We have elsewhere reviewed proposals to reform terminology and improve interpretations of conventional statistics by emphasizing logical and information concepts over probability concepts. We here give detailed reasons and methods for reinterpreting statistics (including but not limited to) $P$-values and interval estimates in unconditional terms, which describe compatibility of observations with an entire set of analysis assumptions, rather than just a narrow target hypothesis. Such reinterpretations help avoid overconfident inferences whenever there is uncertainty about the assumptions used to derive and compute the statistical results. Examples of such assumptions include not only standard statistical modeling assumptions, but also assumptions about absence of systematic errors, protocol violations, and data corruption. Unconditional descriptions introduce uncertainty about such assumptions directly into statistical presentations of results, rather than leaving that only to the informal discussion that ensues. We thus view unconditional description as a vital component of good statistical training and presentation.

KEYWORDS
Statistics · Confidence Intervals · Cognitive Science · Bias · Statistical Models · Information · Data Interpretation · Hypothesis Tests · $P$-values · Statistical Significance · Evidence

1 | BACKGROUND

The present paper (a companion to Chow & Greenland, 2019 [1]) addresses in detail how even accurate descriptions of statistical outputs are misleading when there is considerable uncertainty about background assumptions. This is because they condition on background assumptions (i.e., they treat them as given), and thus do not factor into their assessments the uncertainties that surround those assumptions. While various risk-assessment methods can incorporate such uncertainties, those methods demand considerably more skilled user input than do conventional regression methods [2–5]. We thus present a relatively direct and non-technical approach to assumption uncertainty, called de-conditioning: Treat uncertain assumptions unconditionally by shifting their logical status in descriptions, removing them from what is assumed and placing them instead into what is tested.

We have found that this recommendation to decondition inferences [6] is the most difficult for most readers to comprehend, and is even resisted and misrepresented by some with extensive credentials in statistics. Thus, the present paper explains at length the rationale for de-emphasizing traditional conditional interpretations in favor of unconditional interpretations.

2 | AN EXAMPLE

As in part I [1], we will illustrate problems and recommendations with published results from a record-based cohort study of serotonergic antidepressant prescriptions during pregnancy and subsequent autism spectrum disorder (ASD) of the child [7]. That paper first reported an adjusted ratio of ASD rates (hazard ratio or HR) of 1.59 when comparing mothers with and without the prescriptions, and 95% compatibility ("confidence") limits (CI) of 1.17 and 2.17. This estimate was derived from a proportional-hazards model which included maternal age, parity, calendar year of delivery, neighborhood income quintile, resource use, psychotic disorder, mood disorder, anxiety disorder, alcohol or substance use disorder, use of other serotonergic medications, psychiatric hospitaliza-
tion during pregnancy, and psychiatric emergency department visit during pregnancy.

The paper then presented an analysis with adjustment based on a high-dimensional propensity score (HDPS), in which the estimated hazard ratio became 1.61 with a 95% CI spanning 0.997 to 2.59. Despite the estimated 61% increase in the hazard rate in the exposed children and an interval estimate including ratios as large as 2.59 and no lower than 0.997, the authors still declared that there was no association between in utero serotonergic antidepressant exposure and ASD because it was not “statistically significant.” This was a misinterpretation of their own results, insofar as an association was indeed present [8, 9] and quite close to the 70% increase they reported from other studies [10].

In what follows, we will explain the logic underpinning correct descriptions of these results, such as,

“After HDPS adjustment for confounding, a 61% hazard elevation remained; however, under the same model, every hypothesis from no elevation up to a 160% hazard increase had \( p \geq 0.05 \); Thus, while quite imprecise, these results are most consistent with previous observations of a positive association between antidepressant exposure and subsequent ASD (although the association may be partially or wholly due to uncontrolled biases).”

3 | DECONDITIONING BY EXPLICATION OF ALTERNATIVE CAUSES OF OBSERVATIONS

As is well known, the presence of an association in multiple observational studies does not by itself mean the drugs under study cause autism. In fact, the authors argued that the associations seen in their initial results [7, 10] represented confounding – a spurious association due to an association of the drugs with the actual causes. But such a confounding hypothesis should not be confused with lack of association; instead it should be treated as one of several possible explanations (ranging from real drug effects to random error), any or all of which may be contributing to the observed associations.

The statistical adjustments used by Brown et al. [7, 10] were in fact designed to minimize confounding, and thus they greatly (though not entirely) reduce its plausibility as a major source of the observed association. Furthermore, some of the suggested explanations might have reduced the observed association; in particular, random error is just as likely to deflate as to inflate an observed association. But without study design features to block alternative explanations (e.g., randomization to prevent confounding), statistical results cannot settle these matters. We thus need statistical descriptions that avoid impressions of being definitive and acknowledge possible alternative explanations.

Even when technically correct, common interpretations of statistics are deficient in this regard. Consider that a test with \( p = 0.0625 \) yields \( s = −\log_2(p) = 4 \) bits of information against the test hypothesis \( H \), if all the assumptions used in the test are correct [1, 6]. This description is conditional, in that it assumes an entire set of background conditions in order to compute and interpret \( p \) and \( s \). Typical examples of background assumptions include: patient outcomes are independent; interactions and trends follow the regression model used for analysis (e.g., linear or logistic); there is no uncontrolled source of systematic error (e.g., no uncontrolled confounding, subject-selection bias, measurement error, or sparse-data bias) [11]; and there is no selective reporting of results based on their \( P \)-values, interval estimates, or any other output – or if there is such selection, it is accounted for in the computation of the final results. Taken together, these background assumptions compose the underlying analysis model.

We will refer to the combination of the test hypothesis \( H \) and this underlying background as the test model [12, 13]. In field studies and studies of human subjects, this test model is hypothetical, for it is never the case that all the background assumptions are correct: There are always study problems, and it is implausible that any statistical model we use (whether for outcomes or exposures) is correct or complete in all respects. Modest violations of background assumptions can easily bias the \( P \)-value toward 0 or 1, moving it over thresholds (regardless of whether the test hypothesis is correct), thus invalidating decisions based in part on that \( P \)-value. In mechanistic terms, the possible causes of an extreme test statistic and thus a small \( P \)-value include not only the targeted hypothesis \( H \) being wrong, but also or instead some other
assumption violation.

We thus can and should view the \( P \)-value as referring to a probability derived from the entire test model, and the \( S \)-value as measuring the information supplied by the test statistic against that entire model. This description is unconditional because it places the background assumptions on equal footing with the test hypothesis \( H \): it explicitly states that violation of any one of them may be responsible for the results, with no conditions imposed. For example, selection of models that yield narrow intervals (for “higher precision”) will cause small \( P \)-values even if the test hypothesis is correct. Then too, selection of models or methods that yield wider intervals (for “conservative inferences”) can cause large \( P \)-value even if the test hypothesis is false.

More generally, the smaller the \( P \)-value and thus the larger the \( S \)-value we observed, the more justification we have for saying that it appears one or more assumptions in the test model is wrong. This unconditional analysis does not however indicate which assumptions are wrong. The reasons for the assumption violations might possibly include that the test hypothesis \( H \) is false, but may instead or in addition include uncontrolled bias, or data-collection errors, or programming errors, or data “doctoring,” or some other deviation from the background assumptions hidden in traditional interpretations. This information limitation of statistical analyses is inherent and universal; a notable example is the report of faster-than-light neutrinos which turned out to be due to equipment defects [14].

In parallel, if we observe a large \( P \)-value and thus a small \( S \)-value, we cannot conclude that there is no violation of any assumption; quite contrarily, it may be that the assumption violations biased the \( P \)-value upward instead of downward. This caution is just the unconditional version of the warning dating back to Pearson [15] and often repeated since [16–19], that a large \( P \)-value is not evidence that the test hypothesis is correct. As reflected by the small \( S \)-value, it simply means the test supplied little information against the test hypothesis or any other assumption used to compute the \( P \)-value. This lack of information reflects only the limitations of the test (which in turn may reflect limitations of the study), not the absence of an effect.

4 | THE NECESSITY OF UNCONDITIONAL INTERPRETATIONS

The conditional and unconditional interpretations are contrasted in Figure 1, which shows how the conditional interpretation

- (A) targets only the test hypothesis \( H \) under the dubious condition that there are no violations of background assumptions, whereas the unconditional interpretation
- (B) targets the entire set of assumptions used to frame the test.

The unconditional interpretation is usually far more appropriate in health and medical sciences, where researchers rarely achieve full control of all potential sources of systematic error (even randomized trials will suffer from drop-out, censoring and related problems that may create bias). In contrast, in many “hard science” experiments researchers may control all important conditions and thus justify a conditional interpretation – but again, serious exceptions occur even in particle physics [14].

We view explication of the conditional vs. unconditional distinction as crucial to good teaching, and the unconditional view as essential for good practice: When (as usual in our experience) there is meaningful doubt about the assumptions underlying a statistical procedure, we need to remind ourselves of the unconditional fact that any result (“large” or “small”) may have occurred not only from “chance” but also from assumption violations. Such reminders are seen in well-reported studies, which list and caution about possible sources of bias in the study. We thus hold that unconditional interpretations need to be covered whenever any reasonable doubt can be raised about background assumptions.

The unconditional interpretation is far more helpful than the conditional when there are concerns about violations of assumptions used by the latter. Suppose for example there are plausible concerns about violations of the data collection, processing, or reporting protocols. A common concern is that a \( P \)-value was selected for special emphasis out of several based on its size (whether for being high, “downward hacking”; or low, “upward hacking”)

[14]
or a CI was selected from several based on including or excluding the null, while the other results were downplayed or unreported (thus becoming nonrandomly missing data information). A conditional interpretation assumes there is no such uncontrolled selection of summaries based on what they favor, and so is a potentially misleading hypothetical when selection occurs. In contrast, an unconditional interpretation simply lists uncontrolled selection bias among the possible causes contributing to the observed P-value.

To see the general distinction, we must imagine an open-ended list of contextually plausible mechanistic (causal) explanations for the observed statistics. With conditional interpretations, the only explanations allowed from that list are those consistent with the background assumptions used to compute the statistics; thus, in testing, conditional explanations can only enlist some combination of random error and violation of the test hypothesis H. Because assumption uncertainty is neglected by this interpretation, it creates an illusion that "nonsignificance"/"significance" should be treated as the true/false indicator for H.

In contrast, an unconditional interpretation considers the entire list, including nonrandom physical (causal) mechanisms that violate background assumptions rather than H; such mechanisms may for example produce nonrandomly missing information (informative censoring). In the face of information against the full tested model (H and the background assumptions), this list of possible explanations needs to contain any causal mechanism whose possibility is seen to be near or exceed that of "chance" (the hypothesis that all the causal effects producing the data from the tested model were blocked by conditioning on the model) [20]. In typical social and biomedical applications, there will be multiple such explanations, and they will not be mutually exclusive; for example, an explanation for a temporally directed association will include direct causation, bias, random error, and every combination of the three that produces what was observed.

The multiple explanations allowed by the unconditional view show why it would be an inversion fallacy to say that an S-value measures the information support-
ing or favoring an alternative hypothesis. Considering the example, it would be wrong to say the S-value of 4.31 against the no-effect hypothesis (that the drug does not affect the hazard) measures the information favoring the alternative that taking the drug increases the hazard: Such an interpretation would have to assume that the 61% higher hazard seen with the drug is solely a product of genuine drug effects and random errors, which is not credible due to the possibility of systematic errors from failure of background assumptions (such as the assumption of no uncontrolled bias).

5 | COMPATIBILITY IS INTENTIONALLY LIMITED

One may object that, even unconditionally, compatibility interpretations will still be biased by assumption violations. That objection is simply a failure to understand the meaning of "unconditional": Unconditionally, "high compatibility" merely says the chosen testing procedure did not detect an assumption violation; it makes no claim whatsoever that such violation or resulting bias is absent. It is thus a response to the maxim "absence of evidence is not evidence of absence" [18] in the form of a retreat from any inference about why the data and the model appear as compatible or incompatible as they do.

In general, unconditional compatibility interpretations refuse to satisfy demands for conclusive assessments, even of uncertainty. The core idea is that conventional statistics can only gauge incompatibilities between our data and the models we use to analyze that data. At most, those statistics provide only falsification (never support) of the precise and detailed explanations represented by certain models. And because there is no restriction on how model violations may occur, the low compatibility of one model with the data does not provide support for a competitor: Each alternative model needs to be evaluated directly against the data, with its own P-value and S-value.

Unconditional descriptions are far more appropriately reserved and cautious compared to inherently conditional "long-run" or repeated-sampling descriptions. While this caution may seem excessive, justification of conditional descriptions requires empirical evidence against mechanisms that lead to assumption violations. And in one respect the unconditional interpretation is not cautious enough, because it is no substitute for model diagnostics such as residual plots and direct tests of model fit.

Unfortunately, some assumptions (such as no unreported model selection [21, 22]) will be untestable for the reader, while other assumptions will remain untestable (nonidentifiable) even if we are given the study data and full details on how it was collected. For example, the hypothesis that an observed association (or lack thereof) was due to confounding by an unmeasured variable cannot be tested without assumptions about the relation of that variable to those observed. Thus, if such a hypothesis is entertained seriously, an unconditional interpretation will avoid referring to the observed association as an "effect estimate" because the latter term invites conditioning on the assumption that the analysis successfully adjusted for all important confounding.

6 | UNCONDITIONAL INTERVAL ESTIMATES: COMPATIBILITY WITHOUT COVERAGE CLAIMS

A confidence interval (CI) is often defined as an interval that contains the true parameter value some percentage of the time (usually 95%) in some hypothetical "long run" involving unlimited study repetitions, with only random errors causing interval variation across these repetitions. Consequently, most descriptions write as if CIs are only defined or justified by their long-run coverage properties under the background assumptions [23], without considering unconditional interpretations.

One objection often raised to coverage is the unreality of the very hypothetical repetitions in which said coverage is supposed to take place, a long run which is in fact not necessary under information interpretations [6, 24]. Our primary concern however is that when the assumptions (model) used to compute the interval cannot be assured, neither can coverage, and the resulting confidence interval becomes an overconfidence interval [6, 16]. The coverage interpretation conveys valid information only when we know the assumption violations would not reduce coverage; otherwise, in the face of assumption uncertainty, coverage becomes an irrelevant conditional interpretation [3, 6, 16, 25]. We thus argue that teaching
and practice should de-emphasize long-run coverage in favor of more descriptive, purely logical properties of the intervals as provided by unconditional interpretations.

Specifically, we can bypass the need for a coverage interpretation by using the complementary mathematical relation between \( P \)-values and CIs. A CI of a particular level, say 95%, summarizes the results of varying the test hypothesis \( H \) over a range of parameter values, displaying all values for which \( P > 0.05 \) [26] and hence \( S < 4.32 \) bits [6, 16]. Thus, conditional on the background assumptions, the CI contains a range of parameter values that are more compatible with the data than values outside the interval [6, 13]. Unconditionally, and regardless of long-run coverage, the interval shows the values of the parameter which, when combined with the background assumptions, produce a test model that is “highly compatible” with the data in the sense of having less than 4.32 bits of information against the resulting test model. We thus refer to CI as **compatibility** intervals rather than **confidence** intervals [6, 16]; their abbreviation remains “CI.”

A CI shows the **conditional compatibility** between the data and various target-parameter values, given the background model assumptions; but it also shows **unconditional compatibility** between the data and a family of models identical apart from a varying target parameter [6, 16]. The unconditional-compatibility interpretation is important whenever background assumptions are uncertain, for then coverage becomes uncertain. In that case we can say that the interval describes a family of models defined by varying the parameter or hypothesis, plus the fixed set of background assumptions. These models differ only in the value they assign to the parameter targeted in the hypothesis \( H \); they share the property the data provide “little” information against them (less than 4.32 bits for 95% intervals).

Another attempt at expressing caution due to uncertain assumptions is to describe CIs as only gauging the amount of random error in the results [27]. For example, a randomized trial that produces a CI for a hazard ratio ranging from 0.90 (a 10% rate decrease) and as high as 20 (a 20-fold rate increase) is taken to indicate that the results are too noisy to pin down even the direction of the association. This view leads to use of interval estimates to plan studies for precision based on desired interval width [28, 29] (rather than statistical power, where one fixes on rejecting or failing to reject the test hypothesis). From the compatibility view, the goal is now to ensure that the region of compatibility above a given level is narrow enough to make the study reasonably informative **under the background assumptions**. That goal does not however address violation of those assumptions, which can drastically reduce the informativeness of the study; it can also bias estimates of random variation [3]. Thus the “random error only” interpretation of CIs is not an effective substitute for the unconditional interpretation.

### 7 | WHAT ABOUT THE NEED FOR DECISIONS?

We have been concerned only with how to validly describe statistical summaries. Decisions based on those summaries are often needed, but statistical decision theory [30] is a massive, deep topic beyond our current scope. A key point is that, when background assumptions are uncertain, neither conditional nor unconditional summarizations suffice for statistical decision methods: Those methods require some type of utility measure or loss function, along with prior distributions (whether empirical or subjective) that incorporate all the important uncertainties in the application. The conventional dichotomous decision framework (rejecting or accepting hypotheses based on whether a \( P \)-value passes some cutoff or an interval includes a parameter value) is based entirely on the conditional interpretation; whenever that involves assuming as if known things that are unknown, it fails the requirement for uncertainty accounting. The persistence of the conventional framework despite such deficiencies reflects the complexity of better methods and lack of agreement about simple replacements; but again, this is a huge topic beyond our current scope.

### 8 | CONCLUSION

Treating formal statistics as if they capture all important uncertainty sources has been labeled **uncertainty laundering** [31], which is exactly what is done whenever discussions revolve around whether results are “statistically significant” or whether interval estimates contain a null value. Even when this is not done, expressions of un-
certainty about analysis assumptions is usually reserved for informal discussion, which often seems to be “walking back” conditional descriptions from the results section.

In contrast, unconditional descriptions introduce assumption uncertainty directly into statistical presentations of results, recognizing that those results cannot claim to have captured uncertainty if there are plausible doubts about the assumptions or models used to derive the statistics. We thus view unconditional description as a vital component of good statistical training and presentation, one that should be at the forefront of the statistical reform movement.

ACKNOWLEDGEMENTS

We are most grateful for the generous comments and criticisms on our initial drafts offered by Andrew Althouse, Valentin Amrhein, Darren Dahly, Frank Harrell, John Ioannidis, Daniël Lakens, Nicole Lazar, Gregory Lopez, Oliver Maclaren, Blake McShane, Tim Morris, Keith O’Rourke, Kristin Sainani, Allen Schirm, Philip Stark, Andrew Vigotsky, Jack Wilkinson, and Corey Yanofsky. We also thank Karen Pendergrass for her help in producing the figures in this paper. Our acknowledgment does not imply endorsement of all our views by all of these colleagues, and we remain solely responsible for the views expressed herein.

AUTHORS’ CONTRIBUTIONS

Both authors wrote the first draft and revised the manuscript, read and approved the submitted manuscript, and have agreed to be personally accountable for their own contributions related to the accuracy and integrity of any part of the work.

ABBREVIATIONS

ASD: Autism spectrum disorder; CI: Compatibility/confidence interval; HDPS: High-dimensional propensity score; HR: Hazard ratio; LR: Likelihood ratio; S-value: Surprisal (Shannon-information) value

DATA AND MATERIALS

The datasets generated and analyzed in the current paper are available in the Open Science Framework | DOI: 10.17605/OSF.IO/6W8G9 and on figshare | DOI: 10.6084/m9.figshare.9846674.v3.

FUNDING

This work was produced with no funding.

COMPETING INTERESTS

The authors declare that they have no competing interests.

REFERENCES

[1] Chow, Z. R. & Greenland, S. Semantic and Cognitive Tools to Aid Statistical Inference: Replace Confidence and Significance by Compatibility and Surprise. arXiv:1909.08579 [stat.ME] (2019). https://arxiv.org/abs/1909.08579.

[2] Greenland, S. Multiple-bias modelling for analysis of observational data. J R Stat Soc Ser A Stat Soc 168, 267–306 (2005). https://doi.org/10.1111/j.1467-985X.2004.00349.x.

[3] Greenland, S. & Lash, T. L. Bias analysis. In Rothman, K. J., Greenland, S. & Lash, T. L. (eds.) Modern Epidemiology, 345–380 (Lippincott Williams & Wilkins, 2008), 3rd edn.

[4] Lash, T. L., Fox, M. P. & Fink, A. K. Applying Quantitative Bias Analysis to Epidemiologic Data (Springer New York, 2009). https://doi.org/10.1007/978-1-4020-8992-0.

[5] Lash, T. L. et al. Good practices for quantitative bias analysis. Int J Epidemiol 43, 1969–1985 (2014). https://doi.org/10.1093/ije/dyu149.

[6] Greenland, S. Valid P-values behave exactly as they should: Some misleading criticisms of P-values and their resolution with S-values. Am Stat 73, 106–114 (2019). https://doi.org/10.1080/00031305.2018.1529625.

[7] Brown, H. K. et al. Association between serotonergic antidepressant use during pregnancy and autism spectrum disorder in children. JAMA 317, 1544–1552 (2017). https://doi.org/10.1001/jama.2017.3415.

[8] McShane, B. B. & Gal, D. Statistical significance and the dichotomization of evidence. J Am Stat Assoc 112, 885–895 (2017). https://doi.org/10.1080/01621459.2017.1289846.
[9] McShane, B. B., Gal, D., Gelman, A., Robert, C. & Tackett, J. L. Abandon statistical significance. *Am Stat* 73, 235–245 (2019). https://doi.org/10.1080/00031305.2018.1527253.

[10] Brown, H. K., Hussain-Shamsy, N., Lunsky, Y., Dennis, C.-L. E. & Vigod, S. N. The association between antenatal exposure to selective serotonin reuptake inhibitors and autism: A systematic review and meta-analysis. *J Clin Psychiatry* 78, e48–e58 (2017). https://doi.org/10.4088/JCP.15r10194.

[11] Greenland, S., Mansournia, M. A. & Altman, D. G. Sparse data bias: A problem hiding in plain sight. *BMJ* 352, i1981 (2016). https://doi.org/10.1136/bmj.i1981.

[12] Box, G. E. P. Sampling and Bayes’ inference in scientific modelling and robustness. *J Roy Stat Soc Ser B Methodol* 40, 383–430 (1980). https://doi.org/10.2307/2982063.

[13] Greenland, S. et al. Statistical tests, P values, confidence intervals, and power: A guide to misinterpretations. *Eur J Epidemiol* 31, 337–350 (2016). https://doi.org/10.1007/s10654-016-0149-3.

[14] Moskowitz, C. Faster-than-light neutrinos aren’t. https://scim.ag/faster-than-light-neutrinos

[15] Pearson, K. V. Note on the significant or non-significant character of a sub-sample drawn from a sample. *Biometrika* 5, 181–183 (1906). https://doi.org/10.1093/biomet/5.1-2.181.

[16] Amrhein, V., Trafimow, D. & Greenland, S. Inferential statistics as descriptive statistics: There is no replication crisis if we don’t expect replication. *Am Stat* 73, 262–270 (2019). https://doi.org/10.1080/00031305.2018.1543137.

[17] Fisher, R. A. *Statistical Methods for Research Workers* (Edinburgh, Oliver and Boyd, 1925).

[18] Altman, D. G. & Bland, J. M. Absence of evidence is not evidence of absence. *BMJ* 311, 485 (1995). https://doi.org/10.1136/bmj.311.7003.485.

[19] Fisher, R. A. Statistical methods and scientific induction. *J R Stat Soc Ser B Methodol* 17, 69–78 (1955). https://doi.org/10.1111/j.2517-6161.1955.tb00180.x.

[20] Greenland, S. The causal foundations of applied probability and statistics. In Geffner, H., Dechter, R. & Halpern, J. (eds.) *Probabilistic and Causal Inference: The Work of Judea Pearl*. (In press, 2020).

[21] Gelman, A. & Loken, E. The garden of forking paths: Why multiple comparisons can be a problem, even when there is no “fishing expedition” or “p-hacking” and the research hypothesis was posited ahead of time. http://depts.washington.edu/stat/share/Department of Statistics, Columbia University (2013).

[22] Gelman, A. & Loken, E. The statistical crisis in science. *American Scientist* (2014). https://americanscientist.org/article/the-statistical-crisis-in-science.

[23] Morey, R. D., Hoekstra, R., Rouder, J. N., Lee, M. D. & Wagenmakers, E.-J. The fallacy of placing confidence in confidence intervals. *Psychon Bull Rev* 23, 103–123 (2016). https://doi.org/10.3758/s13423-015-0947-8.

[24] Vos, P. & Holbert, D. Frequentist inference without repeated sampling. arXiv:1906.08360 [stat.OT] (2019). https://arxiv.org/abs/1906.08360.

[25] Greenland, S. A serious misinterpretation of a consistent inverse association of statin use with glioma across 3 case-control studies. *Eur J Epidemiol* 32, 87–88 (2017). https://doi.org/10.1007/s10654-016-0205-z.

[26] Cox, D. R. *Principles of Statistical Inference* (Cambridge University Press, 2006). https://doi.org/10.1017/cbo9780511813559.

[27] Rothman, K. J., Greenland, S. & Lash, T. L. Precision and statistics in epidemiologic studies. In Rothman, K. J., Greenland, S. & Lash, T. L. (eds.) *Modern Epidemiology*, 148–167 (Lippincott Williams & Wilkins, 2008), 3rd edn.

[28] Greenland, S. On sample-size and power calculations for studies using confidence intervals. *Am J Epidemiol* 128, 231–237 (1988). https://doi.org/10.1093/oxfordjournals.aje.a114945.

[29] Rothman, K. J. & Greenland, S. Planning study size based on precision rather than power. *Epidemiology* 29, 599–603 (2018). https://doi.org/10.1097/EDE.0000000000000876.

[30] Parmigiani, G. & Inoue, L. *Decision Theory: Principles and Approaches* (John Wiley & Sons, 2009). https://doi.org/10.1002/9780470746684.

[31] Gelman, A. The problems with P-values are not just with P-values. *Am Stat* 70 (2016). https://stat.columbia.edu/~gelman/research/published/asa_pvalues.pdf.