation of facts belonging to one category in the idioms appropriate to another' (Ryle), this means that you accept that there are inductive arguments - from the observed to the unobserved - but believe they are inevitably invalid because the conclusions are not contained within the premises.
>
> But this is not your position. You claim that by 'induction is a myth' you mean that there are NO inductive arguments - that there cannot be (and never have been) arguments from the observed to the unobserved. This is a much stronger claim than 'inductive arguments are invalid'. It is also a claim that is so obviously false that further argument should be unnecessary.

My position that induction is a "myth", in the sense I've described (no one has ever induced anything), is from Popper. Do you know that's Popper's published view and know his reasoning? You are calling Popper's position "so obviously false that further argument should be unnecessary".

I (following Popper again – see e.g. his discussion of manifest truth) don't think that's a reasonable thing to say about anyone's position. The truth isn't obvious, and argument is necessary for dealing with disagreements.