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Abstract

Ian Deary and Robert Sternberg have been writing about intelligence differences since 1982 and 1977, respectively. As Deary was retiring at the end of 2020, they discussed an idea for their first joint paper. They composed five questions related to research on intelligence differences, about: attempts to find cognitive components of intelligence; the place of theory in intelligence research; the breadth of the concept of intelligence; hard problems in intelligence; and the use of cognitive tests in the real world. They answered them separately and then responded to the other's answers.

Introduction

Why did we write this piece? Robert Sternberg heard that Ian Deary was retiring at the end of 2020. An email exchange began, about their 40-ish years of publishing in intelligence, about their not having written anything together, about their different approaches to intelligence, about their career trajectories, and so forth. Deary suggested they write something together that might illuminate their respective approaches to key issues in the study of intelligence. They settled on answering a series of questions, which they composed and agreed upon. Each wrote their answer before seeing the other's answer. Then, when all the answers were in, they made short responses to the other's answers. There were some self-generated and -imposed rules about length of answer and response, and number of references, that they mostly adhered to.

This article is not a review, or even an overview, or a book chapter. It's more of a hybrid of opinion/editorial piece and magazine article. We hope that readers from different backgrounds will find some things of interest in it; we have provided sufficient, we think, references to follow up on what we have written. We invite readers to think what their answers would be to the questions, and what other questions might have been asked and answered.

Question 1

You began your research career on intelligence by looking at information-processing approaches (Sternberg = components of analogical reasoning; Deary = inspection time). How did that work out? Why is that not your main research topic now?

Information processing: Sternberg

When I published my first book on what I called "componential analysis" (Sternberg, 1977), based on my doctoral dissertation, I was convinced I had found 'the answer' to the true nature of intelligence. Factors, I thought, were manifest sources of individual differences in intelligence, but components of information processing were the latent or underlying sources of the differences represented by those factors. I was proud that, at age 25, when I completed my dissertation, I had discovered the truth about intelligence! And I had learned some really interesting things about intelligence, such as that one could model item response times with high accuracy; that although shorter latencies of most components (e.g., inference) were associated with higher intelligence, longer latencies of other components (e.g., encoding) were associated with higher intelligence; and most of all, that Spearman's (1923) model of the qualitative principles of cognition actually could be expanded upon and then applied to induction problems to help us understand *g.*
But there was an annoying finding, i.e. that the preparation-response component—extracted as the regression (or wastebasket) constant in the modelling—showed the highest correlation of all information-processing components with psychometrically-measured intelligence. That sounded like a rediscovery of *g.*

Of course, I hadn't really found 'the answer' to the true nature of intelligence. How many investigators had made that mistake before (or have made it after)—assuming that they knew the truth and that most everyone else was a fool and a believer in myths? Within a few years, I came, through my research, to the conclusion that I was not a whole lot further in my work than the psychometricians whom I had once disdained. They were looking at individual-differences sources of variation in intelligence-test scores; I was looking at item-difference sources of variation in intelligence-test scores; but that was only a partial answer to what intelligence is.

My later research would lead me to conclude that the intelligence as measured by the tests was not all of intelligence, but rather a limited, if important, part of intelligence. The tests neglected creative, practical, and, as I would conclude still later, wisdom-based aspects of intelligence. I should say that there is nothing new about this view. Implicit theories of intelligence, especially African ones, long have incorporated these elements in various ways (Grigorenko, Geissler, Prince, Okatcha, Nokes, Kenny et al., 2001; Sternberg, 1985b, 2004). Laypeople long have held considerably broader views of intelligence than the experts who have bought into the psychometric tradition of Spearman and his followers over the course of more than a century.

Some scholars, sometimes in critiquing an unpublished test I created in (Sternberg, 1991), the Sternberg Triarchic Abilities Test, have argued that *g* captures essentially all or almost all the reliable variance in all cognitive abilities (e.g., Brody, 2003). This argument does not hold up.

First, in some work, we have obtained significant negative correlations between tests of fluid and crystallized intelligence and tests of practical intelligence (Sternberg et al., 2001), a finding inexplicable on this point of view.

Second, we have shown statistically and practically significant incremental validity of measures of creative and practical intelligence over measures of general (analytical) intelligence in a large study of prediction of college success (Sternberg and the Rainbow Project Collaborators, 2006).

Third, although measures of IQ and *g* correlate with many things, they are not identical to measures of wisdom. For example, wisdom is only weakly related with general intelligence (Grossmann, Varnum, Kitayama, & Nisbett, 2016). And wisdom predicts measured well-being while holding verbal intelligence constant (Grossmann et al., 2013). Moreover, it seems questionable to equate the wisdom of people like Mahatma Gandhi, Nelson Mandela, or Martin Luther King, Jr., with their IQs. There are many smart people. In lay terms, they are a “dime a dozen,” found even among the best educated but least competent politicians the US and other countries have governing them. There are very few truly wise politicians or people in any other professions.

Fourth, as I argue at length elsewhere (Sternberg, 2021), the argument may be incorrect on its face, because it is precisely high-IQ, high-*g* people who have created the machinery behind weapons of mass destruction, carbon-belching vehicles of transportation, and destruction of the Amazon Rain Forest. It is an act of hubris, I suggest, for people who are destroying the environment not only for themselves but also for their children and grandchildren to laud themselves for their high practical or adaptive intelligence. Does high intelligence—which is allegedy about adaptation to the environment--consist of, or even allow, destroying the world as we know it for humans and millions of other species? Are we that tunnel-visioned—that supremely self-confident--in our thinking?

Although intelligence historically has been defined as adaptation to the environment ("Intelligence and its Measurement," 1921; Sternberg & Detterman, 1986), people can be high in IQ, but miserable in what I have come to call 'adaptive intelligence', the ability broadly to adapt to the environment in the real world (Sternberg, 2021) Those who are low in adaptive intelligence may use their general intelligence to make the world a worse place to live, if not for themselves, for others. Adaptive-intelligence abilities can be measured (Sternberg, 2021). For example, in our own work, we have asked participants to solve problems such as how to allocate water fairly to two countries that share a river along their borders and how to deal with industries that pollute and that accelerate global climate change.

The cognitive approach *is* useful for identifying information-processing elements of intelligence, such as componential and metacomponential (executive) processes, mental representations, strategies, the role of working memory, and the role of cognitive speed. But it succeeds by narrowing the construct of intelligence too much, leaving us understanding only a part of intelligence, and, I would argue, a part that is not going to help us negotiate a rapidly changing, distressed world. Fully to understand intelligence, we need not to confuse an important part of the whole of intelligence with the whole itself.

Deary's response

One can be interested in the nomological network of intelligence test scores and also recognise that there are other cognitive and non-cognitive sources of variance in human psychology. The test scores provide a solid empirical platform to work from; concepts invented beyond them often lack that, or are mostly them-again, in disguise. Whereas I did not become as adventurous in inventing new types and tests of intelligence (with whatever validity they have or don't have), I am less credulous about 'components' as valid explanations of intelligence differences, rather than just their (interesting) correlates; thus, I am more Eeyore-ish than Sternberg about both cognitive components and his Tigger-ish new approaches to intelligence. By the time I was planning *Looking Down on Human Intelligence* (Deary, 2000), I saw that (R.J.) Sternberg's reasoning 'components' and other cognitive 'components' (from, e.g. Hick, S. Sternberg, and Posner tasks) were slices of complex problems—not underlying brain processes, and not externally validated—and that they always correlated with intelligence test scores less well than the overall solution time/reaction time (Deary, 2000, chapters 5 & 6). *Rant #1*: cognitive components have not yet got under the skin of intelligence test scores.

Information processing: Deary

My introduction to intelligence research was, as an undergraduate, to study the association between intelligence test scores and visual and auditory (I devised the latter concept and measurement) inspection time (Brand & Deary, 1982). The attraction of inspection time was that it seemed to offer one step on a reductionistic path to understanding human intelligence differences (e.g., Deary, 1996). The intelligence-inspection time association appeared (Netelbeck & Lally, 1976) at a time when there was an upsurge of interest in finding components of cognitive processes that had individual differences that correlated with scores on higher-level, psychometric intelligence tests. There were components of various reaction time procedures and components of, for example, analogical reasoning, such as Bob Sternberg's (there's an overview in Deary, 2000). I thought inspection time appeared, among these, to have the most promise. As a psychophysical test and index, it appeared relatively ‘basic’, but only relatively. There was no response time involved in the procedure, just an assessment of how much information could be extracted from a brief exposure of a simple stimulus. In other reductionistic research, in the 1980s and 1990s, there were associations between intelligence test scores and genetics and with brain size. However, for both of those, the explanatory gap seemed larger. The gap between higher-level cognitive test scores and inspection time performance seemed less. Inspection time had an attractive-looking theory, too, with relatively few parameters (Vickers, Nettelbeck, & Willson, 1972). However, like many theories of psychophysical and cognitive functions, even such simple ones, the validity of the parameters, especially with respect to mapping on to brain structure and function, was lacking. Most early and later studies were on visual inspection time tests (e.g. see the special issue of Intelligence on inspection time, celebrating 25 years of research on its association with intelligence; Petrill & Deary, 2001); my BSc and PhD theses explored auditory inspection time in relation to higher-level intelligence-type test scores.

Why didn’t I stick with inspection time? In a way, I did. I first published using inspection time in 1982 (Brand & Deary, 1982), and I had a paper in 2019 on inspection time, alongside measures of brain white matter integrity (Deary, Ritchie, Maniega, Cox, Valdes Hernandez, Luciano et al., 2019), and I expect there will be a few more to come. In that recent paper, I still argued for the relatively-strong tractability of inspection time by comparison with other measures of so-called processing speed, bearing in mind that processing speed remains an important variable for those studying the brain’s white matter connections. Inspection time still appears to me to be a relatively tractable assessment of speed of information processing, in part because it is not muddied by response time and it also appears relatively culturally-reduced in its content and demands. I've included it in the triennial assessments of the Lothian Birth Cohorts (Taylor, Pattie, & Deary, 2018), and that has afforded the study of how it ages, and its associations with brain imaging variables and other cognitive test scores (e.g., Ritchie, Tucker-Drob, & Deary, 2014; Penke, Munoz Maniega, Murray, Gow, Valdes Hernandez, Clayden et al., 2010). The Lothian Birth Cohorts were also a problem for my attention to inspection time; when we discovered that the Scottish Mental Surveys' data still existed (Deary, Whalley, & Starr, 2009) and foresaw their possibilities for follow-up and making practically-important contributions to research (through cognitive ageing and cognitive epidemiology), inspection time was side-lined in my research portfolio; even though it was included in the cohort's cognitive assessment, it had a walk-on part rather than a starring role.

Inspection time first emerged in 1972 (Vickers, Nettelbeck, & Willson, 1972). Despite the relatively sizeable number of papers on inspection time over the years—on its associations with higher-level cognitive tests (around 0.3; e.g., Deary, Johnson, & Starr, 2010), on psychophysics (Deary, Caryl, & Gibson 1993), on brain imaging (e.g., Deary, Simonotto, Meyer, Marshall, Marshall, Godard et al. 2004), on ageing, on genetics (e.g., Edmonds, Isaacs, Rogers, Lanigan, Singhal, Lucas et al., 2008), on psychopharmacology (e.g., McCrimmon, Deary, Huntly, MacLeod, & Frier, 1996), in medical situations (e.g., Scotland, Whittle, & Deary, 2012; Shipley, Deary, Tan, Christie, & Starr, 2002) etc. (see also Deary, 1996 for an overview; and try typing 'Deary inspection time' into PubMed)—it remains under-investigated. The effect size of the correlation means it should be explored more; as Funder and Ozer (2019) advise, "*r* of .20 indicates an effect of *medium* size that is of some explanatory and practical use even in the short run and therefore even more important, and an effect-size *r* of .30 indicates an effect that is *large* and potentially powerful in both the short and the long run." I recall my surprise when I first heard that such an apparently simple task correlated moderately with what people called IQ scores. It vexes me that that my curiosity has not been more fully followed up. The finding remains curious—at least it has not been explained away—and deserves more attention. The choices I had to make about research topics are well described in Robert Frost's poem 'The Road Not Taken': "Two roads diverged in a yellow wood/And sorry I could not travel both…". I think inspection time has outlasted other information processing indices from its era of origin, although I am also impressed by the associations between mean 4-choice reaction time (around 0.4 with representative samples; a "very large effect size," according to Funder and Ozer [2019]) and intelligence when the association is studied in population-representative samples (Der & Deary, 2017). Inspection time has the advantage over simple- and choice-reaction times—even if its typical correlation with intelligence is a bit smaller than those with choice reaction time—that it is not contaminated with movement time. When one sees claims that intelligence might have an origin in working memory or executive function or some such, one looks at those constructs' tests and finds it easy to object that they look too much like intelligence test items to provide any reductionistic traction; one could not say that about inspection time: it still looks quite simple, and, therefore, surprisingly well correlated with intelligence test scores. It deserves more dedicated research.

Sternberg's response

I have always greatly admired both Deary and his work on inspection time. He has expanded the field into areas that previously were almost untapped, and his Lothian Birth Cohort Study has been a singular contribution to the field, perhaps unlike any other before or since.

Indeed, one of the greatest shortcomings of my own work is that I have not had funding to do longitudinal studies.   I do remember taking Deary's auditory IT test and doing embarrassingly badly on it! I have four concerns about this kind of cognitive-correlates work, in general.

1. *It assumes that intelligence is what IQ tests measure*—that such tests are an adequate basis for criterion validity.
2. *It starts with a task rather than a theory*. The theory of intelligence is developed from the task rather than the other way around.
3. *The correlations are weak to moderate.* They certainly are not high enough to infer a solid or strong relationship between predictors and criteria.
4. *Correlations have no causal claim in any case*. Regardless of level, we do not know whether performance on the task is causal of intelligence, intelligence is causal of performance on the task, or most likely, whether some higher-level variable is responsible for both.

Question 2

What is the place of theory in intelligence research?

Theory: Sternberg

My work always has been theory-based. However, the theory has evolved over time. The goal of theory, in my work as in that of others, is to describe, explain, predict, and provide heuristic value for future research.

My first theory was a componential theory of human intelligence, based on the notion that intelligence could be understood in terms of basic information-processing components (e.g., Sternberg, 1977, 1983). I suggested that three kinds of components underlie intelligence: metacomponents, or executive processes; performance components, which execute the instructions of the metacomponents; and knowledge-acquisition components, which learn how to solve the relevant problems in the first place. In our research, we showed that it was possible to predict large proportions of variance in reaction times using componential models that specified strategies of problem solving and successfully to relate individuals' component scores to psychometric test scores.

By the mid-1980s, I had decided that this theory was too narrow. My experiences teaching graduate students had convinced me that whereas standard intelligence-test items, on which I had based my earlier work, were reasonable measures of *g* or analytical intelligence, they failed to measure creative and practical aspects of intelligence. My second, triarchic theory of intelligence (Sternberg, 1985a) consisted of three subtheories—componential, which specified the elementary information-processing components; experiential, which showed how the components range in their execution from coping with relative novelty (creative intelligence) to automatization; and contextual, which specified that the components are applied to experience to adapt to, shape, and select environments (practical intelligence). In our research on this theory, we showed how it was possible to measure not only analytical, but also creative and practical aspects of intelligence.

By the mid-1990s, I had become convinced that there was no single formula that would capture how to combine the analytical, creative, and practical aspects of intelligence.

Rather, people were successfully intelligent, as I called it, when they identified and capitalized on their strengths and identified and either compensated for or corrected their weaknesses (Sternberg, 1997). In the ensuing years, I applied the third theory, that of successful intelligence, to diverse measurement problems, such as measuring practical intelligence (Sternberg et al., 2000) and measuring intelligence across cultural settings (Sternberg, 2004). In this work, we constructed tests that could be used to measure successful intelligence in cultures with adaptive requirements very different from those of the industrialized West.

However, I later came to see the theory of successful intelligence also as incomplete. According to the fourth, augmented theory of successful intelligence, to be successfully intelligent, one needs to be creative in coming up with new ideas; analytical in discerning whether they are good ideas; practical in ensuring that the ideas can be implemented and that one can persuade others of them; and in the augmented theory, wise in ensuring that the ideas help to achieve a common good; over the long as well as the short term; balancing one's own, others', and larger interests; through the infusion of positive ethical values. Too many people, I believed, were using their ideas toward bad ends (Sternberg, 2003, 2020a). In our research, we measured augmented successful intelligence as relevant for admissions to universities (Sternberg, 2010). For example, my colleagues and I constructed theory-based tests that could be and were used for college admissions. We found that they could substantially increase prediction of academic and extracurricular success while decreasing ethnic-group differences relative to the SAT.

More recently, however, I have come to the conclusion that the augmented theory is itself incomplete. The reason is that even with an augmented view of successful intelligence, people have seemed to be using their intelligence toward ends that are contrary to the very definition of intelligence as adaptation to the environment. Intelligence encompasses behavior leading to adaptation to (and also shaping and selection of) environments. In my fifth theory, of adaptive intelligence, I have argued that traditional notions of intelligence have failed because they have valued behavior that actually is anti-adaptive—hastening the end of the world as we know it for humanity and many other species (Sternberg, 2021). Many people's intelligence is being used not to prevent, but actually to hasten climate change, pandemics, bacterial resistance to antibiotics, pollution, income disparities, and other societal ills. Despite steep 20th century increases in IQs, many so-called "intelligent" people seem unable to function in ways that create a liveable world rather than one that is falling apart around us. What good to the world is IQ, or *g,* if either is powerless to produce the adaptation that creates a liveable environment for ourselves and for future generations—if it is used to destroy the very environment in which we live and our progeny will live? Which is truly adaptively unintelligent, having a low IQ or walking around without a mask in times of COVID-19, infecting and possibly killing others? None of this is to say that IQ is unimportant. Higher IQ can lead to many forms of enlightenment (Deary, Batty, & Gale, 2008). My point is simply that high IQ in itself is not enough for positive adaptation and shaping of the world and is vastly overrated for its importance by many psychologists of abilities as well as laypeople.

Deary's response

Proper theories are predictively-powerful entities, based on known SI-units, and/or on known/newly-devised constructs that are validated independently (not jangles of better-validated constructs). I don't recognise the above as being theory; it is an accumulating list of apparent problem-solving capabilities and real-world problems. It's OK to catalogue the cognitive things that people appear to do—and which show individual differences—and to seek associations between those differences and life's outcomes. I sympathise with Sternberg's itch to complete this catalogue, but is the above a theory?; I don't think so. It seems too driven by face validity; there are too many skyhooks, and too few cranes (Dennett, 1995). It's, as-yet, a *Glass Bead Game* competition entry of invention rather than discovery. Less hifalutin'-ly, it reads like a *Supermarket Sweep* of ways that people appear to think and problems of the world, bundled into a trolley; there has to be a reckoning. There's more criticism of so-called intelligence theory in Deary, Cox, and Ritchie (2016). And, when I did devise a theoretical construct to account for part of the intelligence-survival association—'system integrity'—, look at how I agonised over the fact that it needed more validation, and my embarrassment at people's embracing it too readily (Deary, 2013b). *Rant #2*: most 'theories' of intelligence debase the word theory.

Theory: Deary

During my time in psychological research, I have been unimpressed by so-called 'theories' in psychology. I think I am in good company in thinking that grown-up theories are absent in psychology, including in intelligence research (Borsboom, van der Maas, Dalege, Kievit, & Haig, 2021; Eronen & Bringmann, 2021; Yarkoni & Westfall, 2017; Yarkoni, 2020). Often, they are banal and otiose; worse, they are often beguiling and wrong, and often lack—and sometimes wilfully ignore—well-established empirical findings. At the milder end are the innumerable weeds of box-and-arrow models with invented skyhook-y (Dennett, 1995) non-validated constructs, entities with no explanatory power and that are often ‘jangles’ (Kelley, 1927) of better-validated constructs. In intelligence research, this might apply to theories that try to recast/remove *g* and to those that ‘explain’ intelligence as, for example, working memory or executive function (e.g. see Deary, Cox, & Ritchie, 2016). At the more severe end are the theories that catch on and waste decades of researchers’ time to clear away. In intelligence, this might apply to Gardner’s and Thurstone’s ideas about ‘multiple intelligences’. Beyond intelligence, Freud’s and Piaget’s ‘theories’ are good examples of invasive species that have taken a while to be cleared from psychology, though they still infest and litter the chattering-classes’ media. What I shall do here is expand the literary support I had for my view about being humble with respect to theory as applied to intelligence in my *Looking Down on Human Intelligence* (Deary, 2000, chapters 4 and 10), and then make a case for intelligence research without the sort of ‘cargo-cult’ theory (Feynman, 1985) that is too-commonly expounded in psychology.

In *Looking Down on Human Intelligence* (Deary, 2000), I quoted George Meredith (1859, 1935, p. 89), from *The Ordeal of Richard Feverel*: “There is nothing like a theory for blinding the wise.” *Vide* Gardner’s and Thurstone’s neglect of *g*; enough said. Here, I recruit another great writer, and another great book. George Eliot (1872, 1930) wrote, in *Middlemarch*: “But let the wise be warned against too great readiness at explanation: it multiplies the sources of mistake, lengthening the sum for reckoners sure to go wrong.” (vol. 2, p. 13); and “Everybody liked better to conjecture how the thing was, than simply to know it; for conjecture soon became more confident than knowledge, and had a more liberal allowance for the incompatible.” (vol. 1, p. 10). Between them, those three short quotations pithily capture the problems that psychological theories can produce, *viz*. the ignoring of what the data say in the form of robust empirical regularities, the multiplication of redundant/imaginary constructs, the limitation of the power of reason in multivariate situations, and the comfort and pleasant cognitive pliability of theory. Indeed, the part of Eliot's first quotation after the colon could summarise Hayek's warnings about our ability to understand what is going on in complex psycho-social situations (Miller, 2010, especially Ch. 3).

Shedding the carapace of theory, as it *usually appears in psychology* (I stress that, because I recognise that there are grown-up theories in other areas of science), does not mean that one is impotent in doing research, including on intelligence. A problem with theory—in its debased form that one sees in psychology—is its comforting nature and misleading persuasiveness; as Roger Scruton wrote, "the fictions were far more impressive than the facts." Well, let's look at some impressive and interesting facts (empirical regularities) about intelligence test scores. When I re-wrote my *Intelligence: A Very Short Introduction* (Deary, 2020), I found space for many ideas about cognitive test scores that has empirical regularities: (1) *description* in the form of their covariance structure (the hierarchy of cognitive tests' covariances; the ageing of different cognitive capabilities; sex differences in intelligence); (2) *explanation* in the form of their causes (environmental and genetic contributions to intelligence test scores; brain correlates of intelligence; processing speed and intelligence); and (3) *prediction* in the form of their outcomes (educational and occupational correlates of intelligence; health and mortality correlates of intelligence). All of those appear to me to be important aspects of an important human quality's individual differences; they show robust empirical regularities, and they do not decry or exclude other types of cognitive difference research. All could be done without a theory *as it is usually understood and formulated in psychology*. And so could other topics that could have been included, such as improving intelligence (good luck with that) or how intelligence does or does not relate to other human qualities.

A failure of theory-driven research on intelligence were the candidate-gene studies that failed to reel in a single genetic variant correlated with intelligence (Chabris, Hebert, Benjamin, Beauchamp, Cersarini, van der Loos et al., 2012). I don't say that with any pleasure, because we did some of those candidate gene studies, based on what we thought were good ideas about which genes' variants would relate to cognitive capability. On the other hand, the hypothesis-free genome-wide association approach has been the one that has netted genetic variants that are reliably associated with intelligence test scores (Davies, Lam, Harris, Trampush, Luciano, Hill et al., 2018). Of course, one might argue that the 'infinitesimal' genetic model that R. A. Fisher published in 1918 could have warned against candidate gene studies and predicted what modern GWAS results have shown, but Fisher's foresight is mostly being realised/celebrated in retrospect, including for the centenary of his paper (Visscher & Goddard, 2018).

None of this is meant to imply that there are not contestable ideas or hypotheses about intelligence differences, nor that the gathering of data is free from preconceptions, nor that a passable theory could be constructed of some aspect(s) of intelligence differences. No, the animus here is against vainglorious, over-reaching theories that cannot help—given the complexity of our topic—but be wrong and diverting from more helpful endeavours. As Mr. Brooke says in *Middlemarch* (vol. 1, p. 10), "The fact is, human reason may carry you a little too far—over the hedge, in fact. It carried me a good way at one time; but I saw what it would not do. I pulled up; I pulled up in time. But not too hard. I have always been in favour of a little theory: we must have Thought; else we shall be landed back in the dark ages". That's it: let's have a little theory, but don't get carried away and waste people's time.

I did once (Deary, 1988) write an account of how theory had paved an argument about how adaptive immunity worked, and how this might be applied to intelligence. In the middle of the 20th century, it was not known how humans and other organisms were able to produce antibodies that were capable of attacking invading micro-organisms. These invaders, it was known, had shape-specific antigens on their surfaces that formed the bases for our attacking them with shape-complementary-specific antibodies, produced by lymphocytes. There were two theories. The template or instructionist theory, suggested, for example, by the Nobel prize winner Linus Pauling, hypothesised that our immune system modelled new antibodies by using antigens as a template. The opposing theory—clonal selection theory—was suggested by Nobel prize winner Sir Frank Macfarlane Burnett. He appreciated that species changed to the environment by mutation and selection, i.e. Darwinian evolution. He also appreciated that acquired bacterial resistance to antibiotics occurred by the same process, i.e. those bacteria that had mutated so that they became resistant to antibiotics were more likely to survive and propagate than those that had not. He reckoned that, because two adaptation-to-the-environment processes followed Darwinian selection, then antibody production might, too. He developed the 'clonal selection' theory. This hypothesised that we—humans and other species with immune systems—already had a library of lymphocytes that would recognise antigens on micro-organisms and that, when we were invaded with a given micro-organism, the specific subset of lymphocytes that recognised that micro-organism was selected and multiplied, and then produced antibodies to kill the invader. I describe this at some length because think it described what theory can do, i.e. take basic principles and apply them to make predictions about a new area. I shall give an example of how that's been suggested as format for one theory (mutualism) of intelligence, after making some interim points.

Much of what is called 'theory' in intelligence is just suggested or established empirical regularities, what one recent account calls 'phenomena' (Eronen & Bringmann, 2021). For these authors, two main limitations to theory development are the problems with the validity of psychological constructs, and the lack of established robust phenomena (what I call empirical regularities). Therefore, *g*, I think, is a robust empirical regularity/phenomenon, as is the hierarchical model of cognitive test covariation; it is not a theory. The differentiation hypothesis in cognitive ability is a suggested empirical regularity—that *g* accounts for more of the variance in cognitive test scores at lower levels of ability (Deary, Egan, Gibson, Austin, Brand, & Kellaghan, 1996); it is not a theory. There is some evidence that it occurs (Tucker-Drob, 2009), although studies have rarely implemented the five methodological points made by Deary et al. (1996). Cognitive reserve is a suggested empirical regularity—that has not been easy to operationalise empirically—and not a theory (Staff, Murray, Deary, & Whalley, 2004). One of the problems with cognitive reserve is that it sounds attractive and people cite it with poor operationalisations. We (Staff et al, 2004) operationalised it as, "We formulated tests of the reserve hypotheses as follows: if the cerebral reserve hypothesis is correct, then the measure of reserve [e.g. education] should account for significant variance in the cognitive outcomes in old age after adjusting for variance contributed by childhood mental ability and burden [e.g., brain white matter hyperintensities]. In other words, possessing some reserve means that one's cognitive score is greater than would be predicted from the person's childhood ability and the amount of overt, accumulated burden." I think this provides a testable suggestion for an empirical regularity and avoids the hand-waving, fairy-dust-sprinkling waffle that poorly-operationalised cognitive reserve-as-theory frequently provides in papers. Indeed, this clear operationalisation pretty well reduces the 'cognitive reserve theory' to the superfluous status of 'soup stone' (Navon, 1984). System integrity also has the status of suggested empirical regularity, I think (Deary, 2013). Although I devised the idea (Whalley & Deary, 2001), I dislike the fact that the idea is rather uncritically liked and cited, and sometimes called a theory. The five factor model of personality is a suggested empirical regularity, with a good nomological network, but not a theory (Matthews, Deary, & Whiteman, 2009). Indeed, I think that the descriptive-predictive-explanatory research model I describe above for intelligence research also applies to personality traits (Mõttus, Wood, Condon, Back, Baumert, Constantini et al., 2020). I think some constructs within intelligence's purview—for example, executive function and health literacy, which are largely 'jangles' for general intelligence—don't even get to that stage, because they fail at the previous hurdle of construct validity.

Above, I mentioned that borrowing principles from Darwinian evolution proved successful in formulating the clonal selection theory of adaptive immunity, which was shown empirically to be correct. In psychology, the borrowing of mutualism—that, in an ecosystem, various species might promote each other's growth—has been used to account for *g*, the hierarchy of cognitive covariances, the increase in heritability of cognitive ability with development to adulthood, and the ageing of cognitive functions (van der Maas, 2006; Borsboom, van der

Maas, Dalege, Kievit, & Haig, 2021). However, I am not clear that mutualism theory in intelligence has done more than join a few dots, akin to the way we join stars to make constellations' shapes. I and colleagues have previously and extensively criticised the mutualism model and also Spearman's and Thomson's models of why the positive manifold of cognitive test score correlations might occur (Bartholomew, Deary, & Lawn, 2009), and also the model of Kovacs and Conway's (2016) 'process overlap theory' (Deary, Cox, & Ritchie, 2016); it's not so much that they are wrong, but that they don't offer critical tests at key points.

Therefore, let's retain 'theory' for, e.g. Copernicus, Darwin and Einstein, and let's not devalue its currency by calling the established/empirical regularities in psychology, theories. Far less should we call the psychological castles-in-the-air theories. I am agnostic about what a real theory in psychology might do or add. Say, for example, we had found that general intelligence was related to brain structure and we had also found all the low-level neuronal variables that accounted for that, and then tied some of those associations to variation in gene systems and genetic variants. And that we also knew that general intelligence related to life outcomes. Where would theory sit, and what would it do for us? I don't think we should only try to predict in intelligence, and in psychology more generally—because we want to understand the biological and social origins of, say, intelligence differences as well as predicting from them—but we should not undervalue a strong and elaborate nomological network of empirical regularities, such as we have with intelligence differences just because we haven't needed or invented a theory around them (Yarkoni & Westfall, 2017; Yarkoni, 2020).

I have probably written enough here now; however, I did write extensively on the need for addressing construct validity, ensuring empirical regularities, and working hard on making sure there are good nomological networks for intelligence—and about how theory has blighted psychology—in my now-20-year-old *Looking Down in Human Intelligence* (Deary, 2000, Chapters 4 [Vade-mecum] and 10 [Den Finger in die Wunde legen]). I have read these chapters again just now and stand by what I said there as being applicable today; I recommend them as additional reading.

Sternberg's response

Theory matters!

First, theory is what gives meaning to data. Einstein's theory of general relativity explains and shows the interrelations between an astonishingly wide range of scientific findings, some of which otherwise might have seemed unrelated or even nonsensical.

Second, theory provides predictive power. Darwin's theory of evolution predicts species survival as a function of adaptivity of traits to ever-changing environments.

Third, tested theory provides the basis for disconfirmation of explanation, which is how we know that we can expect germs, not miasma, to cause illness.

Fourth, good theories have heuristic value. They suggest better and worse paths for future theory and research

The problem with lack of theory is well illustrated by cognitive-correlates work, in which intelligence researchers have made numerous post hoc claims for causal relations between their particular tasks and intelligence on the basis of weak to moderate correlations of no probative value.

Question 3

How broadly should we extend the concept of intelligence?

Breadth of intelligence: Sternberg

How broad intelligence, or any concept should be, is a matter of accounting for its definition and no more. There are many definitions of intelligence, but a common feature of diverse and time-honored definitions of intelligence is that intelligence involves adaptation to the environment (Binet & Simon, 1916; Neisser, Boodoo, Bouchard, Boykin, Brody, Ceci et al., 1996; Wechsler, 1940), or what Gottfredson (1997) refers to as, "figuring out what to do."

The greatest problem for the field of intelligence research, I believe, is that it started off putting the cart before the horse; worse, over a century later, it still is doing so. Even Carroll's (1993) landmark theory is a reanalysis of factorial data from previous atheoretically-generated tests. Science is supposed to start out with theories that give rise to hypotheses, which then give rise to empirical tests of those hypotheses, by which the theories that generated the hypotheses are either supported or not. The field of intelligence instead started out with atheoretically-generated tests that measured a construct, so-called "general intelligence" or *g,* plus other less general elements, captured in group and specific factors. Theories were derived from data, rather than according to standard scientific method, which is exactly the other way around. Post hoc claims were then made once the data were collected.

Why is this a problem? Because the theories are then constrained by whatever happened to be measured by the somewhat arbitrary choices of items that originally generated the data to be analyzed. The industry then became one enormous confirmation-bias machine, where new tests were validated by measuring the same thing as old tests. Even theory-based tests draw on theories that originally were derived from the atheoretically-generated items. But the question of what constitutes adaptation to the environment was left behind, which is too bad because it formed the basis for what intelligence is *beyond* what the tests test (Boring, 1923).

When I refer to confirmation bias, I refer not to the positive manifold among conventional psychometric tests, but to the inability of such tests to go beyond the boundaries of the abilities that have become so dear to so many psychometricians and other scientists. Indeed, we have shown in multiple studies that high *g* can be a poor predictor of scientific thinking and especially creative scientific thinking (Sternberg & Sternberg, 2017; Sternberg et al., 2017, 2019, 2020). Society's overuse of analytical tests essentially may funnel into science those who view analytical intelligence *(g)* as important—after all, it is part of what got them where they are—but will deprive the field of creative thinkers whose strength is not in tests that are highly imperfect predictors of the skills that lead to creative scientific thinking.

If we start with intelligence as adaptation, we don't actually worry about "how broad" the concept of intelligence should be, because rather than using the cart-before-the-horse approach of asking how far we should go beyond the *g* or IQ concepts—which were purely derivative from data—we start with the question of what a theory would need to encompass to account for adaptation to the environment. We would be testing theories not against a confirmation-bias machine, whereby new tests are correlated with old tests on the assumption that the new tests are good if they measure the same thing as the old tests, but rather, against operationalizations of adaptation to the environment. Other criteria also are problematical, because they are culturally determined to favor the dominant culture. As the tests were also constructed by the dominant culture, there is no appearance of statistical bias because the predictors and criteria share the same bias.

One might suggest that some tests are culturally reduced, even to the point of being culture-fair. On their face, for example, tests of abstract reasoning or of inspection time might seem to be tests filling this bill. But tests of abstract reasoning have proven to be precisely the intelligence tests *most* susceptible to the Flynn effect (Flynn, 1987), showing that they are the tests on which performance is most culturally affected, presumably because abstract reasoning with geometric figures is so profoundly a cultural act, central to Westernized, postindustrial education but not to many other forms of education; similarly, all speeded tests are culturally loaded, because not all cultures value speed, and some might value much more slow deliberation and careful reflection (Sternberg, 2004).

A reviewer of this article asked whether it could "be argued that IQ test items represent a sample of problems faced in modern societies and tap the ability to adapt to (and solve) these problems?" I would answer with an unequivocal "no." Table 1 addresses this question. The characteristics of problems faced in modern society are extremely different from the characteristics of IQ-test problems. It is for precisely that reason that IQ tests, although somewhat predictive of performance on many real-world tasks, always will be very limited in their prediction. They predict to a kind of problem solving that is extremely different from that required in the real world.

[Insert Table 1 about here]

One could argue that the characteristics in Table 1 deal only with face validity, not empirical validity. But the problem is that the measures used to assess empirical validity are usually contaminated, as test scores in part determine the life opportunities that enable people to attain higher scores on whatever criteria are assessed. And most of them are criteria valued by one culture or subculture (e.g., school grades, completion of years of schooling, or prestige of job), but not necessarily by others. Attempts have been made to look at more consequential criteria by assessing complex problem solving (e.g., Greiff et al., 2013a, 2013b), but these problems also do not fulfill all or perhaps even most of the criteria noted in Table 1.

The field's operationalizations of adaptation to the environment often have been narrow, as anyone may have noticed who is simultaneously living through a pandemic and the greatest political polarization in the United States since the US Civil War. Comparable polarization is happening elsewhere. What good are criteria such as individual grades or income, if the people with the grades can't breathe the air, drink the water, stay healthy, or talk to their neighbours with differing political views? How broad should a theory of intelligence be? It should be broad enough to identify those individuals who can contribute to the solution of these real-world adaptive problems, which bear none of the narrow characteristics of IQ-test problems.

Our conception of intelligence has to be broad enough to describe, or hopefully, explain how people solve real problems that are almost the antithesis of IQ-test problems, problems that (a) are not pre-recognized, (b) are not predefined, (c) are unstructured, (d) have no single correct answer, (e) are emotionally arousing, (f) are for high stakes, (g) need to be solved collectively, (h) transform themselves over time, even as we are solving the problems, (i) have multiple paths to solution, and (j) are never multiple-choice. Criteria also need to be collective ones critical or adaptation, not only culturally-defined but somewhat arbitrary measures of individual success.

My theory of adaptive intelligence (Sternberg, 2019, 2021) seeks to address these issues. Other theories could too. But they will not if they stay wrapped in the small box that the work of early psychometricians created for the field, the box that demands that conceptions of intelligence stay narrow because the boundaries of the box are deemed to be more important than what the box contains.

Deary's response

A detail, first: Carroll's (1993) model is not a theory; it is a description of an empirical regularity—known at least since 1940 (Vernon)—of the variance/covariance patterns of cognitive test scores; it is the pattern found in almost all big data sets. It can be tested—and wins out—versus other descriptions. The fascination of psychometric intelligence research is not its confining by the narrow range of tests first chosen; it was that the results emerged almost no matter which types of cognitive items were used. Science need not start from theory. It can proceed by trying to explain replicable and interesting accumulations of findings in data; *vide* Darwin. Eronen and Bringmann (2021) described clearly how Darwin collected large numbers of well-established empirical regularities (what they call "phenomena") *before* building a theory, e.g., "These phenomena were therefore generally agreed on in the scientific community and imposed very strong constraints on the space of possible theories. A theory of evolution had to fit with not just one or two of these robust patterns but with all of them." Remember, regarding theory, and its mini-me, hypotheses: *ex nihlio nihil fit*. Research can usefully spot some data-pattern/result out there that's interesting, and pursue it, like a handhold or foothold when climbing. It's valuable to proceed by exploring well-replicated empirical regularities. I worry about Sternberg's looking at world phenomena and introjecting to invent a structure of intelligence, or its failures; it appears to assume a simplistic isomorphism between what's out there and what's in the mind. *Rant #3*: being interested in certain areas of cognitive capability does not mean that those are all that matter.

Breadth of intelligence: Deary

My initial response to this was shamefacedly to think that I have not thought about this, and probably should have. I suppose that those who set up a theory and/or a definition of intelligence and then dance around it have the advantage over me on this one. Rather late in my career, therefore, I find that I am, (i) looking to see if I did, already, commit myself to delineating the breadth of the concept, albeit with no strategy in that regard, and (ii) making something up on the spot.

Let’s do (ii), first. It is tempting to be glib and say that anything that has a right or wrong, or a better or worse, answer and in which there are individual differences in the ability to give the correct or better answer might be considered to do with intelligence. (Yes, I know the brain does other stuff, and that that is important, but here we are concentrating on cognitive capability.) However, I am aware that it would not take long to think of an example that would provide a counter-example for that effort at defining. For example, the personality trait of openness to experience correlates about 0.3 or more with intelligence test scores and its items do not have right and wrong answers. I and co-author Stuart Ritchie came up—on the spot, again—with an attempt at defining intelligence’s breadth in a chapter for Robert Sternberg’s (2018) book on the *Nature of Intelligence*. “Intelligence is the massive accruing pyramid, the strongly knitted nomological network of facts [meaning well-replicated empirical regularities] relating to cognitive ability test scores: how they correlate, what they predict, and why they come about. It’s the quality that we recognise as showing differences between people when we (correctly) understand that people who tend to be good at one mental ability tend to be good at others also; these include remembering things, manipulating information, working out general principles from a set of examples and then applying them more broadly, thinking quickly, organising mental work, working things out in two or three dimensions, knowing word meanings, and knowing facts about the world.” I can see that that’s a bit dry, overall (and the opening is a bit purple, from this distance since writing it), and is a list-like coverage of the demesne and topics of psychometric approach to intelligence. However, I like the organic-sounding flexibility of the nomological network.

On thinking about (i), I find that I have declared some prior commitments to what intelligence includes. When I wrote *Looking Down on Human Intelligence* (Deary, 2000), I drew a diagram of the topic of intelligence that had psychometric concerns about mental tests in the centre, with their practical and real world correlates and implications above that, and their biological and social origins below that. Thus, I did see three broad areas of study regarding intelligence differences, i.e.: description--assessing those differences (including their multivariate structure); studying what they predict; and explanation—trying to discover how they developed. Another clue—actually, a kind of variant of the prior sentences—was the chapters in my Intelligence: *A Very Short Introduction* (Deary, 2020). In that book I organised the text into 10 questions (abbreviated here) about cognitive test scores: how many types of intelligence are there?; how does age affect intelligence?; are there sex differences in intelligence?; do genes and environment affect intelligence?; do intelligent people have bigger brains?; do intelligent people think faster?; is intelligence relevant in education and the workplace?; is intelligence relevant for health?; is intelligence rising with the generations?; can experts agree about intelligence? Again, that's focussed on: descriptive aspects of cognitive test scores, their predictive validity, and explaining their origins.

Is that a sufficiently larger and important allotment on which to spend one's career researching? Yes, I think. Does that all feel bereft of important other stuff? In a way, yes; for example, navigating the Polynesian seas, or the Arctic during a white-out, or the Australian interior, are amazing cognitive feats, carried out before modern technology, and in which I guess there were individual differences. And I wish I could get on with people as well as do those who are said to have high emotional intelligence (though I think that is more to do with my personality traits), and I wish I could play cricket like Ian Botham did, and I wish I could play soprano saxophone like Jess Gillham. There are many other such human skills; they are omissions that are not obviously part of what we regularly study in intelligence research. Oh, and wisdom and creativity are not covered, but I judge that they have yet to be operationalised or theorised-about in an adequate way. Those gaps could be corrected, by researchers more regularly looking beyond their own surroundings. There were other omissions, too, that were corrected. For example, it is within only the last 20-odd years that health, illness and survival were incorporated as part of intelligence tests' predictive validity. And, as I said above, it is still a surprise to me that a psychophysical test as simple as inspection time has individual differences that correlate moderately with intelligence test scores.

One should be broad-minded then, about the breadth of intelligence research. I don't think it is a bad strategy to start with cognitive tests scores' psychometric structure and their origins and predicted outcomes; and we should not cast these aside when we hear the siren song of some new intelligence 'theory'. But we should be open to other cognitive skills and see how they relate to existing psychometric cognitive tests. Making sure that we do look out for any such relations can help us avoid the jangle fallacy; there is a tendency for the purveyors of new cognitive concepts to assume rather than test that they are different from intelligence.

Sternberg's response

To me, the problem with Deary's approach is shown by his statement, "Intelligence is…the strongly knitted nomological network of facts relating to cognitive ability test scores: how they correlate, what they predict, and why they come about." Intelligence is not merely a "network of facts." Is memory, or evolution, or general relativity, merely a "network of facts"? No. The network of facts is what a theory should predict. Correlations are merely the outcomes of the scientific phenomena of interest--natural processes. Moreover, the network doesn't tell us "why they come about," because correlations such as these do not provide causal explanations. Theories are what make "networks of facts" meaningful and predictive rather than merely untestable bases of post hoc explanations. Intelligence is broad not because of a network of correlations, but rather because of the complexity of adaptive demands evolutionary and cultural processes have created on our cognitive capacities.

Question 4

What do you see as the hard problems confronting intelligence research?

Hard problem: Sternberg

I believe the single hardest problem confronting intelligence research—which dwarfs all the others—is entrenchment. The problem resides not out there in the world, but rather, in our minds as intelligence researchers. We are entrenched (Frensch & Sternberg, 1989; Sternberg, 1981). We need breakthrough thinking, not more of the same (Kuhn, 2012; Sternberg, 1999, 2018).

There are a number of different paradigms for studying intelligence (see review in Sternberg, 2020b)—psychometric, cognitive, biological—to name three. At one level, these approaches are distinct. At another level, they can be seen as a single super-paradigm based on Spearman's (1927) *g* or some variant of hierarchical models, such as Carroll's (1993). From this point of view, they are all roughly the same. All seek validation through scores on *g*-based psychometric tests.

The argument of the *g* theorists, of course, is that they are all basing their work on *g* because *g* is no longer even a theory; it is a fact. I actually agree with that, at least, psychometrically. But the question for me is not whether *g* and its subfactors exist in some form, but rather, whether they are well equated with *intelligence.*

Binet and Simon (1916), Wechsler (1940), and other creative early theorists of intelligence ("Intelligence and its measurement," 1921) recognized that intelligence is, beyond all, the ability to adapt to the environment. I have suggested that, in part as a result of a psychometric operationalization, we have lost track of the meaning of intelligence (Sternberg, 2020b).

COVID-19 has struck over 87 million and killed almost 2 million people as of the day I am writing this (January 7, 2021). By the time you read this, the numbers will be much higher. How many people has global climate change killed? Very soon, it is estimated that a quarter-million will die *per year* as a result of climate change (Rettner, 2019). That does not include discomfort, lost homes, lost incomes, injuries, and the like. These problems were largely, although probably not entirely, preventable. Where was *g* while these problems were in need of urgent solution and were not being solved? Or do we care more about predicting future test scores, grades, salaries, and awards than human lives saved and, indeed, the future of humanity? Our Zeitgeist of intense individualism has come at the expense of collective sanity. If we want to measure adaptive intelligence, we would better ask questions about how society could deal effectively with a pandemic than questions about what an isolated word means or how a number series problem is best completed.

I am not writing this answer to suggest that my own theory of adaptive intelligence is *the* answer. Of course, it isn't. I am susceptible to the same biases as everyone else. But in terms of *collective* adaptation, the definition of intelligence as *g* has been a serious bust, and so has intelligence research. The world saw a gain of 30 points over the course of the 20th century (Flynn, 1987). Meanwhile, the environment has been and is being degraded by the *g*-whizzes of the world; totalitarian governments have been spreading like wildfire all over the world, not through coups, but rather through elections; people's beliefs in senseless conspiracy theories are intact and probably growing; global climate change gets worse; a pandemic rages and people argue about whether they truly are responsible for others' deaths when they sicken those others so they can protect their "freedom" not to wear a mask; and air pollution is killing more than 4 ½ million people a year (https://www.sciencedaily.com/terms/air_pollution.htm).

Why do intelligence researchers continue to believe that *g* is largely equivalent to intelligence? If the researchers show a smug certainty, it is perhaps understandable because it is not clear how *g,* as largely equivalent to intelligence, ever could be disconfirmed. It is essentially…a matter of faith.

A problem is that validations of intelligence tests are all correlational, so of no real causal value. They also have been biased from the start, in a hidden way, because the criteria are biased, thereby hiding statistical bias. That is, it is possible to hide bias by choosing criteria that are biased in much the same way as the predictors, sometimes because the predictors directly lead to criterion bias. For example, IQ predicts various kinds of success in occupational attainment and accomplishment (Sackett et al., 2020). But proxies for IQ tests (SAT, ACT, GRE, LSAT, MCAT, GMAT, etc., in the United States) are used for admission and sometimes financial-aid decisions for educational programs that prepare students for occupational success. If a person does not test well, it will be harder, perhaps in some cases, impossible, to attain admission to the programs that allow one to enter prestigious programs for preparation for particular jobs, or even the jobs themselves. For example, it is very challenging to become an MD in the United States with a low or even marginal MCAT score. More prestigious business and law firms, in turn, tend more to recruit at schools that have higher mean test scores, in part because these institutions gain part of their prestige from the higher test scores. So the test scores actually can be partially causal of the opportunities students have later to succeed, creating, in part, the correlation that later is used to argue for the predictive validity of the tests.

The problem is that one's ability to succeed in the societally-chosen criteria used to validate the intelligence tests depend in part upon people's scores on those self-same tests. People higher in *g,* in part because of greater opportunities their socialization provides, succeed more in test-based societies because those societies use *g*-based tests to allocate opportunities, including the opportunity to be an intelligence researcher. If people were given comparable advantages as a function of height, taller people would be more successful and likely conclude that their height somehow made them superior to the short people who failed. Similarly, the children of very rich parents often believe themselves to be exceptionally talented, failing to see how their parents' wealth played an inordinate role in their success. All think their success is "earned."

High-*g* people well may indeed be better in maximizing individual societally valued outcomes, even beyond the extra opportunities they are given. But collectively, they have created a tragedy of the commons, where their individual successes have resulted in catastrophic collective failures, most of them long-term whose results are only beginning to be seen. Societies need to create more disincentives for behavior of high-IQ people who behave badly, but unfortunately, the incentives often go the opposite way, because today, as always, ethically questionable behavior can lead to gains, if ill-gotten ones. Some organizations have been willing, at times, to turn a blind eye to those who increase profit or prestige at the expense of ethics. The field of intelligence has been expert at seeing trees, but not at seeing the forest. The forest is burning, and they are looking, metaphorically, at which trees get more sun and water and thus grow taller in the smaller and smaller forest that remains.

Deary's response

Wow!; where does one begin? First, by listing other problems that we could blame on psychometric intelligence researchers, so that the critique can implode by *reductio ad absurdum*. Second, by calming down, and understanding that psychometric intelligence researchers are not entrenched: they are doing their bit, by finding out whether and why some people are better than others at all sorts of cognitive tests, and what those differences predict; at the same time, recognising other psychological and social variables that correlate with human problems and life outcomes; and remembering that cognitive capability is a trait and not a virtue, and that the decent high-ability person will contribute that skill to help mankind, as will the conscientious, as will the strong, and so forth; "from each according to his ability, to each according to his needs". As Richmal Crompton (1922) wrote in *More William*, "William was neither quiet nor gentle, nor courteous not intellectual—but William was intensely human". It might be correct, as Goodhart (2020) argued, that a good society will value cognitive skills only as much as it values other capabilities, but correcting any over-emphasis toward those with intelligence will come from re-emphasising those other skills, not decrying the intelligent. *Rant #4*: don't shout at the screwdriver for being unable to hammer in a nail.

Hard problem: Deary

In terms of public relations, I think that one hard problem is to get across all the empirical regularities known about intelligence test scores. I think that's a success without equal in psychology, and yet so many people, including psychologists, think so many of these well-established findings are disputed. I have tried to describe these successes, and also to point out the unknowns (e.g., Deary, 2000, 2001, 2012, 2013c, 2020: Deary, Whalley, & Starr, 2009; Deary, Penke, & Johnson, 2010; Deary, Cox, & Hill, 2021; Deary, Hill, & Gale, in press).

Explanation: that's ***The*** *Hard Problem* (yes, I know… for Tom Stoppard, it was consciousness; I think that's impossible just now, rather than hard). I acknowledge that stating "explanation" means that I have—unforgivably, for some, no doubt—accepted that individual differences in cognitive test scores are an interesting-enough phenomenon, and that one may proceed from there. I am prepared, then, to move along and start asking what they are related to, and why they occur. That's when I think we in intelligence research are cursed with success. There are so many intriguing associations that arise between intelligence test scores and other stuff that then meet the difficult problem(s) of explanation. I mean both explaining the correlation between intelligence and other things, and explaining intelligence test score differences.

An example of the hard problem of explaining a correlation is with the association between intelligence test scores from childhood and survival to old age (and to health behaviours, health, and illness in between). Don't misunderstand me; since the discovery of the childhood intelligence versus survival-to-old-age association (Whalley & Deary, 2001) we have discovered lots more (e.g., Deary, Weiss, & Batty, 2010; Deary, Hill, & Gale, in press); the types of illnesses that are involved, partial statistical mediation by social factors, and relevant genetic correlations (Calvin et al., 2017; Deary, Harris & Hill, 2019; Hill, Harris, & Deary, 2019). Each of these adds to the explanatory story, which is building, but we are far from a full or satisfactory account.

Two examples of the hard problem of explaining intelligence differences are the two—I would argue—best-supported biological associations with intelligence test scores: brain size and genetic variation. There is a replicable correlation between overall brain size and intelligence among health adults. It is just below 0.3, I think. The effect size solidified as structural magnetic resonance brain imaging studies became more numerous (Pietschnig, Penke, Wicherts, Zeiler, & Voracek, 2015), and with single large studies such as UK Biobank (Cox, Ritchie, Fawns-Ritchie, Tucker-Drob, & Deary, 2019). That's progress, but have a look at the Discussion sections of those papers; that's when the hand-waving begins—i.e., there are ideas, but few confirming empirical regularities to back them up. Is it neuronal number?, and/or myelination?, and/or several other lower-level brain parameters that drive the correlations? It is important to have the answers, and there will be more ideas in the future: the search for explanation continues.

We've known for ages that intelligence test score differences are substantially heritable. Thus, something about genetic variation relates to intelligence. Until 2011, those findings came from twin/adoption/family designs. That year, genome-wide testing of DNA from unrelated people was used to demonstrate partial heritability of intelligence, bypassing the assumptions in, say, twin studies (Davies, Tenesa, Payton, Yang, Harris, & Liewald et al., 2011). Then, by 2018, there began to be replicated single nucleotide polymorphisms in DNA that are related to intelligence differences (Davies et al., 2018; Hill, Marioni, Maghzian, Ritchie, Hagenaars, McIntosh et al. , 2019). With sample sizes of about 300,000, there were about 150 independent genetic loci associated with intelligence. For seekers after explaining intelligence differences, this can be a heart-sinking result. How can one compose an explanatory account from all those genetic loci? It gets worse. Not all of those genetic loci are located in genes. And, worse still, it is only a smallish part of the heritability detectable by testing DNA (which is less than that obtained from twin studies, although this is probably due to twin studies' ability to capture differences in genetic variation missed by GWASs' focus on common SNPs, such as rarer variants and copy number variation [Plomin & von Stumm, 2018; Hill, Arslan, Xia, Luciano, Amador, Navarro et al., 2018]) that is accounted for by those 150-or-so genetic loci. That is, those very small effects are the larger among, probably many thousands of minuscule genetic contributions; intelligence is massively polygenic. It's hard, as-yet, to see how an explanatory account can be assembled from those findings, though they are clearly relevant and demanding of an explanation. Appeals have been made, for example, to intermediate constructs such as gene systems, neuronal cell types, gene expression, and epigenetic modifications (especially methylation), but they have yet to close the explanatory gap appreciably.

I focussed on the above examples because those are the areas I know better, and recently reviewed (Deary, Cox & Hill, in press). A case could also be made for the hard problem of finding out what it is about the non-shared environment that contributes to intelligence; or, what accounts for the substantial stability of intelligence differences from childhood to older age (Deary, 2014), and also the substantial instability (Corley, Cox, & Deary, 2018). However, as death, brains, and genes demonstrate, when it comes to intelligence differences, it is easier to find associations than it is to explain them.

Sternberg's response

In his responses, Deary seems to have gone back to full operationism—defining phenomena in terms of the operations by which they are studied. For example, he says, after discussing .3 correlations, "That's progress, but have a look at the Discussion sections of those papers; that's when the hand-waving begins. Is it neuronal number, and/or myelination, and/or several other lower-level brain parameters that drive the correlations?" The *psychology,* I would argue, is in the explanation, not in the .3 correlations. There are probably thousands of cognitive tasks that correlate roughly .2 to .3 with each other and with IQ. The science is not merely in the mass of correlations, but in understanding the underlying psychological mechanisms that produce these correlations. This is not "hand-waving." It is science—trying to describe and understand natural phenomena through theoretical explanation, organization, and prediction of data.

Question 5

Is there a place for intelligence testing in everyday life?

Intelligence testing in everyday life: Sternberg

Intelligence tests have been found to predict a wide array of kinds of behavior. Other works have written about this (e.g., Deary, Whalley, & Starr, 2009; Sackett, Shewach, & Dahlke, 2020; Sternberg & Grigorenko, 2002) so I don't need to write much here. Intelligence tests not only are predictive but also diagnostic (Kaufman, Raiford, & Coalson, 2016), something I learned even way back as an undergraduate (Alison, Blatt, & Zimet, 1968).

The problem with intelligence tests actually is not whether they are useful, but rather that their usefulness has led some researchers and practitioners to be too satisfied with them. No test, of course, is complete in what it tells us. Yet some researchers have spent much of their careers showing one thing after another that *g* predicts and smugly refuting those who claim that *g* is not predictive. There is always one more publication showing *g* predicts yet something else for some other population in some other situation.

A way of redefining the problem might be to say that we have known since the early to mid-20th century that *g* is predictive of many things and that now it is time to move on. We don't need to build careers showing the same thing over and over and over again, proving our incapability of becoming bored. If something has an intellectual element, we pretty much can just assume *g* will correlate with it.

The medical tests of the early 20th century, like the intelligence tests of that era, also were successful at what they did. But their success did not stop medical researchers later from creatively exploring what at the time were unimaginable ways of testing for various illnesses. Many cancers that once were virtually always detected too late to stop their spread now can be detected and treated early. Why did intelligence testing get stuck, whereas medical testing did not? Is it perhaps embarrassing that the intelligence tests of today do not differ much from those of the early 20th century, except for their delivery vehicle (e.g., computers) or their mode of presentation (e.g., adaptive testing)? Are we, as a field, that uncreative, do we believe that somehow, we, as a scientific field, uniquely have found TRUTH and so need look no further?

Intelligence tests predict between 10% and 30% of the variance in a variety of criteria. So that leaves 70% to 90% of the variation unpredicted. One solution, I believe a cop-out, is to define any other predictive factor as "not-intelligence." That is a little too convenient. This solution implicitly draws on Boring's (1923) operational definition of intelligence as being whatever intelligence tests. But that is not the worst of it.

That solution relies on putting into factor analyses as input whatever early intelligence researchers decided, without any serious prior theory, constituted what intelligence tests should test. These researchers did have theories, but they were not a priori theories, but rather theories *derived from* their factor analyses. One cannot get out of a factor analysis anything more than a transformation of what one put in. Even Carroll's (1993) theory and derivatives of it (e.g., McGrew, 2005) are based on reanalyses of existing tests and data sets. For almost two centuries, operationally defining intelligence has been a closed system with each new conception just a reworking of old conceptions. Arguably, that is not a history to be proud of. It is more a long-term stasis than a dynamic history. This conception has not done justice either to science or to society.

Another option would be, instead of starting with intelligence tests, to start with testing operations that are based on biological, cognitive, or cultural theories. Some have done this, such as those who have used Luria's (1966) theory as a basis for intelligence testing. In our own research, we have used my various theories of intelligence as a basis for testing (e.g., Sternberg, 1997, 2020a, 2021). Others have used Gardner's (2011) theory. But testing companies, having large markets, have been reluctant seriously to try anything really new that is not a variant on the old.

Yes, Gardner's theory is flawed (Visser, Ashton, & Vernon, 2006). But that misses the point that all theories are imperfect, even Einstein's theory of general relativity, which cannot be squared with quantum theory. At least Gardner had the creativity, the intellectual curiosity, and the guts to try to redirect intelligence theory (Sternberg, 1999) rather than just to create yet another look-alike test that is a cosmetic variation on existing tests. He had the courage to do something new and even to set criteria for an "intelligence," rather than using as criteria something that has been done before. That is almost a sin in the intelligence-testing business.

To the extent that one's interest is in measuring general intelligence, it does not matter much what intelligence test you use (Johnson, Bouchard, Krueger, & McGue, 2004; Johnson, te Nijenhuis, & Bouchard, 2008), because they all measure more or less the same general factor. They also measure other less general abilities that can be predictive of various kinds of success (Wai, Lubinski, & Benbow, 2009). Perhaps we can move beyond that? Perhaps we can explore new ideas instead of variants on old ones?

The intelligence-testing business is locked in something of a time warp. Unlike medical testing or industrial testing, it got stuck in the early to mid-20th century. The tests are useful. But no theory, no test, is perfect, and cosmetic variations of the same thing, over and over and over again, are not going to change much, if anything. We are now approaching the middle of the 21st century. It's time for new ideas rather than cosmetic reworkings of old ones.

We have tried in much of our work to create what we hoped are better tests of intelligence, broadly defined to include creative, practical, and wisdom-based skills as well as memory and analytical ones (much of which is described in Sternberg, 2004, 2010, 2021). I have no objection to conventional tests, so long as they are viewed as part of a larger picture of human abilities. The problem, pretty much from the start of the intelligence-testing movement, is that they have been scientific realizations of the figure of speech called synecdoche, where a part is taken to be the whole (e.g., "the crown" in referring to the king). There is more to intelligence than what the current tests test, and after so many years of testing the same things and validity coefficients having remained pretty much where they were early on, it is time to move forward and experiment with broader tests.

Investigators of intelligence will have different ideas about what these ideas should look like. But a notion of intelligence based exclusively on *individual differences*—on differential psychology--has been a horrible mistake. Intelligence has become, in our minds, about what enables some people to acquire more resources—better school grades, better university admissions, more awards, more prestigious publications, more money, a bigger house—than others. Meanwhile, we raise our hands in despair as a pandemic, global climate change, increasing income disparities, pollution, nuclear weapons, and the like, steady worsen our lives and prospects for the future. Our notion of intelligence is, forgive me, stupid. We hasten the day of the demise of our species, congratulating ourselves on our brilliance. If we do not start thinking of intelligence as adaptive—as needing to achieve collective goods that make the world a better place for all through contributions that are positive, meaningful, and enduring—then we are lost and will be like Shelley's Ozymandias, who said:

"'My name is Ozymandias, King of Kings;
Look on my Works, ye Mighty, and despair!'
Nothing beside remains. Round the decay
Of that colossal Wreck, boundless and bare
The lone and level sands stretch far away."

Deary's response

Poor psychometric intelligence researchers: in Q#4 they were "smug", and in Q#5 they refute "smugly"; cads! I spotted, from Sternberg, recognition that the test scores have valuable correlates; good-oh. It is not a problem *per se* that some tests' contents have not been renewed. One does not discard useful measuring tools (they sink those Galtonian [1890] "shafts" in a few important places of the mind); doctors use thermometers, measure haemoglobin etc. using many tests that have been around for a while: still-being-used can mean still-useful, and valid. The strongly-urged moving-on is happening, in the work explaining cognitive differences, and finding new areas of prediction (and predictive limitations, where other variables can add or do better). Cognitive test scores are interesting human phenotypes—not theories—for which we should seek the structure, causes. and consequences. Some do that with lung function, i.e. use venerable tests (e.g. the FEV1 is many decades-old) that are still useful, and then explore the test results' consequences and causes (Young, Hopkins & Eaton, 2007; Shrine, Guyatt, Erzurumluoglu, Jackson, Hobbs, Melbourne et al., 2019); and critics don't squawk about outdated theory, nor complain that studying the lungs ignores the heart (and the liver, and the kidneys…). *Rant #5*: intelligence is not all that matters—we know!—and psychometric intelligence researchers would be justified in being irritable with people who think they/we do not appreciate that.

Intelligence testing in everyday life: Deary

The brain is not an easy organ to get at. There are times when it is handy to know how well it is working. Most will recognise the Linda Gottfredson (1997) formulation, which goes something like, 'life is an intelligence test'. We can be more specific and consider some practical situations. It could be useful to know whether a child's mind will cope with, or be under-stimulated with, the material and pacing of a vanilla education. It could be useful to know if an illness or a drug or a medical procedure makes people think less well, or better. It could be useful to know if an older person's mind is affected by an illness, or just by becoming very old, and perhaps lessening their ability to live independently. What would one measure?; where would one begin with such a complex organ whose functions are many and varied?

The answer is surprising. Without knowing about the history of mental testing, the sensible answer would be: 'cognitive neuropsychologists have delineated and enumerated many components of many mental processes from their elaborate cognitive theories and have devised tests for each of them so that a bespoke compendium of cognitive tests may be used in any developmental/educational/medical/gerontological situation'. Well, that's not the answer.

The surprise is that surprisingly simple and non-specific tests have been surprisingly handy and surprisingly persistent in these sorts of practical, everyday situations. Items from the original intelligence test devised by Binet can still be seen in recent versions of the Stanford-Binet test and Wechsler tests (Gibbons & Warne, 2019). It's utility that has kept them there, and not inertia, or obtuseness, or ill will. Yes, the Binet tests were over- and mis-applied (Zenderland, 1998), but that does not mean they did their originally-intended job badly, viz. to help in the assessment of the ability to cope with normal schooling. Most will know that the Binet test that Goddard brought back to the USA and spread quickly was on a sheet of paper. That's also just about the case for another mental-test phenomenon, the Mini-Mental State Examination (Folstein, Folstein, & McHugh, 1975), which is used as an indicator of cognitive pathology in older people. It is very short, it appears to be too crude, it has a large ceiling effect in many samples of older people, and yet it has proved very handy and widely used. And, like the Binet test, one can see its items embedded in newer, often more elaborate, tests that are applied in the same sorts of situations.

I think Spearman was right about Binet. That is, whereas he appeared to ignore Spearman's findings regarding *g*, Binet's tests were such a hodgepodge of different mental obstacles that he could hardly avoid assessing it (*g*) more or less well. The same applies to the MMSE, which has a lucky-bag of little mental tasks. Godfrey Thomson also disagreed with Spearman; he did not think *g* was the only solution to the manifold of positive correlations among mental tests (Thomson, 1916), he did not like using a single omnibus test to assess mental ability, and he often urged that cognitive assessment should be a movie and not a snapshot (Deary, 2013a). Yet, when he, over decades, devised mental tests to assess millions of British children at age 11+ for secondary education (age 12 to 17/18), he used an omnibus test with a single score: the Moray House Test series (Deary, Whalley, & Starr, 2009). (That 45-minute paper-and-pencil test devised almost 100 years ago also predicts which of those children will live to their 8th decade of life [Calvin, Batty, Der, Brett, Pattie, Cukic et al., 2017). Note that the situation Thomson was in is not the same as nowadays; that is, only a small minority of children at that time (the 1930s) went on to 5 or 6 years of secondary education, and the places were considered precious, and educational authorities used intelligence and other tests to select children for those few places. Of course, everyone in Scotland now has a publicly-funded free place in school to about age 18. Yet, there are still cognitive tests given, in their hundreds of thousands in the UK, at the end of primary school. They are called Cognitive Ability Tests, and can give verbal, non-verbal, and quantitative ability domain scores; nevertheless, these domain scores correlate strongly and have a massive *g* factor that has most of the predictive power for educational outcomes five years later (Deary, Strand, Smith, & Fernandes, 2007).

So far, we have been asking, ‘can we measure how well this brain works’? We might also ask—especially when people are ill or just old—‘how well did this brain used to work’? One test that still blows my mind with brevity and handiness is the National Adult Reading Test

(NART; Nelson & Willison, 1991). It takes a few minutes to administer and involves the unhurried attempts to pronounce 50 words that have irregular grapheme-phoneme relations. The results of this test can, in the ageing or even demented mind, reach back several decades and have a good stab at estimating how bright this person was in their premorbid days, perhaps even at their peak (McGurn, Starr, Topfler, Pattie, Whiteman, Lemmon et al., 2004). Note that the NART provides only an *estimate* of prior/premorbid cognitive functioning; therefore, the mutation of the same testing idea into the Test Of Premorbid Function resulted in a test name that is an oxymoron. During delirium, another complex mental state, another short and crude-seeming test—the 4AT—has proven to be useful in that practical, pressing situation (Tieges, MacLullich, Anand, Brookes, Cassarino, O'Connor et al., 2020).

This duo of 'how well does this brain work' and 'how well did this brain used to work' is the basis of the Raven's Matrices-Mill Hill Vocabulary pairing, and the unfairly-underused Alice Heim tests.

When you are shown a battery of complex tests in a practical situation and are given a complex story about each of their differences and age-related changes, ask this: how much of the cross-sectional associations between the tests are due to general cognitive ability (and cognitive domains)? And, if the age-related (or, say, illness- or medication-related) changes are described individually, ask: how much of the changes are due to changes in general cognitive ability (and cognitive domains)? In both instances, the answer will probably be: a lot (Tucker-Drob, Brandmaier, & Lindenberger, 2019). This is a simple and really important, but much under-appreciated and ignored point: all cognitive tests—

including those with a name that implies they are assessing a specific cognitive capability or domain—assess *g* to some extent, sometimes to a large extent. This is not to say that sometimes one will not want more specific tests; it is just to say: (1) beware the label on the cognitive test's tin; and (2) short, general cognitive assessments have proven handy.

Yes, yes, yes: one might sometimes need specific and time-consuming mental tests for a given person in a given setting. No, no, no: this short piece is not an over-conservative defence of old and bad cognitive tests. It is an urging not to be too hasty in reinventing the wheel, and being too sniffy about useful, though crude-looking, tests. You will frequently come across psychologists who will try to sell you a 100-piece toolkit* whereas sometimes you need just a hammer and a screwdriver, or just a hammer. (*Yes, yes, yes, sometimes you will need items from it.)

Sternberg's response

It would be hard to find a more brilliant empiricist than Ian Deary. But I think operationism--pure empiricism—ultimately proves to be a dead-end. *No* natural science has successfully followed Deary's model of pure operationism. Psychology largely abandoned it in the early 20th century. Why? Because advances occur through deep theoretical explanation and prediction of data, not merely by collecting more and more data in the absence of an explanatory framework.

In the era of COVID-19, medical scientists of various types raced to find a vaccine. They rapidly produced a new kind of vaccine, an m-RNA vaccine. How? Through deep theoretical understanding of how viruses work. If they had just relied on thousands of studies of conventional vaccines, as intelligence researchers do with conventional intelligence tests, they might have come up with a vaccine, but much more slowly and probably with much less effect.

Discussion

We hope readers find this interesting: Deary in his psychometric/epidemiological *wirkin claes*, and Sternberg in his theoretical *kenspeckle* motley. The email exchange between questions etc. was good-natured and fun, as were some of the omitted bits. For example, Sternberg stated, in a not-included section, "I had a sentence in there originally that I took out, actually because of word length you insisted on. The sentence was to the effect that if I were to be anyone else in the field, I would wish it to be you! We disagree rather strongly on some issues, but you are really utterly the best at what you do. Just not what I do! And there are not 'right' answers about what to do." Deary omitted, also because of length, "I read Sternberg's 1977 paper on the components of analogical reasoning in 1979, when I was a third-year undergraduate year at university. I was impressed by the method's ingenuity and the importance of the results. When I became lecturer in psychology (in 1985), I taught this approach in my advanced intelligence course. Scales fell from my eyes, however…"

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Table 1

Sixteen Key Differences between General Intelligence (Standardized Test) Problems and Adaptive Intelligence Problems

Difference	Issue	General Intelligence	Adaptive Intelligence
1	*Type of Answer Required*	Right vs. Wrong (sometimes with partial credit)	More Adaptive vs. Less Adaptive to the Environment
2	*Structure of Problem*	Well-structured: Clear, Well Defined Path to Unique Solution	Ill-structured: Multiple Ill-Defined Paths to Solutions that Are Differentially Adaptive
3	*Emotional Arousal*	Low Emotional Arousal, Encouraging Clear Thinking	High Emotional Arousal, Discouraging Clouded Thinking
4	*Stakes for Adaptation in Life*	Usually Low; thus, Low Stakes If a Solution Is Wrong	Often High; thus High Stakes if a Solution Is Wrong

5	*Contextualization with Regard to Everyday Life*	Largely Decontextualized Problems Weakly Related or Unrelated to Everyday Life Events	Highly Contextualized Problems Often Strongly Related to Everyday Life Events
6	*Need for Recognition of the Existence of the Problem*	None: Problems are Given by Standardized Test	Great: One Has to Figure Out for Oneself that the Problem Even Exists
7	*Need for Definition of the Problem, Once Recognized*	Low: Problems are Usually Partially or Completely Defined by Test	High: Problems are Poorly Defined or Not Seriously Defined at All
8	*Time Allowed for Solution*	Low: Problems Generally Must Be Solved in a Few Seconds to, at Most, a Few Minutes	High: Problems Are Addressed Over Time and Often Unfold Over Time rather than All at Once
9	*Need to Search for Information*	Low: Much or Most Information Needed for Solution Is	High: Information Needed for Problem Solution Has to Be

		Presented in the Test Problem	Located in Available Reference Material
10	*Need to Evaluate Information for Relevance and Validity*	Information Given in Test Problem is Generally Viewed as Relevant and Valid	Information Sources Are Often Low in Relevance and Validity; Often They Are Mutually Contradictory
11	Role of Individual vs. Collective	Individual	Both Individual and Necessarily Collective
12	Motivation for Solution	Motivation (for Some) is to Receive a High Score on a Test	Motivation (for All) is to Resolve an Important Life Problem
13	Structural Complexity of Problems	Problems Tend to Be Structurally Rather Simple	Problems Tend to Be Structurally Quite Complex
14	Number of Steps to Solution	Problems Tend to Have Relatively Few Steps to Solution	Problems Tend to Have Many Steps to Solution
15	Intrinsic Interest of Problems	Tend to Be Relatively Boring	Tend to Be Relatively Engaging

16	Knowledge Needed to Solve Problems	Tends to Be Formal Knowledge of the Type Learned Inside of School	Tends to Be Informal Knowledge of the Type Learned Outside of School