Evolutionary Behavioral Sciences

2330-2925/19/$12.00
http://dx.doi.org/10.1037/ebs0000181

BOOK REVIEW

Solid Numbers, Missed Opportunities: Review of *The Intelligence of Nations*, by Richard Lynn and David Becker, London, United Kingdom: Ulster Institute for Social Research, 2019, 436 pp., £5.00, ISBN: 9780993000164

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The Intelligence of Nations is the latest iteration of Richard Lynn's long-running compilation of data on the intelligence of countries. The book has four chapters, the first of which briefly (6 pages) introduces the reader to the question of why some countries are richer than others (Smith, 1776). This chapter serves to summarize the three previous books by Lynn and Vanhanen on the same topic (Lynn & Vanhanen, 2002, 2006, 2012). In his summary, however, Lynn neglects to mention his own 1978 book chapter (Lynn, 1978) in which he first summarized test results for ethnic groups and countries. In that paper, Lynn did not produce a table of estimates, so I have constructed one based on his reported studies; the corresponding values from the present book are reported alongside in Table 1 for comparison.

The 1978 estimates were often from unrepresentative elite samples, and there were no adjustments made for the Flynn effect, which was not brought to significant attention until 6 years later by James Flynn (Flynn, 1984). Still, the correlation between the 1978 and 2019 values is $r = .81$ ($n = 23$). This is quite strong, especially given that the early estimates were based on approximately 30 studies, whereas the latest data set is based on 667 studies (version 1.3.1, the one found in the book).

Considering the stability of results over time, the reader might wonder: What's new in this book, as compared with the previous works? First and most foremost, Tatu Vanhanen passed away in 2015, so Lynn recruited the much younger (born 1983) German intelligence researcher David Becker to assist him. The previous editions were sometimes criticized for opaque methods and faulty calculations (e.g., Barnett & Williams, 2004). To address these criticisms and to remove errors in the calculations, the authors set out to rerun all calculations for the present book. They also set out to bring their methodology in line with emerging principles for open science, including open data, methods, and code (Hesse, 2018; Lindsay, 2017; Paxton & Tullett, 2019). The authors therefore sought to obtain a copy of every paper that had been used. However, many of these studies have not yet been obtained (514 accounted for, 70%, with 216 still missing) and so were excluded from the present edition of the book. This is not so surprising, given that many of the original articles were published, over the last century, in extremely obscure, sometimes non-English outlets that no longer exist.

The main take-away from the new set of results is that the values estimated in earlier works hold up well under increased methodological rigor and recalculation by a new researcher (Becker did the calculations, Lynn provided copies of the sources). It is worth saying something about the new

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The author has an unpaid affiliation with the institute of the first author of the book (Richard Lynn). The authors of the book had nothing to do with the writing of the book review, which was also not solicited by them.
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Table 1
Estimates of National IQs of Select Countries, 1978 and 2019

Country	1978 IQ	1978 notes	2019 IQ
United Kingdom	100		99.12
United States	100	European descent	97.43
New Zealand	98.5	European descent	98.57
Australia	95		99.24
Belgium	104		97.49
France	104		96.69
Germany	100	East Germany	100.74
Denmark	100		97.83
Italy	100	Florence	94.23
Spain	87		93.9
Croatia	89	Zagreb	96.19
Greece	89	Thessaloniki	93.56
Iraq	80		89.28
Iran	82		80.01
India	86		76.24
Uganda	84	Elite samples	76.42
Jamaica	79		75.08
Tanzania	88	Elite samples	74.95
Ghana	75		58.16
South Africa	78	African descent	68.87
Taiwan	100		106.47
Japan	100		106.48
Indonesia	96	Bandung, elite	78.49

Note. The 1978 values are based on Lynn (1978), using medians to combine studies when needed. The 2019 values are based on Lynn and Becker's book.

methods used, which is the topic of the second chapter. The authors illustrate their methodology by inviting the reader to consider data for two fictional countries. They provide various characteristics about the fictive studies, tests used, test administration year, test standardization year, representativeness, sample size, age span, and so on. Then they introduce the reader to the various ways of adjusting for distortive effects (including the Flynn effect, which differs by both test and region), and the various ways of weighting the results by study quality. The process ends up being somewhat similar to the one used for Cochrane systematic reviews, although the authors do not seem to take inspiration from any guidelines for meta-analyses (e.g., Preferred Reporting Items for Systematic Reviews and Meta-Analyses; Swartz, 2011). Next, the authors discuss methods for computing national intelligence quotients (IQs) from international scholastic achievement/assessment tests (PISA, TIMSS, etc.), as was also done by Heiner Rindermann earlier (Rindermann, 2007, 2018). After the process is explained, tables are presented with the new IQ estimates. However, it is unlikely that anyone will be using these tables specifically because, as the authors explain, the national IQs are now more frequently updated and are available on David Becker's website for download in machine-friendly format (http://viewoniq.org/). The current version is 1.3.2, which contains several small error fixes compared with the values in the book and adds a few new studies. This approach brings the database of national IQs up to par with other widely used data sets, such as the Maddison Project (https://www.rug.nl/ggdc/historicaldevelopment/maddison/), which provides national economic measures, and Clio-Infra (https://clio-infra.eu/), which provides historical data of interest to economic historians.

Of particular interest to the study of group differences in intelligence are the comparisons with the previous data sets because so much research has been conducted on these. It has been frequently claimed that Lynn's national IQs were biased in favor of European nations and against African ones (Rindermann, 2013a; Wicherts, Dolan, & van der Maas, 2010). Such bias does not need to result from deliberate actions but can occur as part of routine scientific work, which involves making

make judgment calls in which preconceived notions might have an effect. In fact, the new IQs correlate at $r = .85$ with Lynn & Vanhanen (2012) shown in Figure 1. Because there is a huge overlap in sources, one might wonder why this value is not higher. The reason seems to lie mostly with the use of expanded norm ranges. Lynn did not previously rely on extrapolated norm ranges to convert raw scores lying outside the range of standardization samples, whereas the new calculations do make use of these. As a consequence, samples that would have previously been given the minimum normed value (usually 60 or 65) are now given very low values, even into the 40s. These extremely low values, of course, raise serious questions about the measurement invariance of the tests (i.e., do they measure the same thing in Germany as they do in Chad?), but it should be noted that some such values can be expected on sampling error grounds alone. Because these are found only in developing countries, especially African and Central American (e.g., Guatemala's national IQ is estimated to be 48), the use of the new more consistent methodology actually means that Lynn had been overestimating some non-European national IQs. In avoiding the use of outside-norm values, Lynn effectively winsorized the low scoring samples, giving them the benefit of the doubt. In dealing with these low scores, Lynn and Becker (2019) recommend that authors apply a winsorization at 60 IQ in the book (p. 201). The discussion of possible human bias in the prior calculations is surprisingly not given much attention in the book at all, but Becker elaborated on it in a recent conference talk. He analyzed relationships between changes in IQ estimates from the prior 2012 manual calculations to the present mostly automated ones and on relations to criterion variables such as ancestry/ethnicity and well-being. If such relationships are found it can be taken to indicate a bias whereby the researcher pushes the calculation toward value that better fit with other data. In Bayesian terms, this would actually be the right thing to do (i.e., taking the prior into account), but it does open one to criticism if other researchers have different priors (which obviously they often do). Using the published data set, I calculated the correlations

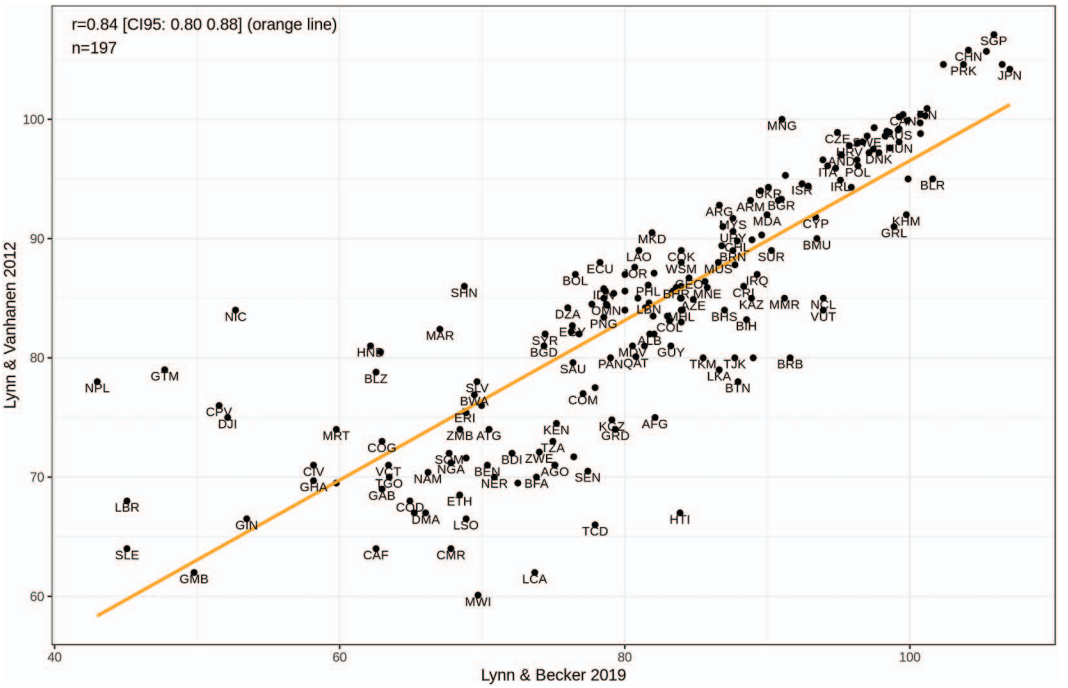

Figure 1. 1978 values are based on (Lynn, 1978), using medians to combine studies when needed. 2019 values are based on Lynn and Becker's book. See the online article for the color version of this figure.

between European%, African%, HDI 2013 and the change in scores between estimates. I estimated the ancestry fractions using Putterman's migration matrix. There were no relationships to ancestry: r European% = .11 (p = .17), r African% = −.03 (p = .74). There was a weak relationship to HDI, r = .16 (p = .03), meaning that lower HDI countries received slightly worse scores in the new estimates, opposite of the expected direction of bias. Thus, the criticism of bias against African nations was contradicted. Furthermore, as can be seen in Figure 1, there is a marked heteroscedasticity in the regression, that is, the variance of changes to the scores is much higher for the countries lower in intelligence and well-being. This, of course, reflects the generally poorer data quality in these countries.

In reviews of the previous books in the series, much discussion centered on the use of imputed country scores based on similar neighboring countries (Foster & Frijters, 2013; Gale, 2013; Strate, 2013). In the new book, the same protocol is applied but this time using the border lengths as weights whenever possible (p. 43ff). Actually, data imputation is not unusual in the least and in fact should be the norm because the data are not missing at random; indeed, missing values are concentrated in poorly developed countries. If one used the national IQs without imputing the missing values, one would get biased results from the missing observations (restriction of range bias, in particular). Countries, of course, have spatial positions, and this allows the use of spatial statistics (Gimond, 2019). Results from such analyses show that there is a very high degree of (positive) spatial autocorrelation in the data, which in plain language means that country IQs are highly predictable from neighboring countries (Gelade, 2008; Hassall & Sherratt, 2011). This obviously also means that one can impute missing data with high accuracy, justifying Lynn and Becker's method. Spatial autocorrelation is just one type of autocorrelation. Autocorrelation features are widely used to impute data in other fields, for instance, in medicine in which within-person autocorrelation in time is used to fill in missing data when observations in longitudinal studies are missing (Bell, Fiero, Horton, & Hsu, 2014). Furthermore, if one used a multivariate imputation method, relationships to other variables, such as health, ethnicity, and wealth, would also be used to fill in missing values.

The relative stability of the national IQ estimates across decades of data compilation is worth remarking on in more detail. Since about 2012, social science and biomedical sciences have been plagued by open discussions about the lack of replication and general unreliability of findings, called the replication crisis (Shrout & Rodgers, 2018). It is generally agreed upon that one of the major causes of the poor reliability of findings is that studies are too small and underpowered for their purposes. In contrast, as has been remarked by Steven Pinker, there is little to no replication crisis in (noncandidate gene) behavioral genetics, and IQ research generally (Pinker, 2015; Plomin, DeFries, Knopik, & Neiderhiser, 2016). Reviews of statistical (median observed) power by field show why this is the case. Nuijten, van Assen, Augusteijn, Crompvoets, and Wicherts (2018) reviewed intelligence research for statistical power and found an overall power of 53%, which compares favorably with other social science fields (e.g., neuroscience at 21%, Button et al., 2013; economics, 18%, Ioannidis, Stanley, & Doucouliagos, 2017). Furthermore, they looked at subfields of intelligence research and found that group differences had a median power of 62%, the highest reported of any social science field, and closing in on the minimal requirement of 80% suggested by Cohen decades ago. The philosopher Neven Sesardic suggested an explanation for why this is so, namely that because intelligence research and group difference research in particular is disliked so much by generally left-wing academics (Duarte et al., 2015), the standards of evidence in peer review have been increased (Sesardić, 2005, section 6.4). Whereas this results in some suppression of published works, it also has the effect of increasing the average rigor of the published research. As a case in point, the median sample size of studies in psychology is somewhere between 40 and 120, depending on subfield (Marszalek, Barber, Kohlhart, & Holmes, 2011), whereas the median sample size in the current national IQ data set was 353.

The third chapter of the book (116 pages) relates to the correlations between national IQs and various other variables. This chapter is structured like the previous books: Each section summarizes findings from studies using the national IQs. These sections do not represent

systematic reviews of studies published but seem to be the authors' chosen examples. The values from the reviewed studies (i.e., the correlations) are also given in tables. The presentation here is very dry. As other reviewers have noted, this was the case also for the previous books: X study reported a correlation of A, Y study reported a correlation of B, and so on. There is little to no attempt at describing results from more causally informative studies, such as time-lagged regressions, path models, or various econometric designs. As such, it invites the skeptical reader to think that the authors are simply assuming causation at the aggregate level from the correlations (Barnett & Williams, 2004). The authors probably regard these as probable inferences based on the strong evidence from individual level studies (Herrnstein & Murray, 1994; Strenze, 2015; Trzaskowski et al., 2014), but they make no serious attempt at convincing a skeptical reader, which is a pity. (For examples of studies that attempt to decompose cause and effect, see, e.g., (Christainsen, 2013; Jones, 2016; Jones & Potrafke, 2014; Jones & Schneider, 2010; Rindermann, 2018; Wong, 2007.)

A problem raised by the authors, but not adequately discussed, concerns the matter of measurement bias. The omission is odd because the unestablished measurement invariance of national IQs has been a frequent point of criticism (e.g., Wicherts & Wilhelm, 2007). There are in fact a small number of studies that have examined measurement invariance in cognitive ability more broadly on the national level. The first question to be asked, perhaps, is whether a general factor of intelligence exists at all in the data from poor non-Western countries. It is conceivable that this factor could vary by level of development and perhaps be smaller or absent in poor countries (although this would be in contradiction of the so-called Spearman's law of diminishing returns [Blum & Holling, 2017; see also Coyle & Rindermann, 2013]). The question was answered with a large analysis by Warne and Burningham (2018). These authors found that almost every data set analyzed from non-Western countries showed a g-factor similar to that seen in Western data sets. Furthermore, one might ask whether methods for detecting measurement invariance find that the tests function similarly across different countries. Research using the international TIMSS mathematics test data by (Wu, Li, & Zumbo, 2007) analyzed data from Western developed countries as well as northeast Asia (Japan, South Korea, and Hong Kong). They used state-of-the-art multigroup confirmatory factor analysis and found that scores were comparable only within the broad cultures, not across cultures. That is, one could compare scores for, for example, the United States and Australia, as well as Japan and South Korea but not, for example, for the United States and Japan. Other research has also found that measurement invariance held for comparisons between the United States and Canada (Bowden, Saklofske, & Weiss, 2011). The lack of measurement invariance is concerning and means that one cannot simply interpret the score difference between nations as being of the same nature as that between individuals with nations. The matter clearly calls for further investigation, which can be done using the publicly available data in scholastic ability data sets (PISA, etc.), as well as the various translations of IQ batteries such as the Wechsler batteries.

The fourth and final chapter discusses the future of national IQs. In fact, the chapter is about how to increase national IQs and takes for granted that these are to some extent causal for the many associations summarized in the third chapter. The authors discuss five ways: three environmental and two genetic. Nutrition is advocated as an important cause, and the authors cite a few studies of breastfeeding and vitamin/mineral supplementation. However, although one can find such studies, there are other equally or better studies showing no effect. Moreover, the authors do not cite recent studies examining breastfeeding's effect on IQ using a sibling control design (which controls for genetic confounding; Der, Batty, & Deary, 2006) or a study with a very thorough set of parental controls (Girard, Doyle, & Tremblay, 2017), both of which show negative effects.

A particularly good study is the randomized controlled trial of about 800 children in Nepal whose mothers received or did not receive multivitamins while pregnant and that followed up the children until age 12 years when they were tested for IQ (Dulal et al., 2018). There was only a 1 IQ advantage ($p = .18$) for the intervention group despite the well-designed

intervention and the large sample size. Thus, changing nutrition to increase intelligence in the way the authors propose is probably not as easy as their discussion implies. The second environmental factor the authors suggest is improving health. Whereas this may be done on general well-being grounds, does it really improve intelligence? The authors cite two studies of infectious diseases (Hadidjaja et al., 1998; Jardim-Botelho et al., 2008). Unfortunately, neither are persuasive. The first is a cross-sectional study without rigorous controls, which is expected to have genetic confounding. The second is a randomized controlled trial. However, the analysis and reporting is suboptimal, and it is difficult to work out what the effect size is; the posttest score means reported for traits of interest are quite comparable between intervention and control groups. The authors had six measures of intelligence, but only three of them showed an effect (in ANOVA) despite a comparatively large sample size of 483. Although it is very likely that intelligence levels could be increased by improving health, these particular studies do not provide strong evidence for action. For a recent meta-analysis of interventions with somewhat optimistic conclusions, see Protzko (2017). The third proposed method is improved education. Of course, the relationship between education and intelligence is complicated and still unsettled. A recent meta-analysis of studies showed that the method used to estimate the causal effect of education on intelligence has a large effect on the estimated outcome. Specifically, pre-post control studies found quite small effects, whereas natural experiments based on policy changes found, probably unrealistically, large effect sizes (Ritchie & Tucker-Drob, 2018). The matter is also complicated by the fact that IQ gains associated with education duration do not appear to be on the general intelligence factor (g) but rather on the non-g factors (Ritchie, Bates, & Deary, 2015). Considering the evidence that it is mainly the g-factor that adds predictive validity to IQ test scores (Jensen, 1998), it is not clear what improving non-g factor scores would accomplish. The matter requires more psychometrically sophisticated research to clarify.

The authors also discuss genetic means of improving national intelligence. First, the authors review the history of research into dysgenics, chiefly in studies reporting negative relationships between IQ measures and fertility measures. Second, they review economic policies attempting to encourage smart people to have more children, especially smart women. They cite some old reviews of policies on maternity leave, and tax/cash benefits in Western countries, finding small positive effects on fertility. Given that Western countries vary widely in fertility levels, from near replacement (about 1.8) in Nordic countries and the United Kingdom to quite low (about 1.4) in neighboring Germany and southern Europe, it seems likely that one can indeed influence fertility levels by interventions. Unfortunately, there seems to be a lack of randomized controlled trials on the topic, so researchers are left with suboptimal research designs. Alternatively, instead of attempting to get smart people to have more children, one could attempt to get less bright people have fewer children. The most obvious way to do this, the authors note, is to scale back welfare policies that enable the practice of single motherhood. The authors consider such changes to be doubtful in Western countries because of popular resistance. Finally, the authors discuss the role of immigration in national IQs. Letting in immigrants with lower IQs will generally lead to a decline in national IQ, whereas letting in high-IQ immigrants will have the opposite effect (Borjas, 2016; Kirkegaard & Tranberg, 2015; Nyborg, 2012; Rindermann & Thompson, 2016; Woodley of Menie, Peñaherrera-Aguirre, Fernandes, & Figueredo, 2018). They discuss the current political realities of immigration, which need not concern us here. The final prediction for the future echoes Lynn's previous writings (Lynn, 2001): Western civilization is declining for a variety of reasons, and China will probably emerge as the global superpower sometime in this century.

All in all, the book is similar to the predecessors in presentation and discussion of material. On the negative side, there is little to no attempt at using advanced statistical methods to clarify matters of disputed causality or even just the relative importance of predictors. Existing studies on the question are not seriously discussed either, and an important opportunity is missed. The presentation is quite dry. On the positive side, the book describes the current state-of-the-art calculations for national IQs, introduces the reader to the open data set of national IQs, shows their high

replicability, and reviews their use by tens if not hundreds of other researchers. This last point bears noting because it shows that, despite criticism, the national IQ subfield has become a very productive research program (Rindermann, 2013b; Urbach, 1974a, 1974b). In fact, one might say that national IQs are getting quite popular because various other groups have begun publishing very similar national cognitive ability estimates (Angrist, Djankov, Goldberg, & Patrinos, 2019; Coutrot et al., 2018; Lim et al., 2018), even if they call it something other than intelligence and rarely cite the pioneering efforts of Richard Lynn and colleagues.

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Received June 12, 2019
Accepted June 16, 2019 ■