Investigating the analytical robustness of the social and behavioural sciences

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The same dataset can be analysed in different justifiable ways to answer the same research question, potentially challenging the robustness of empirical science[1–3]. In this crowd initiative, we investigated the degree to which research findings in the social and behavioural sciences are contingent on analysts' choices. We examined a stratified random sample of 100 studies published between 2009 and 2018, in which, for one claim per study, at least five reanalysts independently reanalysed the original data. The statistical appropriateness of the reanalyses was assessed in peer evaluations, and the robustness indicators were inspected along a range of research characteristics and study designs. We found that 34% of the independent reanalyses yielded the same result (within a tolerance region of ±0.05 Cohen's d) as the original report; with a four times broader tolerance region, this indicator increased to 57%. Of the reanalyses conducted, 74% reached the same conclusion as the original investigation, 24% yielded no effects or inconclusive results and 2% reported the opposite effect. This exploratory study indicates that the common single-path analyses in social and behavioural research should not be simply assumed to be robust to alternative analyses[4]. Therefore, we recommend the development and use of practices to explore and communicate this neglected source of uncertainty.

Over the past decade, social and behavioural scientists have been striving to enhance the robustness, objectivity and replicability of their findings through systemic reforms in the conduct and communication of empirical research. Practices such as preregistration[5], registered reports[6], multisite replications[7], analytical reproducibility checks[8,9] and automated result validation techniques[10] have been investigated and recommended to produce robust and replicable findings. An important aspect of robustness has yet to be systematically charted across these sciences: the contingency of the results on researchers' analytical choices.

In a typical research pipeline, the collected empirical data are analysed by a single analyst or team, and the published report presents a conclusion on the basis of one analytical path, occasionally accompanied by a few robustness tests. The peer review process aims to ensure that the analysis approach meets the relevant statistical and field-specific standards. However, this procedure does not systematically ascertain whether justifiable alternative analytical choices could have led to different results.

Theories and empirical designs rarely constrain analysts to a single analytical path. Many degrees of freedom exist in how researchers operationalize their variables, process their data, construct their statistical models, select algorithms and software for model estimation and define their inference criteria, whether they follow frequentist, Bayesian or likelihoodist analytical approaches; use machine learning; or conduct computational modelling to answer the same research question[1,4]. This inherent freedom of the analyst constitutes the so-called analytical variability contained within empirical projects, a key component in the robustness of the statistical results. In practical terms, it is the manifested variation among the choices independent scientists consider justified. Figure 1 presents some sources of analytical variability that can manifest themselves in analysts' statistical results and the conclusions drawn from the results.

One way to explore analytical variability is to use a multiverse methodology[2,11], in which analysts conduct all combinations of analytical choices they are able to generate across a wide range of reasonable scenarios. Alternatively, in the multi-analyst approach, several analysts analyse the data following their best judgement. The latter approach requires more organization, but it takes advantage of alternative expert perspectives without the combinatory expansion of the number of results. A multi-analyst approach also examines naturally occurring variation, empirically answering the counterfactual question of what might have happened if another investigator had considered the same research question using the same data.

Multi-analyst projects[3,12–24] have provided some evidence of the extent to which analysts' individual choices influence results and conclusions. From economics to neuroscience, these explorations have demonstrated that the robustness of empirical findings can be compromised by researcher degrees of freedom[25]. The estimates of previous multi-analyst studies suggest that the variability in effect-size estimates attributable to analytical heterogeneity can exceed the variability one would expect owing to sampling error[26].

Do we know how robust published findings are to analytical choices across the social and behavioural sciences? One could argue that multi-analyst projects so far have been purposefully conducted in research areas with little consensus on the best analytical approach or were motivated to demonstrate the potential effects of analytical choices and, therefore, may represent rare cases in which alternative analyses produce important differences in results. For example, the datasets selected may have afforded researchers greater degrees of freedom than is typical, raising issues about the generalizability of

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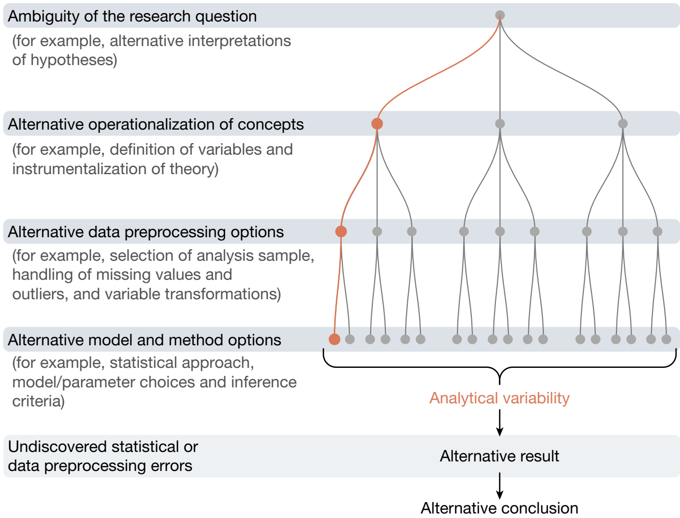

Fig. 1 | Main sources of analytical variability. Analytical variability can arise from the ambiguity of the research question, alternative operationalizations of concepts, variations in data preprocessing options, model and method choices and undiscovered statistical or data processing errors.

the findings to scientific research more broadly. Differences between academic methodologies and fields also seem plausible. For example, relatively simple experiments sometimes used in social psychology and behavioural economics may contain fewer analytical decisions than the complex longitudinal observational datasets used in macroeconomics and finance, and therefore may be more analytically robust in general[22]. To the extent that this is the case, findings from the existing multi-analyst projects could be biased towards worst-case scenarios, and the traditional analytical practice and review system may not require fundamental adjustments. If, on the other hand, observed results are contingent on the analyst's choices across fields, methodologies and types of dataset, then the scientific literature could be less robust than is often assumed. If so, the general practices of how we conduct, report and review empirical analyses should be reformed to address this source of uncertainty.

After conducting 504 reanalyses with the involvement of 457 independent reanalysts on a stratified random sample of 100 social and behavioural studies, we conducted strictly exploratory analyses to describe the patterns in the findings. Inspecting the results across different research characteristics and study designs gives rise to a number of hypotheses for future research on how to maximize transparency and address this often-neglected component of scientific uncertainty.

Variability of the results

To explore the robustness of published claims, we selected a key claim from each of our 100 studies, in which the authors provided evidence for a (directional) effect. We presented each empirical claim to at least five analysts along with the original data and asked them to analyse the data to examine the claim, following their best judgement, and report only their main result. The analysts were encouraged to analyse studies where they saw the greatest relevance of their expertise. Therefore, in this study, analytical variability, as a key component of robustness, is defined as the variation among the analytical results when different analysts are provided with the same research questions and data.

First, we explored the degree to which the reanalysts produced the same statistics in the reanalysis of each study. We found that in 81% of the studies, the corresponding analysts reported different statistics about statistical test families (such as t-tests, F-tests and χ^2 tests) and their values (after rounding them to two decimal places).

A challenge in any multi-analyst project is to find a common metric that allows the results of the different analyses to be compared. A practical solution is to transform the reported point estimates into a standard effect-size measure. Although these transformations have limitations and their calculation relies on assumptions that may not hold in all considered analysis settings[25–28], for the sake of comparability, we decided to compute Cohen's d for each reanalysis, wherever it was feasible (for an alternative approach, see Supplementary Fig. 1). The methods, materials, analysis plan, peer evaluation and data management strategy of the project were preregistered on the Open Science Framework (OSF) repository of the project (https://osf.io/q5h2c) (deviations from the registered plan are reported and explained in 'Deviations from preregistration' in the Supplementary Information). In our preregistration, we defined two results as qualitatively the same when their effect sizes are within the tolerance region of ±0.05 Cohen's d. However, we also present analyses with alternative tolerance regions. Our results revealed how far the new estimates were from the original ones (Fig. 2a) and how often the effect sizes of the reanalyses fell within this tolerance region (Fig. 2i).

We found that in 5% (5 out of 95) of the studies for which we could obtain the original effect size, all reanalysis effect sizes were inside the tolerance region (±0.05 Cohen's d) of the result of the original study (Fig. 2a). Of the 396 available reanalysis effect sizes, 34% were inside the tolerance region. As a robustness test of our analysis, we explored the degree to which we would observe different results with different tolerance regions. With a four times broader tolerance region (±0.20 Cohen's d), in 23% of the studies, all corresponding reanalysis results were inside the tolerance region. Further, of the 396 available reanalysis effect sizes, 57% (224) were within this region (Extended Data Fig. 1a).

Alternatively, we could define the tolerance region as the percentage of the given effect size. As a further robustness test, we varied the tolerance region between ±5% and ±20%, but it made barely any difference to the percentage of robust studies (Extended Data Fig. 1b).

We next considered whether these robustness results vary by study discipline, design, expertise of the analysts, their prior familiarity with the data or sample size in the data. Figure 2b,c presents the results for the major disciplines in our sample (ten or more studies). For Fig. 2c, we created an effect-size estimate range for each study as the numerical difference between the highest and lowest estimates of reanalysis effect sizes. In our reading, the listed disciplines do not yield large differences in the robustness of the results. Still, it is reasonable to think that the level of analytical robustness in different disciplines can be influenced by the types of study commonly conducted. For example, one could conjecture that empirical claims on the basis of observational data show lower robustness of the conclusions because they probably involve more researcher degrees of freedom in terms of viable analysis paths than experimental research settings. Figure 2d,e explores this question and indicates that the results of studies with observational study designs have lower analytical robustness in our sample, relative to experimental designs (Supplementary Tables 5 and 6).

Considering the analytical variability found in the statistical results of the reanalyses, one immediate concern is that it could be an artefact of a lack of analytical expertise among some reanalysts. Therefore, we explored whether our robustness results exhibit a different pattern when examined in relation to the self-reported statistical expertise of the reanalysts. Visual inspection of Fig. 2f shows no support for this proposition, because a higher level of expertise corresponds with no increase or decrease in the ratio of the reported results being different from the original ones. It is noteworthy, however, that the level of self-perceived expertise was clustered at the higher end of the scale.

Reanalysing published studies entails a potential risk of bias if the reanalysts' familiarity with a given study influences their choice of analysis. Reanalysts reported that they were familiar with the original study in only 8% of cases. Moreover, there was no more than 3% difference in robustness between those who were and those who were

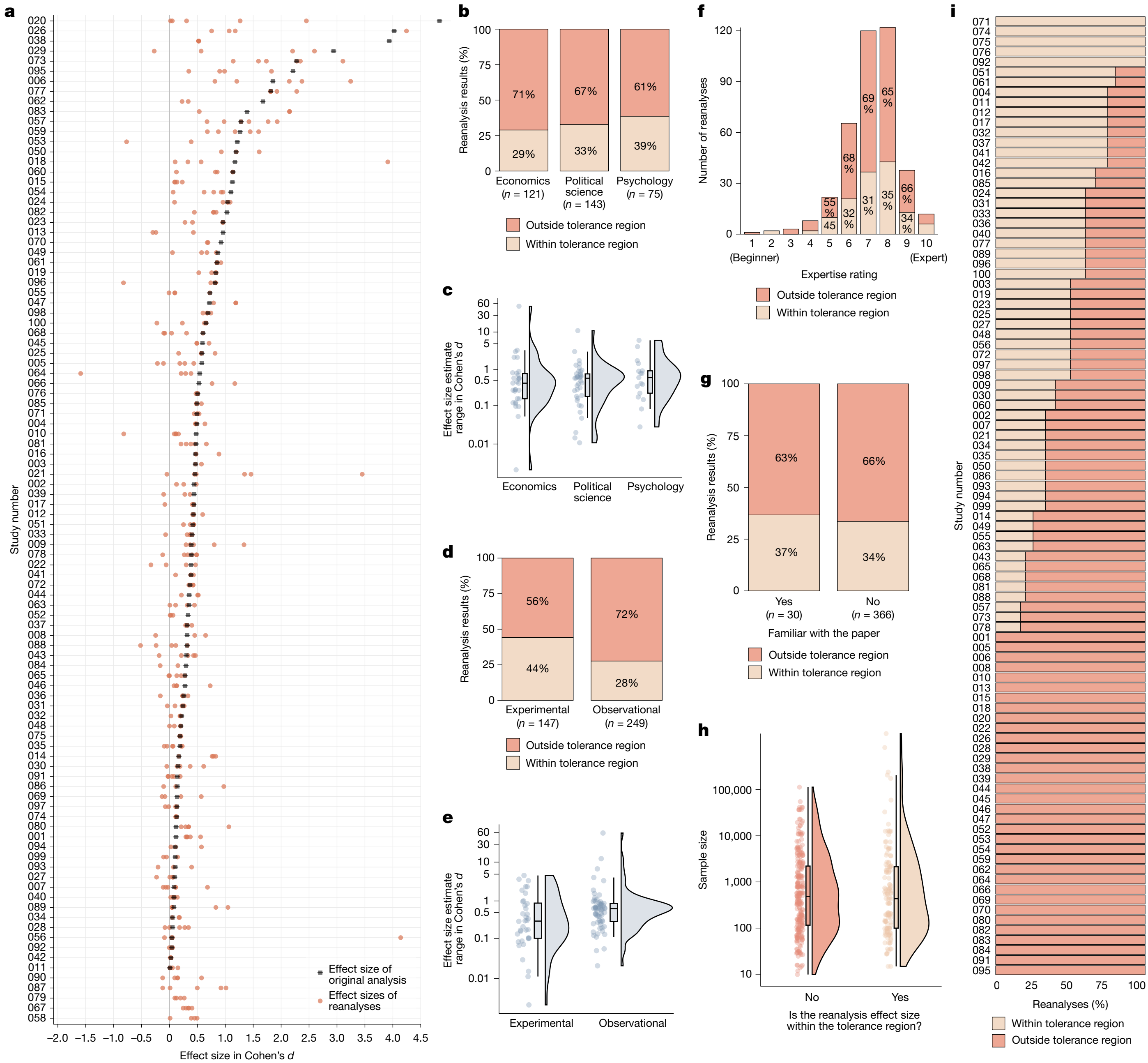

Fig. 2 | Analytical robustness of the statistical results. a, Effect size of the original analysis (grey squares; all represented as positive values) and the effect sizes of the reanalyses (red dots) for each study. The figure displays 415 reanalysis effect-size estimates that were convertible to Cohen's *d* and excludes effect sizes outside the [−2, 4.5] range. For the five studies listed at the bottom of the figure, we could not determine the original effect size because of missing information. Study numbers correspond to the studies listed at https://osf.io/mkwhn. The studies are ordered by the original effect size. **b**, Percentage of reanalysis results falling within or outside the tolerance region of the original results of the studies by major disciplines. The figure displays the count of reanalyses next to each discipline name. **c**, Distributions of effect-size estimate ranges calculated per study for each major discipline. **d**, Proportion of reanalysis results falling within or outside the tolerance region of the original results of the studies by study type. The figure displays the count of reanalyses next to each discipline name. **e**, Distribution of effect-size estimate ranges calculated per study for observational and experimental studies. **f**, Percentage of reanalysis results falling within or outside the tolerance region of the original results of the studies by self-rated expertise (1, beginner; 10, expert). **g**, Percentage of reanalysis results falling within or outside the tolerance region of the original results of the studies by declared familiarity with the study. **h**, Distribution of sample sizes separately for reanalysis effect sizes falling within or outside the tolerance region of the original results. **i**, Proportion of effect sizes falling within the preset tolerance range (±0.05 Cohen's *d*) for each study.

not familiar with the original study (Fig. 2g). For both groups, around two-thirds of the estimates fell outside our tolerance region. Finally, we were interested to see whether these robustness results would show a different pattern when considering sample size, as one could assume that studies with larger sample sizes could offer more robust results. Figure 2h does not support this assumption, because the density distributions of the sample sizes for results that are within and outside of the tolerance region are virtually the same. Therefore, studies with large sample sizes are not immune to analytical variability.

We next asked whether the reanalyses show a trend or shift in effect sizes compared with the results of the original studies. If the reanalysis effect sizes randomly vary around the original effect size, we would expect that they are larger or smaller than the original ones with an equal chance. Figure 3a,b (reanalysis data trimmed at Cohen's $d \leq 5$

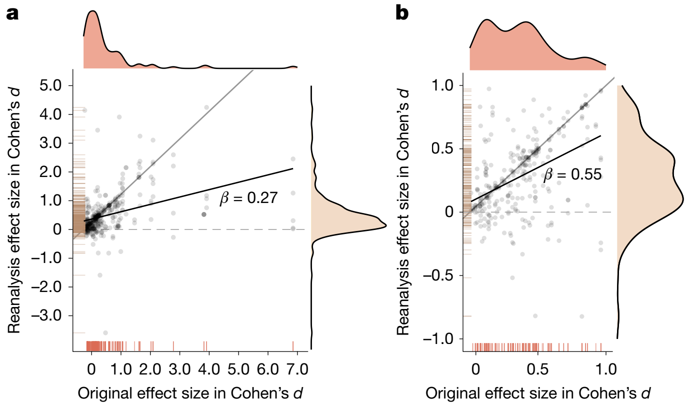

Fig. 3 | Original study effect size versus reanalysis effect size. The thin diagonal line represents the ideal case in which the reanalysis effect sizes are equal to the original effect size, whereas the thick line shows the best-fitting (least-squares) line of the displayed dots. Density plots of original ($n = 95$) and reanalysis ($n = 504$) effect sizes are parallel to their respective axes. β refers to the regression slope. **a** shows the effect sizes with Cohen's $d \leq 5$, whereas **b** shows the same for effect sizes with Cohen's $d \leq 1$.

and $d \leq 1$, respectively) indicates that reanalysis effect sizes show a tendency to be smaller than the original effect sizes, as reflected in their best-fitting (least-squares) line. The distribution of original and reanalysis effect sizes also supports this, because the peak of the density distribution of the latter is markedly lower. The original results showed a mean effect size of 0.73 (median = 0.43), whereas the reanalysis yielded a mean effect size of 0.49 (median = 0.35), with Cohen's d calculated for d values ≤ 5. This result is consistent with the possibility that the original authors were biased towards reporting larger effects than the reanalysts, that the reanalysts were biased towards reporting smaller effects than the original analysts, or both.

Variability of the conclusions

Another focal question of our study was whether the reanalysts reached the same qualitative conclusions as the original study analysts. To answer this question, we asked the reanalysts to implement any statistical reanalysis they deemed most appropriate to test the original claim using the original data, with the goal of arriving at a single conclusion. Across all individual reanalyses ($n = 504$), 74% of analyses arrived at the same conclusion as the original investigation, 24% yielded no effects or inconclusive results and 2% indicated an effect in the opposite direction from the original investigation (Fig. 4a).

Of 100 reanalysed claims, 34% were robust to independent reanalysis, such that all reanalysts reported that they found evidence for the originally reported claim. However, this result is contingent on the level of agreement we use to define analytically robust findings. With a more liberal definition of analytical robustness, this value was 39% when analytical robustness was defined as greater than 80% reanalysis agreement with the original conclusion, and it was 80% when this definition was greater than 50%. The results with alternative levels of agreement are displayed in Fig. 4j.

We examined whether these results show a different pattern when we inspected them along the previously mentioned aspects of the analyses. Figure 4b,c presents the proportions of conclusions that were robust in each of the listed disciplines. As in the analyses of the robustness of the statistical results, the listed disciplines do not manifest large differences in robustness of the conclusions, whereas their robustness may be influenced by the study designs most common in a given field or subfield. Figure 4d supports this notion, as it indicates that nearly half of the conclusions from experimental studies remained robust upon independent reanalysis, whereas less than one-third of observational studies yielded robust conclusions. Moreover, Fig. 4e indicates that, although most of the reanalyses for both study designs reached the same conclusions as the original study, the figure was 13% higher for experimental studies than for observational studies. As with the robustness of the results, we can ask whether the deviation from the originally reported claim in terms of conclusions is explained by the reanalysts' lack of analytical expertise. Figure 4f shows no support for this conjecture when evaluating the pattern of results as a function of self-reported statistical expertise. The same conjecture can be assessed by considering the quality of the submitted statistical analyses that were evaluated by peer evaluators on a subset of the analyses (Methods). Figure 4g shows that the proportion of inferentially robust conclusions is numerically larger for analyses that were rated as medium quality by peer evaluators than for analyses that were rated as high quality. Whether this pattern was a result of noise or whether more sophisticated analyses are characterized by greater heterogeneity in approaches and results should be the topic of future metascientific projects.

As with the analyses of the robustness of the statistical results, we were interested to see whether these results showed a different pattern when we inspected them as a function of the analysts' prior familiarity with the dataset. Although those familiar with the original study reported the same conclusion in a higher proportion than those who were not familiar, 17% of their reanalyses still indicated a conclusion different from the original (Fig. 4h).

Again, we aimed to explore whether these robustness results would show a different pattern when we considered sample size. As presented in Fig. 4i, the density distribution corresponding to the analyses with the different conclusion types shows a comparable spread, suggesting that the conclusions of studies with smaller and larger sample sizes seem to be similarly contingent on analytical choices.

For descriptive information about the reanalysts, peer evaluators and further robustness analyses, see Extended Data Figs. 2–4 and Supplementary Information (general descriptives, demographics of the reanalysts, peer evaluation and robustness analysis sections).

Limitations

This study has a number of limitations. First, our collection of 100 articles represents only a tiny fraction of all the empirical studies in the social and behavioural disciplines. Despite our efforts to select a representative sample of published articles across disciplines from the investigated time period, we excluded studies for which the underlying data were not obtainable and when our screening attempt to analytically reproduce the original results following the published procedures failed. We cannot exclude the possibility that these prerequisites, in addition to the self-selection of the analysts, led to sampling bias.

Although we conducted more than 500 analyses, our project included only five independent analyses for most datasets. Therefore, we do not know to what degree these analyses capture the full variability of analyses and results for the given research question and dataset. In addition, because we reanalysed already published studies and the reanalysts were provided with these studies, the original analysis pipeline could have anchored some of the choices of the reanalysts. On the other hand, some analysts may have been motivated to produce alternative results, given the basic scientific incentive to present something new.

Although Cohen's d has the advantage of being easy to compute and comparable across different analyses, Kümpel and Hoffmann[29] recently proposed the concept of generalized marginal effects, an effect size metric that is both formally applicable and comparable across different statistical models. We had not originally planned to calculate standardized generalized marginal effects and, accordingly, did not collect all required analysis outputs to compute them across the board. Still, we calculated generalized marginal effects for a sample of our studies to showcase their potential for future multi-analyst studies (Supplementary Fig. 1).

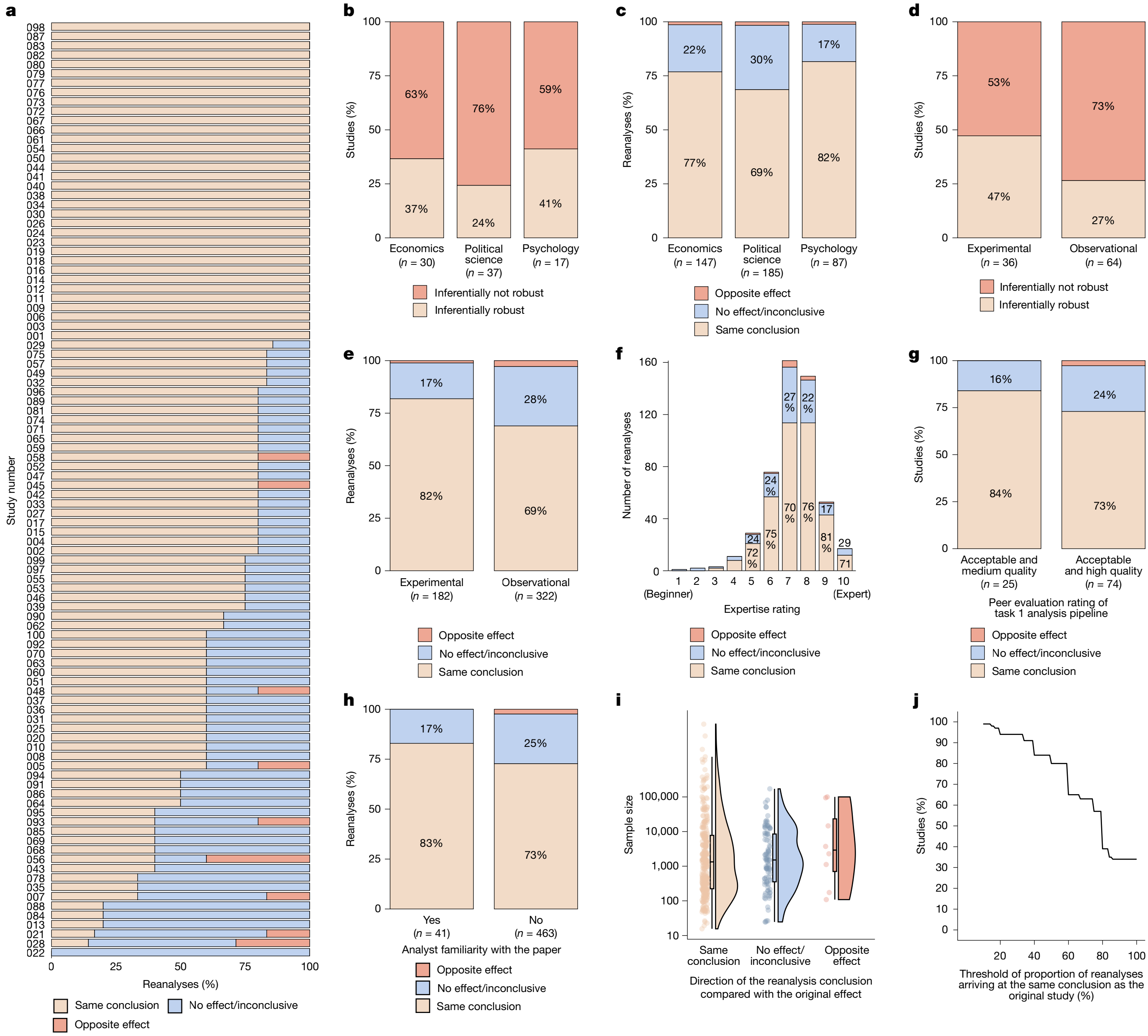

Fig. 4 | Analytical robustness of the conclusions. **a**, Proportion of analyses reaching the same conclusion, yielding no effect/inconclusive results and indicating an opposite-direction conclusion for each study. Study numbers correspond to the studies listed at https://osf.io/mkwhn. **b**, Proportion of inferentially robust results (all reanalyses arrived at the same conclusion for the given study) by major disciplines (more than ten studies in our collection: economics, political science and psychology). **c**, Proportion of analyses showing the same effect, no effect/inconclusive results and conclusions in the opposite direction to the original studies by major discipline. The number of reanalyses is displayed below each discipline. **d**, Proportion of inferentially robust results by study design (experimental versus observational). The number of reanalyses is given below each study design. **e**, Proportion of analyses showing the same conclusion, no effect/inconclusive results and opposite effect to the reanalyses by study type (experimental or observational). **f**, Proportion of analyses showing the same conclusion, no effect/inconclusive results and opposite effect by self-rated expertise (on a scale of 1 (beginner) to 10 (expert)). **g**, Proportion of inferentially robust studies by the acceptability of the analysis pipelines according to the peer evaluators. For this panel, we included only studies with more than one peer evaluation and in which the peer evaluators agreed on their rating. This panel shows only the rating options with five or more reanalyses in that category. **h**, Proportion of analyses showing the same conclusion, no effect/inconclusive results and opposite effect by declared familiarity with the study. **i**, Distribution of sample size for reanalyses resulting in the same conclusion, no effect/inconclusive results and opposite effects. Sample size values were available for 345 reanalyses. **j**, Percentage of studies with robust conclusions above different levels of reanalysis consensus. Reanalysis consensus refers to the agreement among the conclusions drawn by the original study and the independent reanalyses.

We have presented some exploratory analyses, but there are many other factors to explore that could contribute to analytical variability (such as topical expertise). Finally, despite our best efforts to conduct quality checks on the reanalyses to ensure the soundness of the analytical strategies[16], it is possible that some of the discrepancies between the original and new results are attributable to weaknesses in the reanalysts' approach rather than to equally justifiable alternative analysis decisions. It is likewise possible that there are weaknesses in the original analysts' approaches. It is unknown whether the quality control processes for the reanalysts resulted in better, worse or similar overall quality of analysis decisions as compared with the quality control processes for the original analysts' decisions. The declared statistical expertise of the reanalysts makes us believe that the observed heterogeneity in analyses and outcomes is a good representation of variation in informed analysis decision-making in social–behavioural research.

Discussion

Are published results in the social and behavioural sciences robust to independent reanalyses? The present exploration shows considerable variability owing to researcher degrees of freedom in statistical choices. Overall, when independent researchers analysed the same research question on the original data, 34% of studies remained robust to independent reanalysis in the sense that all reanalysts arrived at the same conclusion as the original analyst or analyst team. Notably, the new conclusions converged with the original ones in 74% of the individual reanalyses. Our descriptive results indicate a number of hypotheses concerning the circumstances in which we could expect greater analytical variability.

Why there can be several answers

Faced with the variability in the analysts' effect-size estimates and conclusions, one intuitive hypothesis is that the variation must be attributable to researcher characteristics, such as statistical or field-specific knowledge. Previous multi-analyst studies found little to no effect of researcher-specific characteristics, such as experience in the field or statistical expertise[16,19,27]. Instead, they suggest that analytical results are dependent on the particular choices that the analysts make among similarly acceptable data processing and analysis choices[27]. For example, when 46 independent analyst teams analysed the same speech dataset to answer the same research question, the authors concluded that "depending on the choice of how the speech signal is operationalised, researchers might find evidence for or against a theoretically relevant prediction" (p. 21)[27].

In line with previous findings, our results showed no strikingly different patterns across self-reported statistical expertise and experience in a matching field (Fig. 2f, Extended Data Fig. 4 and Supplementary Tables 5–9). At the same time, a few analysts who reported being familiar with the original article produced alternative results and conclusions at a comparable rate. Our peer evaluation process did not indicate that the analytical variability of the reanalyses was attributable to inadequate statistical practices. These results are in line with those of Menkveld et al.[22], in which the quality assessment of the proposed analysis pipelines did not statistically explain the results.

Another line of thought would suggest that the lack of robustness in the original published results reflects some conceptual ambiguity in the theories or methodology[30]. Research hypotheses are often short verbal expressions that do not force the specifications of the analyses. The underspecification of claims[31] could represent a main source of ambiguity in analytical decisions. We could not test the role of hypothesis ambiguity in a controlled manner, but it is a plausible contributor, considering that social science theories often make general claims across many variables, creating theory-laden choice points about how constructs are operationalized and how hypotheses are tested[32].

In terms of methodology, we examined our results by separating them by experimental and observational study designs. We observed that the proportions of results and conclusions that were analytically robust were 15–20% higher in the experimental studies. The estimated range of effect sizes was also apparently wider for observational studies than experimental studies. This exploratory finding motivates the hypothesis that the increased control over data collection circumstances and the reduced number of variables in experimental versus observational research translate to more limited analytical flexibility. However, substantial statistical variability remained among the findings from experimental studies.

Why these findings matter

In cases in which several acceptable analytical paths exist, researchers can use this freedom opportunistically[33,34] and bias the results towards desired findings ('myside bias'[35]). The much-discussed credibility challenges in the social and behavioural sciences stem partly from the suspicion that the prevailing incentive systems for publication encourage researchers to report and interpret empirical data to serve non-epistemic goals, such as storytelling[36]. Reform initiatives, such as the preregistration of research and analysis plans, aim to decrease researcher degrees of freedom to tweak the analytical method or the research question to the observed data. Would results in these fields become markedly more credible if every study was preregistered? Because preregistration is a protection against overfitting, we suggest that it would reduce or eliminate the observed finding that original analyses showed stronger evidence for positive results than reanalyses. However, we also suggest that preregistration would have little impact on the observed heterogeneity across alternative analysis strategies, because registering and following a single analytical path constrains the analysts only from choosing opportunistically from the alternative analytical paths. Still, it does not confer any unique statistical or epistemic status to the preselected analytical path[26]. Unexplored but alternative justifiable analyses applied to the same data could still lead to very different results. This exploration is clear about the presence of this variability in approaches, results and inferences in the social and behavioural sciences. Without exploring this variability, authors cannot guarantee consumers of their research that the reported conclusions hold a privileged status over alternative conclusions.

What we can do

The outcomes of this project suggest that the empirical answers to research questions in the social and behavioural sciences depend on the analytical paths taken to pursue them. Therefore, we advocate for the broader adoption of approaches that explore, recognize and address the uncertainty created by analytical variability.

Two main types of solution are (1) multi-analyst studies, such as our own, in which several investigators independently follow their own approach; and (2) the multiverse[2,11,37] approach, in which one investigator or team performs numerous analyses across a set of reasonable pipelines. Conducting exploratory studies to identify analytical uncertainties and holding out samples are further advisable practices to tackle analytical variability.

Project leaders aiming to conduct multi-analyst studies can consult various tutorial papers and guidelines. Aczel et al.[38] provide an expert consensus guideline on the entire life cycle of multi-analyst projects, from recruiting suitable analysts through conducting the project to the reporting of the outcomes. Kümpel and Hoffmann[29] offer a framework for synthesizing objective outcome metrics. The Subjective Evidence Evaluation Survey[39] is a tool for systematically exploring and quantifying subjective measures of evidence in multi-analyst studies, allowing analysis teams to subjectively reflect on various aspects of evidence, such as coherence, robustness and relevance, as well as the quality of the research design and data.

Multiverse analysis is also useful, especially when the dataset cannot be shared with other research groups for confidentiality reasons or when there are insufficient human resources to recruit several independent analysts. Several guideline papers help researchers to conduct and interpret such analyses[2,37,40–42].

Recently, many scholars have called for a stronger focus on replication in science[43]. Similar to preregistration, however, replications are unlikely to help address the robustness of results to several analysis strategies, because they intentionally repeat the same (or at least a very similar) analysis path. In this sense, replications can help to detect bodies of work in which authors may have leveraged their researcher degrees of freedom to generate results that are in line with their own or the journal's expectations. All other things being equal, a severely *P*-hacked literature should contain fewer replicable findings. However, replicability does not eliminate analytical variability itself. Nevertheless, having several studies creates an opportunity to observe whether analytical variability is itself replicable. For example, imagine that study A provides evidence for a claim with analysis 1 but not with analysis 2. If several replications also find evidence for the claim with analysis 1 but not with analysis 2, then the analytical choices are directly implicated in how evidence for the phenomenon is observed. However, if it is random across replications whether analysis 1 or 2 provides evidence for the claim, then the implications of the analytical variability are very different. The combination of replications and robustness investigations will facilitate the advancement of stronger theoretical underpinnings of the topics of study and could reduce analytical variability in the long run by creating a more direct mapping between theory and measurement[11,30].

Overall, we argue that the scholarly communication system could foster more engagement with systematic and transparent robustness testing. As a starting point, the research data shared openly alongside codebooks and analysis scripts are a prerequisite for any assessment of analytical robustness. Research findings of particular scientific or societal importance could be accompanied by robustness reports[44] that summarize the results of alternative theory-motivated analytical choices by independent analysts. This publication format already provides a platform for analysts to scrutinize the fragility of the findings before they have a major impact on scholarship and policy (see https://scipost-staging.org/JRobustRep).

What we learned about robustness

Our results support the view that results in social and behavioural science studies are contingent on the analysts' choices, and if the analysts report a single result from a single analytical path, they have not exhausted the possible answers that the dataset can provide. This finding aligns with the conclusions drawn by Wagenmakers et al.[4] that the belief that "for any dataset, there exists a single, uniquely appropriate analysis procedure" and "multiple plausible analyses would reliably yield similar conclusions" (p. 424) are no more than statistical myths. Without multi-analyst and multiverse approaches, the fragility of empirical findings remains.

Nonetheless, we emphasize that an optimistic or pessimistic interpretation is a matter of perspective and greatly depends on what evidential support we expect from a given study. Therefore, whether a result is satisfyingly robust will always depend on our epistemic needs and the precision we expect from our results. We caution against using blanket rules in aggregating or interpreting results across different analytical approaches within the same investigation.

Objectivity is a fundamental ideal of science, implying that claims about the world should not be contingent on the predispositions of the claimant. What our results reveal is not that we must distrust or reject the results of the past, including the studies we analysed. Instead, they suggest that we should adopt greater caution about the evidence that single analytical paths can offer to support social and behavioural science claims. We believe that the limitations of 'single-shot' analyses cut across numerous scientific disciplines. Methodological innovations, such as multi-laboratory collaborations, multi-analyst approaches and multiverse methods, could increase the robustness of the social and behavioural sciences and, perhaps more broadly, other empirical fields.

Online content

Any methods, additional references, Nature Portfolio reporting summaries, source data, extended data, supplementary information, acknowledgements, peer review information; details of author contributions and competing interests; and statements of data and code availability are available at https://doi.org/10.1038/s41586-025-09844-9.

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Methods

All methods and procedures in this study were reviewed by a panel of experts with previous experience in multi-analyst studies or who are specialists in the relevant methodology (see Supplementary Information for further methodological details).

Preregistration

The methods, materials, analysis plan, peer evaluation and data management strategy of the project were preregistered on the OSF. Deviations from the registered plan are reported and explained in 'Deviations from preregistration' in the Supplementary Information.

Ethical considerations. The datasets resulting from this project were not considered human subject research and are covered under an umbrella ethics protocol that was managed by the Center for Open Science (BRANY SBER IRB protocol no. 21-056-749), with concurrence from the US Naval Information Warfare Center Pacific, Human Research Protection Official. The institutional ethics board of the Faculty of Education and Psychology at Eötvös Loránd University determined that the reanalysts are not considered research participants and that the project raises no ethical concerns.

Materials

Selection of studies. The selection of studies was completed in two stages. In the first stage, the Systematizing Confidence in Open Research and Evidence (SCORE) team created an initial study and claim collection. From this collection, we selected our sample using further criteria.

In the SCORE project, a stratified random sample of 600 articles was identified from a larger pool of approximately 30,000 randomly stratified articles across 62 journals published between 2009 and 2018. The journals covered the main branches of social and behavioural sciences (criminology, economics, education, health-related, marketing/organizational behaviour, management, political science, psychology, public administration and sociology). To obtain the original studies, the following steps were taken: first, each paper was reviewed. If data and/or code were available, they were downloaded and saved into a project on OSF. If data and/or code were not available, the SCORE team attempted to contact the corresponding authors to request that they share the data and code used for the original publication. Studies were excluded from the sample if they did not contain at least one inferential test using non-simulated human data, in which human data are defined at any level of human organization (such as the individual person, family, political entity, firm and economic unit). Most of the studies were tested for analytical reproducibility using the original specification, which is to be distinguished from robustness to alternative specifications. Analytical reproducibility was tested in cases when both original data and code were available ($n = 63$) or when the original data were available but the original code had to be adapted by the SCORE team to successfully reproduce the result ($n = 7$). If data were available but the original code was not, SCORE sourced a collaborating laboratory to generate a new analytical code for the reproduction ($n = 10$). If data and code were not available, the collaborating laboratory used the secondary source data, which were shared upon request (acquired by SCORE), alongside the newly generated analytical code for the reproduction ($n = 11$). Some reproductions were not attempted ($n = 9$). If the analytical reproduction failed, the paper was removed from the pool. Therefore, the present project focused solely on robustness to alternative specifications and did not conduct direct reproducibility checks using the original specification, because these had already been carried out by SCORE. Further details of the SCORE methodology (list of journals, selection process and so on) are available in the original report (A. L. Abatayo et al., manuscript in preparation).

In this study, a further requirement of the selected studies was to contain a single inferential statistical test result that corresponded to the claim with our instructions. Thus, we ensured that given the claim and the instructions, no other statistical result could correspond to the claim in the original article. If all potential claims from the study were too ambiguous and, therefore, could not be linked with a single inferential test statistic with the specification instructions, the study was excluded from our sample. The aforementioned study selection process was continued until we reached our target number of 100 studies, corresponding claims and datasets.

The selected studies and all available corresponding data and materials were made available to the reanalysts so that they could fully understand the selected claim and approach. There are trade-offs for how much information to give to the reanalysts to conduct reanalyses. Complete blinding of the original analysis strategy would ensure an entirely independent decision-making process about how to analyse the data. However, in much scientific writing, there is insufficient clarity in the description of the theoretical background, rationale and specification of the conceptual model to be tested. In some papers, there is a clean break between these and clear hypotheses to test. In other papers, the narrative intermixes theoretical statements and analysis decisions and may not clearly state hypotheses or how they correspond with observed results. As a consequence, attempts to blind papers inevitably lead to variation in what is blinded across papers and many subjective decisions about what should be blinded (because it provides information about analysis strategy) and what can remain unblinded (because it provides information about theory and rationale). A major risk of those blinding decisions is that important information could be removed, which would weaken the reanalysts' ability to conduct a fair reanalysis of the original claim. As such, we opted for complete transparency of the original article so that no potentially important information was missing for the reanalysts, and we instructed reanalysts that they should create an analysis plan on the basis of their own decisions for how best to assess the study's claim. On balance, this increases the risk of dependent decision-making but reduces the risk of misspecification of the hypothesis and rationale of the original research. In this context, we judged the latter to be a more important precondition for conducting an informative study.

Claim selection. Claim selection was built on phase 1 of the SCORE project effort. The claims identified for phase 1 of SCORE were executed according to a 'single trace' approach, in which only a single claim trace was extracted from the article, which corresponded to one statistically significant inferential test result[45]. Within the current project, first, the lead team ensured that the extractions were understandable, contained only one claim and indicated the direction of the effect; a statistical hypothesis test-based result was provided in the article, which corresponded to the claim; and the claim was phrased on a conceptual rather than statistical level. If not, they extracted the part of the claim that was relevant; if this could not be achieved, they selected another more suitable sentence from the abstract, and if this could not be achieved, they searched for another suitable sentence from other parts of the article that could satisfy all of our criteria. When none of these steps presented a claim that satisfied the expectations, the article was excluded from our study (for the list and explanation of exclusions, see Supplementary Information). When the lead team judged an expression of a claim to be ambiguous or rhetorical, they replaced the expression with an ellipsis mark (for example, 'dramatically increased' to '…increased') while preserving the original wording and meaning of the claim. Only when the selection rendered the wording of the claim complicated, ungrammatical or marked by an ambiguous definition or unexplained abbreviation did the core team make necessary (and marked) adjustments in the grammar or wording of the claim while preserving the original meaning of the extraction. For example, the selection "Three factors increase the salience of the proliferation threat: (1) prior violent militarized conflict…" was changed to "[prior violent militarized conflict] increase[s] the salience of the proliferation threat…". The list of claims can be found at https://osf.io/mkwhn.

Analysis instructions. For the reanalysts' second task, instructions were needed in cases in which the original paper contained more than one statistical analysis corresponding to the high-level claim to compare the new result to that in the original paper. For this, the lead team prepared certain instructions (for example, data selection and exclusions) that singled out only one statistical result in the original paper. The instructions always remained circumstantial (for example, data selection, exclusions and choice of measurement) and never gave direct instructions as to the choice of statistical approach or full specification of the model.

Procedures

Reanalyst recruitment. Our preregistered aim was to have at least five independent reanalyses carried out for each of the 100 selected studies (Extended Data Fig. 5). Our choice of five analyses per study was guided by practical considerations, because we judged that recruiting 500 analysts for a project was the limit of our capacity.

Participation in the project was advertised on social media, at conferences, in mailing lists (such as SCORE collaborator list), through personal networks and in research newsletters. As a response to our recruitment call, 1,141 researchers signed up to participate in our study. Of these volunteers, 459 signed up to analyse at least one dataset and submitted their work by the deadline or an extended deadline. From all the eligible volunteers, we selected reanalysts and peer evaluators on a first-come first-served basis. The expectation for participation in the study was experience in conducting statistical analyses, and this was communicated to the volunteers from the start of recruitment. Reanalysts were informed that they would qualify as authors on the publication of this study if (1) they completed their analyses and submitted all required materials and post-analysis surveys on time; (2) their analyses passed the peer evaluation; and (3) they reviewed and approved the manuscript in time.

Reanalysts received a flat fee of US$100 for each of their completed reanalyses (including tasks 1 and 2) if they submitted their work before March 2023, the deadline of the grant budget, unless they were from an embargoed country, in which case we were unable to transfer any payment. Peer evaluators received a flat fee of US$10 per peer evaluation. Any further volunteers were informed that this payment did not apply to them.

Upon joining the project, the volunteers for reanalysis were required to accept the project requirements. They were informed about (1) their tasks and responsibilities; (2) the project confidentiality agreements; (3) the plans for publishing the research report and presenting the data, analyses and conclusion; (4) the conditions for an analysis to be included or excluded from the study; (5) that their names will be publicly linked to the analyses; (6) the reanalysts' rights to update or revise their analyses; (7) the project time schedule; and (8) the nature and criteria of compensation. Reanalysts were informed that, although they could consult other researchers during their analyses, they could not work in teams within this project. Before discussing the details of the analyses with others, the reanalysts were asked to ascertain that the person was not another reanalyst on that dataset. All communication materials of this study are openly available on the public repository of the project at https://osf.io/q5h2c/.

Assignment of analyses and tasks. The following procedure was first piloted with two analysts to learn about the practical challenges and time demands of the following tasks. The results of these analyses were not of central interest; therefore, we kept no records of them.

First, each reanalyst was asked to assign themselves to one study, but at later rounds of recruitment, we allowed reanalysts to complete analysis on another paper other than the one they completed earlier. They were asked to choose studies in which they saw the greatest relevance of their expertise. The authors of the original study could not be the reanalysts of that study.

For several practical reasons, the reanalyses were not started at the same time for each study and each analyst. First, it took us several rounds of recruitment to gather the target number of analyses for each study, mainly owing to dropouts, delays, unplanned personal difficulties and a shortage of staff. Second, our analysts found it difficult to retrieve, open or interpret some of the datasets. In some cases, we had to reach out to the original authors, causing further delays in the project.

The task of the reanalysts was to reflect on the corresponding claim (see claim selection) by reanalysing the corresponding data. The reanalysts were provided with access to the datasets, extracted claims, the original articles and all the corresponding materials. They were informed that their analyses should be conducted preferably with scripts that could reproduce all their results (including data preprocessing, extraction of test statistics and *P* values/Bayes factors and computing effect-size measures), but they could use the statistical software of their choice to produce an analysis script. Reanalysts were asked to write and structure their code such that others could understand their analysis scripts (for example, by annotating the different analysis steps), and they were also informed that the analysis scripts from all analysts would be made publicly available with their names linked to the analyses.

Reanalysts received two main tasks for each study, in which task 2 was given after the completion of task 1. Once task 1 was submitted, the analysts could not change the submission of task 1 unless they were asked by the lead team to provide some missing information from their analysis.

In task 1, the reanalysts were asked to reflect on the selected claim by reanalysing the corresponding data. They could conduct and report as many analyses as they wished, but they had to draw a single conclusion from their analysis. They were asked to report their analyses and indicate whether their results provided evidence for the relationship/effect as claimed by the original study.

In task 2, the reanalysts had to produce only one statistical result corresponding to the claim they studied in task 1, which would be compared with a statistical result in the original paper. The lead team provided certain instructions (for example, data selection and exclusions) for this analysis to be able to compare the new result with one result in the original paper (see 'Analysis instructions' section). Reanalysts were asked to report their results in terms of statistical families of *r*, *z*-test, *t*-test, *F*-test or χ^2 test (or their non-parametric versions). In addition, they were asked to report sample sizes (for example, per group) and the corresponding degrees of freedom. By this means, most results could be translated into standardized coefficients by the coordinators.

The reason for requiring two analyses from the reanalysts was that they served two different aims. The results of task 1 aimed to answer our first preregistered project question ('Do different analysts arrive at the same conclusions as the analyst of the original study?'), whereas the results of task 2 aimed to answer our second preregistered project question ('Do different analysts arrive at the same effect estimates as the analyst of the original study?'). We found that asking only one of the tasks would not have been sufficient to fully address both questions. In task 1, researchers were not constrained to one analysis; therefore, they could have produced more than one statistical result to draw a conclusion from the dataset. Therefore, in task 1, it was not guaranteed that we would be able to select a single effect size from each analyst to answer our second project question. Another challenge in answering our second question was that in some of the original articles, one claim could have had more than one corresponding statistical result listed. In these cases, we prepared instructions for task 2 to single out only one statistical result in the original paper. For example, if the original study contained two corresponding regression models (one with some exclusions and one with no exclusions), we chose one of them (for example, the latter) and instructed the reanalysts not to apply any exclusions to the analysed data. In all other regards, the reanalysts were free to conduct their calculations according to their best judgement.

After completing the analysis and writing up the methods, results and conclusion, the reanalysts were expected to upload their analysis code (if available) to the corresponding OSF folder. Their reported methods, results and conclusions were collected through an online form (https://osf.io/q5h2c/). When uploading the materials, they were also asked to fill out a post-analysis survey. All major communications between the core project team and the reanalysts from the study are openly available on the public repository of the project.

Peer evaluations

The goal of peer evaluation in this project was to assess whether the applied analytical choices were acceptable and whether the reported conclusion followed from the statistical results. By acceptable, we mean that peer evaluators agree that the analysis pipeline is within the variations that could be considered appropriate by the scientific community in addressing the given analytical task.

The peer evaluation phase did not address potential errors in translating the description of the analytical methodology into analysis scripts. To mitigate potential gross errors in the analysis, the peer evaluators were provided with a thorough and standardized description of the results and conclusions obtained using the described analysis, including sample sizes, effect size, test statistic and degrees of freedom. From descriptions of the dataset and analysis and the reported results and conclusions, the peer evaluators were able to identify potential flaws in the implementation of the analysis that could stem from errors and/or mismatches.

Assignment of the analyses. When assigning the volunteer peer evaluators to analyses, the initial rule was that they should not evaluate any reanalyses conducted on datasets they had reanalysed as a reanalyst. In practice, for logistical reasons, this rule was applied in all but six cases (99% of peer evaluations were carried out on a dataset that was different from the dataset they analysed themselves). They were asked to evaluate the analyses in which they saw the greatest relevance to their expertise. Peer evaluators who, after choosing a study, did not feel adequately skilled or experienced to judge whether the proposed analysis was acceptable were told to leave the template blank, return the reanalysis to the pool and choose another study.

Peer evaluation procedure. For details, see the corresponding section in the Supplementary Information.

Analysis methods

This exploratory study contains no inferential statistics. In addition to the frequency-based and proportion-based summary statistics, we calculated only the effect sizes of the results from the original articles and reanalyses.

Cohen's *d* effect sizes. Following our preregistration, we converted all results into Cohen's *d* values wherever possible. For a number of cases, we could not achieve this owing to missing information in the original studies or reported statistics that cannot be converted into Cohen's *d* (for example, logistic regression). All conversions are listed in the R scripts and data documentation. All original effect sizes are reported as positive values, whereas the reanalysis effect sizes are negative only when they indicate an opposite effect compared with the original study.

For further information on methods, see Supplementary Information.

Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

Data availability

Study data and materials are available on the project OSF (https://osf.io/q5h2c/) and GitHub repositories (https://github.com/marton-balazs-kovacs/multi100/). Archived data include the original datasets or a description of how to gain access to them. Our shared materials include all the survey questions and the general communication texts and instructions that we sent to the reanalysts and peer evaluators. We excluded from our data files the email addresses of the reanalysts and the records of those analysts who did not comply with the instructions and did not submit all the required analyses by the deadline. For further details about our exclusion criteria and procedure, see the Supplementary Information.

Code availability

All analysis codes for this project are available at https://github.com/marton-balazs-kovacs/multi100.

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Additional information

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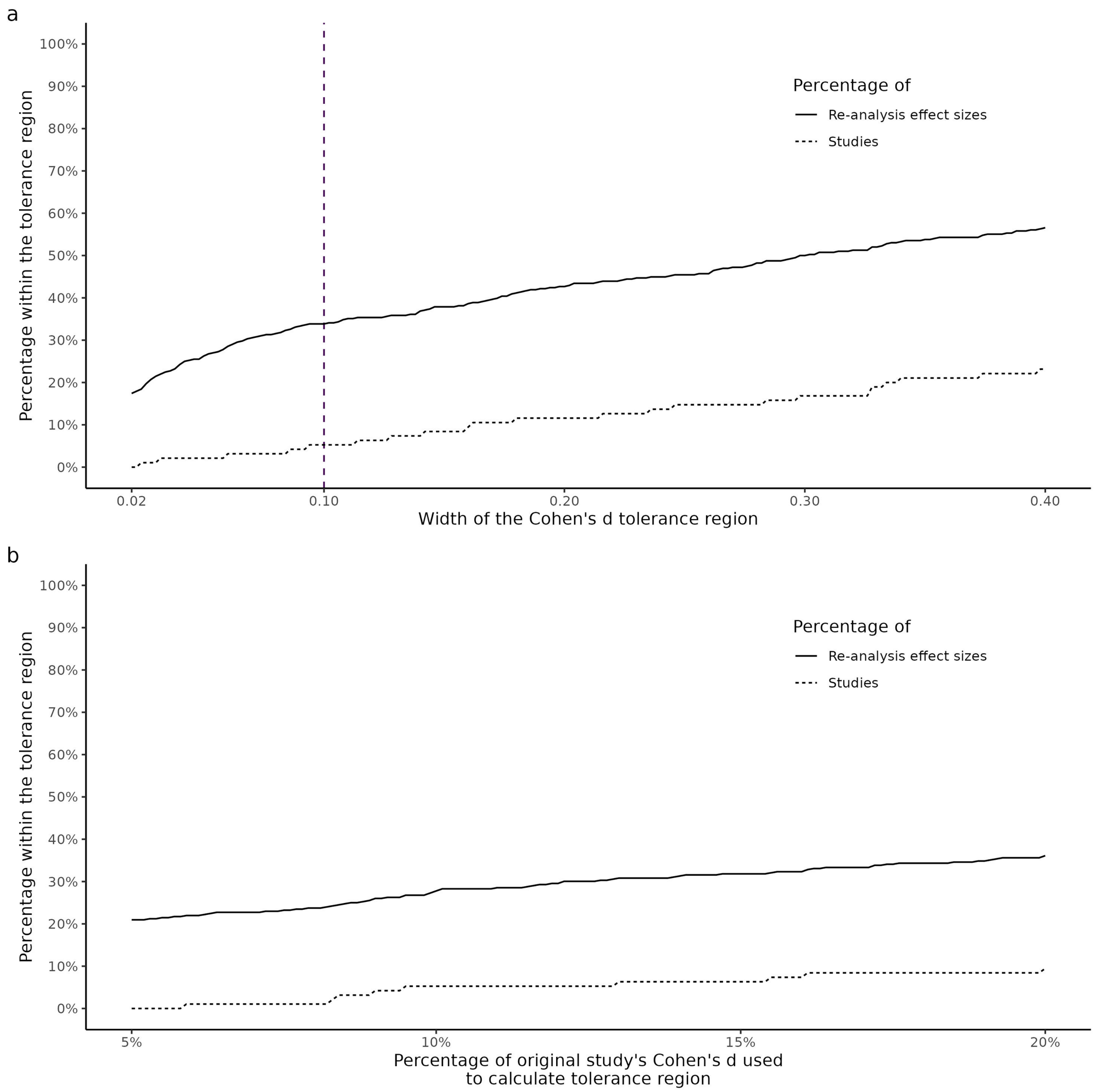
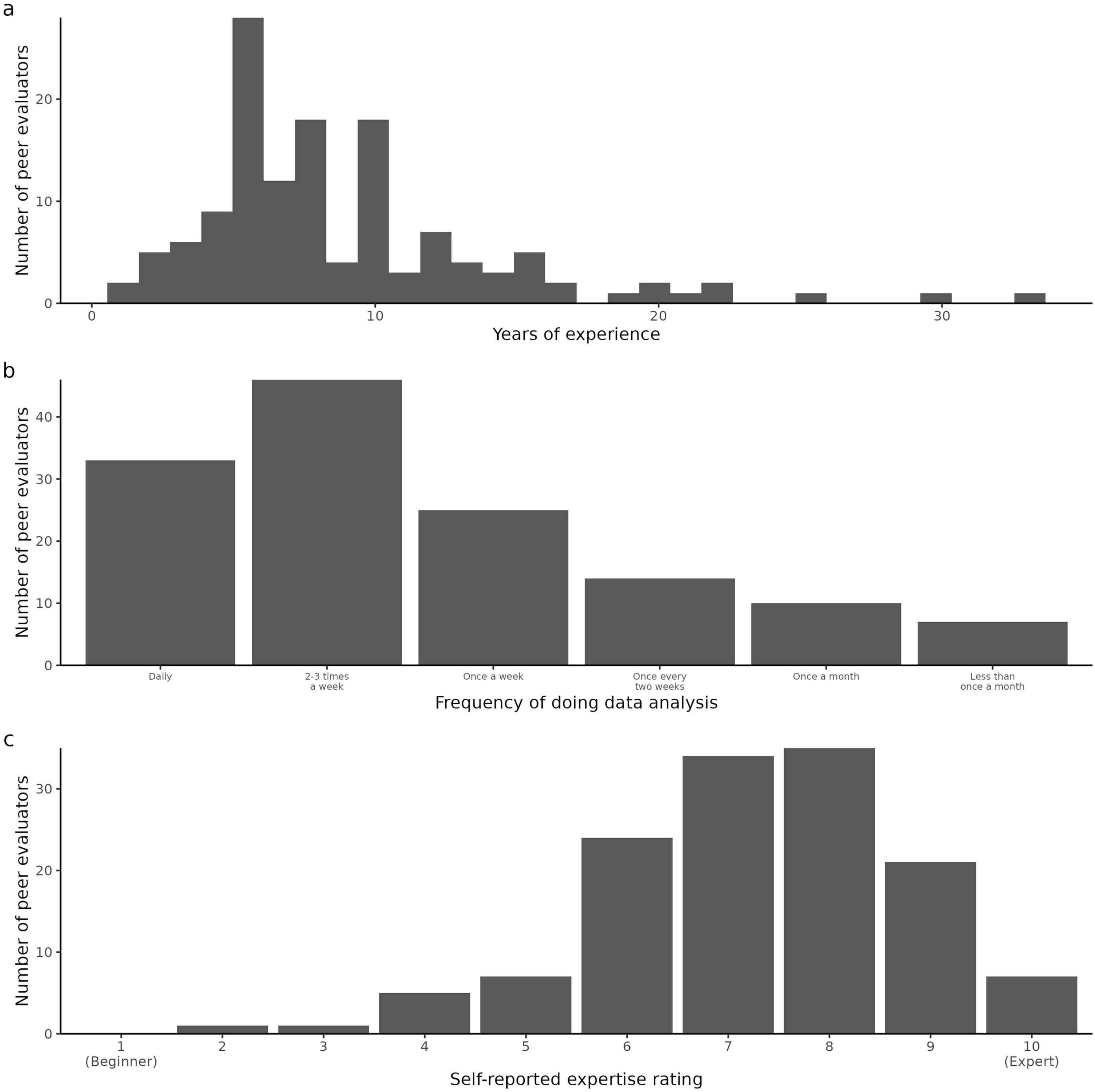
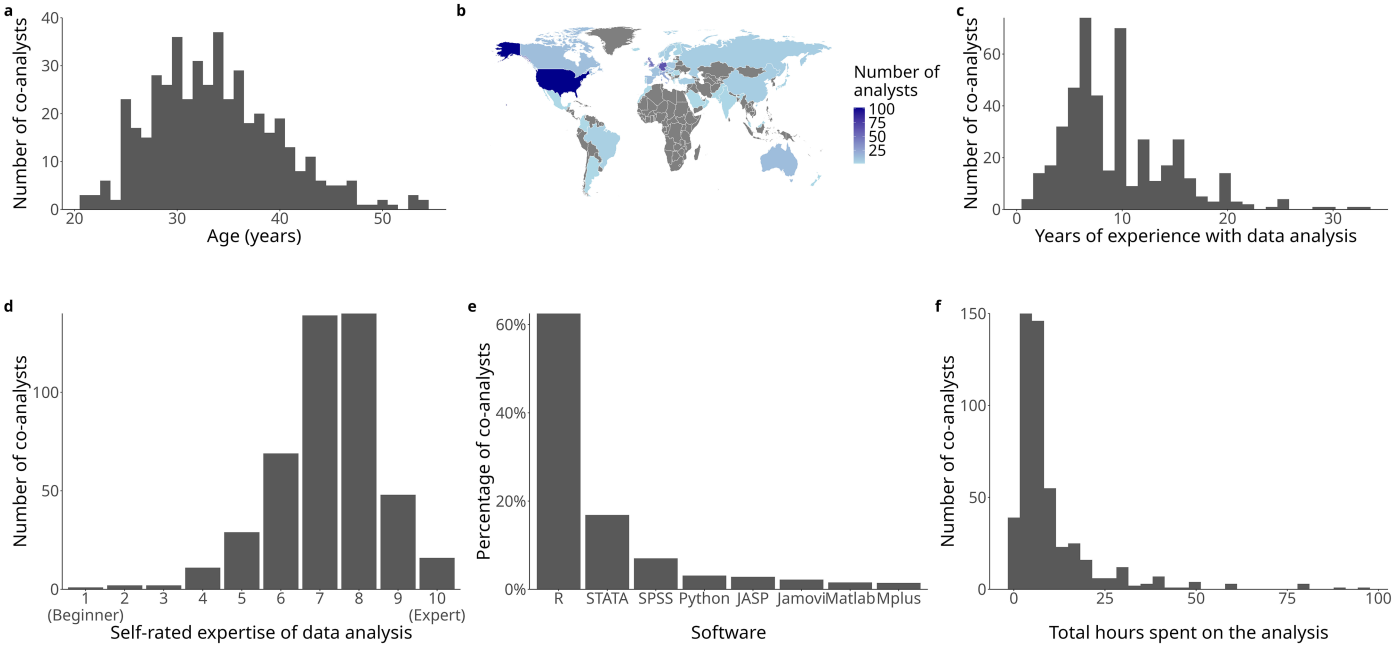


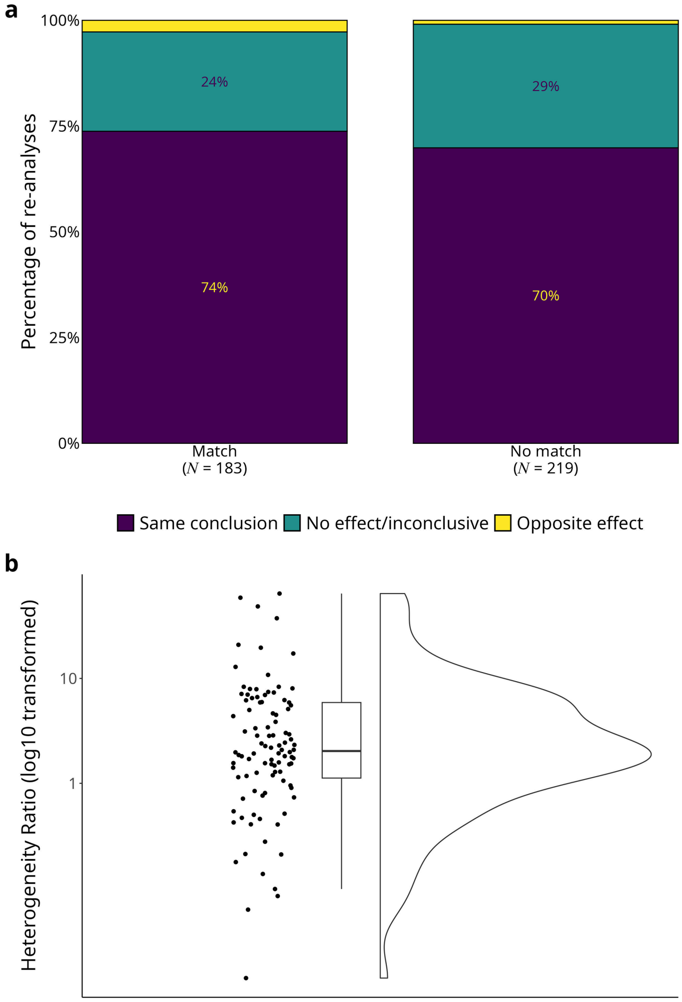
Extended Data Fig. 1 | Additional statistical results requested by the reviewers. a, Proportion of same effect, no effect/inconclusive results, and conclusions in the opposite direction of the original studies, by matches and nonmatches between the discipline of the re-analyst and the original study. **b**, The distribution of the heterogeneity ratios calculated between the effect size variability over the re-analyses and the sampling variability of the original study effect-size estimates.



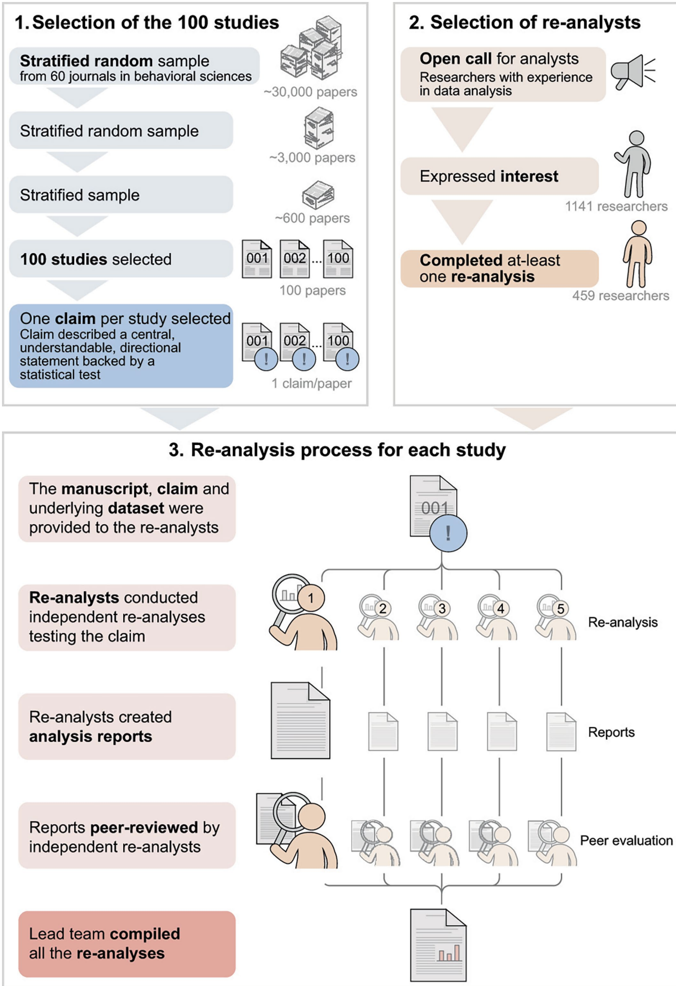
Extended Data Fig. 2 | Descriptive statistics of the peer evaluators. a, The peer evaluators' years of experience with data analysis. When a peer evaluator submitted more than one evaluation and a year passed between the responses, we kept only their first response. **b**, The regularity with which peer evaluators perform data analysis. **c**, The peer evaluators' self-rated level of expertise in data analysis. When a peer evaluator submitted more than one re-analysis, we kept only their first response.



Extended Data Fig. 3 | Descriptive statistics of the analysts and the analyses. **a**, The distribution of the analysts' age. When an analyst submitted additional re-analyses with a higher reported age, we kept only their age at the time of their first submission. Moreover, one analyst is not represented in the figure because they did not disclose their age. **b**, The analysts' country of residence. When an analyst submitted more than one re-analysis, and they moved between the submissions, we only kept their first response. **c**, The analysts' years of experience with data analysis. We only kept their first response when an analyst submitted additional re-analyses with a higher reported age. **d**, The regularity of the analysts' data analysis. **e**, The analysts' self-rated level of expertise in data analysis. When an analyst submitted more than one re-analysis, we only kept their first response. **f**, The software the analysts used for their re-analysis tasks. In case an analyst completed multiple re-analyses or reported using multiple software applications, we kept all their responses for this figure. The figure displays only software applications used by more than 1% of the analysts. In case an analyst completed multiple re-analyses, we retained all their responses for this figure. One response was excluded due to being an outlier (999 h), which we assumed was an error.



Extended Data Fig. 4 | Robustness of the statistical results. a, Robustness of the statistical results with different widths (+/−[0.01-0.20] Cohen's *d*) of the tolerance region. **b**, Robustness of the statistical results with different percentages (5–20%) of Cohen's *d* as a tolerance region. Calculations on the study and re-analysis levels are shown in different lines.



Extended Data Fig. 5 | Overview of the project procedures. The figure depicts the procedural workflow of the selection of the studies (1); the selection of the re-analysts (2); and the re-analysis process for each study (3).

nature portfolio

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For all statistical analyses, confirm that the following items are present in the figure legend, table legend, main text, or Methods section.

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☒	☐	A description of all covariates tested
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☐	☒	A full description of the statistical parameters including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals)
☒	☐	For null hypothesis testing, the test statistic (e.g. *F*, *t*, *r*) with confidence intervals, effect sizes, degrees of freedom and *P* value noted *Give P values as exact values whenever suitable.*
☒	☐	For Bayesian analysis, information on the choice of priors and Markov chain Monte Carlo settings
☒	☐	For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes
☒	☐	Estimates of effect sizes (e.g. Cohen's *d*, Pearson's *r*), indicating how they were calculated

Our web collection on statistics for biologists contains articles on many of the points above.

Software and code

Policy information about availability of computer code

Data collection	No software was used
Data analysis	R 4.4.2

For manuscripts utilizing custom algorithms or software that are central to the research but not yet described in published literature, software must be made available to editors and reviewers. We strongly encourage code deposition in a community repository (e.g. GitHub). See the Nature Portfolio guidelines for submitting code & software for further information.

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All manuscripts must include a data availability statement. This statement should provide the following information, where applicable:

- Accession codes, unique identifiers, or web links for publicly available datasets
- A description of any restrictions on data availability
- For clinical datasets or third party data, please ensure that the statement adheres to our policy

Study data, and materials are available on the project OSF (https://osf.io/q5h2c/) and GitHub repositories (https://github.com/marton-balazs-kovacs/multi100/). Archived data include the original datasets or a description how to gain access to them. Our shared materials include all the survey questions, and the general communication texts and instructions that we sent to the re-analysts and peer-evaluators.

Research involving human participants, their data, or biological material

Policy information about studies with human participants or human data. See also policy information about sex, gender (identity/presentation), and sexual orientation and race, ethnicity and racism.

Reporting on sex and gender	No human participants were involved in this study.
Reporting on race, ethnicity, or other socially relevant groupings	*Please specify the socially constructed or socially relevant categorization variable(s) used in your manuscript and explain why they were used. Please note that such variables should not be used as proxies for other socially constructed/relevant variables (for example, race or ethnicity should not be used as a proxy for socioeconomic status).* *Provide clear definitions of the relevant terms used, how they were provided (by the participants/respondents, the researchers, or third parties), and the method(s) used to classify people into the different categories (e.g. self-report, census or administrative data, social media data, etc.)* *Please provide details about how you controlled for confounding variables in your analyses.*
Population characteristics	*Describe the covariate-relevant population characteristics of the human research participants (e.g. age, genotypic information, past and current diagnosis and treatment categories). If you filled out the behavioural & social sciences study design questions and have nothing to add here, write "See above."*
Recruitment	*Describe how participants were recruited. Outline any potential self-selection bias or other biases that may be present and how these are likely to impact results.*
Ethics oversight	*Identify the organization(s) that approved the study protocol.*

Note that full information on the approval of the study protocol must also be provided in the manuscript.

Field-specific reporting

Please select the one below that is the best fit for your research. If you are not sure, read the appropriate sections before making your selection.

☐ Life sciences ☒ Behavioural & social sciences ☐ Ecological, evolutionary & environmental sciences

For a reference copy of the document with all sections, see nature.com/documents/nr-reporting-summary-flat.pdf

Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

Study description	In this exploratory study, we investigated the degree to which research findings in the social and behavioural sciences are contingent on analysts' choices. Therefore, we did not collect new data, but reanalysed already published datasets.
Research sample	The reanalyses have been conducted on the data of a sample of 100 studies published between 2009 and 2018 in criminology, demography, economics and finance, management, marketing and organisational behaviour, political science, psychology, and sociology. Therefore, our study did not involve collecting primary data.
Sampling strategy	The reanalysed studies have been selected by a previous project in which a stratified random sample of 600 articles was identified from a larger pool of randomly stratified ~30,000 articles from 62 journals, published between 2009 and 2019. In the present work, a further requirement of the selected studies was to contain a single inferential statistical test result that corresponded to the claim with our instructions. Thus, we ensured that given the claim and the instructions, no other statistical result could correspond to the claim in the original article. If all potential claims from the study were too ambiguous, and, therefore, could not be linked with a single inferential test statistic with the specification instructions, the study was excluded from our sample. The above-described study selection process has been continued until we reached 100 for our target number of studies, corresponding claims and datasets.
Data collection	The study did not involve any collection of primary data, other than the descriptives of the coauthor analysts.
Timing	● Start of the project Feb 10, 2021 ● Recruitment of expert panel Feb 24, 2021 ● Start of re-analyst recruitment Jan 21, 2022 ● Start of re-analyses Nov 19, 2022 ● Completion of the empirical work Oct 22, 2024
Data exclusions	Out of the submitted analyses, one was omitted from the summary analysis as its analysis failed the peer evaluation, and an additional 4 analyses were excluded due to incomplete responses.
Non-participation	No participants were involved in the data analysis.
Randomization	*If participants were not allocated into experimental groups, state so OR describe how participants were allocated to groups, and if allocation was not random, describe how covariates were controlled.*

Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

Materials & experimental systems

n/a	Involved in the study
☒	☐ Antibodies
☒	☐ Eukaryotic cell lines
☒	☐ Palaeontology and archaeology
☒	☐ Animals and other organisms
☒	☐ Clinical data
☒	☐ Dual use research of concern
☒	☐ Plants

Methods

n/a	Involved in the study
☒	☐ ChIP-seq
☒	☐ Flow cytometry
☒	☐ MRI-based neuroimaging

Plants

Seed stocks	*Report on the source of all seed stocks or other plant material used. If applicable, state the seed stock centre and catalogue number. If plant specimens were collected from the field, describe the collection location, date and sampling procedures.*
Novel plant genotypes	*Describe the methods by which all novel plant genotypes were produced. This includes those generated by transgenic approaches, gene editing, chemical/radiation-based mutagenesis and hybridization. For transgenic lines, describe the transformation method, the number of independent lines analyzed and the generation upon which experiments were performed. For gene-edited lines, describe the editor used, the endogenous sequence targeted for editing, the targeting guide RNA sequence (if applicable) and how the editor was applied.*
Authentication	*Describe any authentication procedures for each seed stock used or novel genotype generated. Describe any experiments used to assess the effect of a mutation and, where applicable, how potential secondary effects (e.g. second site T-DNA insertions, mosiacism, off-target gene editing) were examined.*