

1 **Investigating the analytical robustness**
2 **of the social and behavioural sciences**
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4 Balazs Aczel^{1*}, Barnabas Szaszi^{2,1*}, Harry T. Clelland^{1,3}, Marton Kovacs^{4,1}, Felix
5 Holzmeister⁵, Don van Ravenzwaaij⁶, Hannah Schulz-Kümpel⁷, Sabine Hoffmann⁷,
6 Gustav Nilsson^{8,9}, Livia Kosa^{1,4}, Zoltan A. Torma¹, Yousuf Abdelfatah¹⁰, Christopher
7 L. Aberson¹¹, Oguz A. Acar¹², Ensar Acem¹³, Matus Adamkovic^{14,15,16}, Timofey
8 Adamovich¹⁷, Krisna Adiasto¹⁸, Love Ahnström⁸, Atakan M. Akil¹⁹, Adil S. Al-
9 Busaidi^{20,21}, Ali H. Al-Hoorie²², Casper J. Albers⁶, Peter J. Allen²³, Taym Alsalti²⁴, Micah
10 Altman²⁵, Shilaan Alzahawi²⁶, Ettore Ambrosini²⁷, Saule Anafinova²⁸, Rahul Anand²⁹,
11 Martin Angerer³⁰, Ariadna Angulo-Brunet³¹, Alberto Antonietti³², Jozsef Arato³³, Andreu
12 Arenas³⁴, Marco M. Aviña^{35,36}, Flavio Azevedo³⁷, Marko Bachl³⁸, Bence Bago³⁹, Štěpán
13 Bahník⁴⁰, Bradley J. Baker⁴¹, Elza Balayan⁵, Cassandra L. Baldwin⁴², Benjamin Banai⁴³,
14 Kasia Banas⁴⁴, František Bartoš⁴⁵, Ernest Baskin⁴⁶, Jojanneke A. Bastiaansen⁴⁷, Nadège
15 Bault⁴⁸, Christopher W. Bauman⁴⁹, Quintin H. Beazer⁵⁰, Maciej Behnke⁵¹, Theiss
16 Bendixen⁵², Sebastian Berger⁵³, Anna Bernard⁵⁴, Ursa Bernardic⁵⁵, Paul A. Bloom⁵⁶,
17 Annika Boldt⁵⁷, Ciril Bosch-Rosa⁵⁸, Rotem Botvinik-Nezer^{59,60}, Adam Bouyamourn⁶¹,
18 Ozge Bozkurt⁶², Laurel Brehm⁶³, Johannes Breuer⁶⁴, Ryan Briggs^{65,66}, Hilmar Brohmer⁶⁷,
19 Erin Buchanan⁶⁸, Johannes Buckenmaier⁶⁹, Jeffrey Buckley⁷⁰, Jacek Buczny^{71,72},
20 Matthias Burghart⁷³, Bilal H. Butt^{74,75}, Nick Byrd⁷⁶, Valentina Cafarelli⁷⁷, Patrick
21 Callahan⁷⁸, Tabaré Capitán⁷⁹, Kevin Carriere⁸⁰, Andrea M. Cataldo^{81,82}, Gabriel
22 Cepaluni^{83,84}, Eugene Chan⁸⁵, Jesse J. Chandler^{86,87}, Chia-chen Chang⁸⁸, Xi Chen^{89,90},
23 Shirley (Shuo) Chen⁹¹, Fadong Chen⁹², Hao Chen⁹³, Valerii Chirkov^{94,95}, Daniela
24 Cialfi^{96,97}, Beth Clarke⁹⁸, Sophie G. Coelho⁹⁹, Clara Cohen¹⁰⁰, Jason Collins¹⁰¹, Susan W.
25 Cook¹⁰², Gaia Corlazzoli¹⁰³, Jamie Cummins^{104,105}, Christian Czymara^{106,107}, Jonathan
26 D'hondt^{108,109}, Anna Dalla Rosa¹¹⁰, Abi M. B. Davis¹¹¹, Charles P. Davis¹¹², Martin V.
27 Day¹¹³, Freya De Keyzer¹¹⁴, Joshua R. de Leeuw¹¹⁵, Tjeerd Rudmer de Vries^{116,117}, Ramit
28 Debnath^{118,119}, Filip Dechterenko¹²⁰, Elif E. Demiral¹²¹, Marc Desgroseilliers¹²², Dominik
29 Dianovics^{4,123}, Veronica Diveica¹²⁴, Stephan Dochow-Sondershaus¹²⁵, Simone Dohle¹²⁶,
30 LiChen Dong¹²⁷, Jonas Dora¹²⁸, Angela R. Dorrough^{129,130}, Anna Dreber^{131,5}, Hongfei
31 Du^{132,133}, John E. Edlund¹³⁴, Anita Eerland¹⁸, Emir Efendic¹³⁵, Jacob Elder¹³⁶, Mahmoud
32 M. Elsherif^{137,138}, Mareike Ernst¹³⁹, Eduardo Estrada¹⁴⁰, Luis Eudave¹⁴¹, Thomas R.
33 Evans^{142,143}, Arodi Farrera¹⁴⁴, El Mehdi Ferrouhi¹⁴⁵, Lenka Fiala^{146,147,148}, Fabrício M.
34 Fialho^{149,150}, Josh L. Fiechter¹⁵¹, Miloš Fišar¹⁵², Pablo Ezequiel Flores-Kanter^{153,154},
35 Michał Folwarczny¹⁵⁵, Jessica L. Fossum¹⁵⁶, Vithor R. Franco¹⁵⁷, René Freichel¹⁵⁸,
36 Danilo Freire^{159,160}, Joris Frese¹⁶¹, Alexander C. Furnas^{162,163}, Johann D. Gaebler¹⁶⁴, Lisa
37 C. Gajary^{165,166}, Carl Michael Galang^{167,168}, Benjamin Ganschow¹⁶⁹, S. Mason
38 Garrison¹⁷⁰, Agata Gasiorowska¹⁷¹, Bruno Gasparotto Ponne¹⁷², Romain Gauriot¹⁷³, Alice
39 Geminiani^{174,175}, Diogo Geraldés^{176,177}, Morton Ann Gernsbacher¹²⁷, Cinzia Giani¹⁷⁸,
40 Enrico Glerean¹⁷⁹, Vukašin Gligorić¹⁸⁰, Timo Gnams¹⁸¹, Amélie Godefroidt^{182,183},
41 Bastián González-Bustamante^{184,185}, Andreas Goreis¹⁸⁶, Lorenz Graf-Vlachy^{187,188},
42 Manuel Grieder^{189,190}, Dmitry Grigoryev¹⁹¹, Sandra Grinschgl^{192,193}, David J. Grüning¹⁹⁴,

43 João F. Guassi Moreira^{195,196}, Clément Guichet¹⁹⁷, Lilas Gurgand¹⁹⁸, Hooman
44 Habibnia¹⁹⁹, Andrew C. Hafenbrack²⁰⁰, Sebastian Hafenbrädl²⁰¹, Carolin Häffner²⁰²,
45 Felix Hagemeister²⁰³, Matthew Haigh²⁰⁴, Nandor Hajdu¹, Narges Hajimoladarvish²⁰⁵,
46 Jonathan D. Hall²⁰⁶, Maik Hamjediers²⁰⁷, Robert M. Hardwick²⁰⁸, Mehmet Harma^{209,210},
47 Nicholas R. Harp²¹¹, Áron D. Hartvig^{212,213}, Raphael H. Heiberger²¹⁴, Arthur Heim^{215,216},
48 Øystein Hernaes²¹⁷, Dennis Hernaus²¹⁸, Tom Heyman²¹⁹, Joshua Hicks²²⁰, Jeremy
49 Hogeveen²²¹, Julia Höpler⁷, Sean Dae Houlihan^{222,223}, Christoph Huber²²⁴, Conor
50 Hughes²²⁵, Teresa Hummler²²⁶, Karoline Huth²²⁷, Moritz Ingendahl²²⁸, Tatsunori Ishii²²⁹,
51 Ozan Isler²³⁰, Kamil Izydorczak²³¹, Iain R. Jackson²³², Andrew Jahn²³³, Maitri Jain²³⁴,
52 Alexander Jakubow²³⁵, Daisung Jang²³⁶, JunHyeok Jang²³⁷, Marc Jekel²³⁸, Fanli Jia²³⁹,
53 William Jiménez-Leal²⁴⁰, Rebecca Johnson²⁴¹, Alex Jones²⁴², Sebastian Jungkuz^{243,244},
54 Pavol Kačmár²⁴⁵, Caspar Kaiser^{246,247}, Yağmur Kalaycı²⁴⁸, Jaroslaw Kantorowicz²⁴⁹, Anıl
55 Karabulut^{250,251}, Julian Karch²⁵², Hamid Karimi-Rouzbahani²⁵³, Johannes A. Karl^{254,255},
56 Austėja Kažemekaitytė²⁵⁶, Aliaksandr Kazlou²⁵⁷, Zoltan Kekecs¹, Jin Kim²⁵⁸, Michael H.
57 Kirchler²⁵⁹, Bence Kiss-Dobronyi²⁶⁰, Kai N. Klasmeier^{261,262}, Jack W. Klein^{263,264}, Cemal
58 Koba²⁶⁵, Marta Kołczyńska²⁶⁶, Pavlos Kolias²⁶⁷, Matěj Kolouch Grabovský²⁶⁸, Max
59 Korbmacher^{269,270,271}, Živa Korda⁶⁷, Marta Kowal²⁷², André Kretzschmar²⁷³, Vladislav
60 Krivoshchekov²⁷⁴, Angelos-Miltiadis Kryptos^{275,276}, Marcus Kubsch²⁷⁷, Yoshihiko
61 Kunisato²⁷⁸, David Lacko²⁷⁹, Jan R. Landwehr²⁸⁰, Martin Lange²⁸¹, Hongmi Lee²⁸²,
62 Daniel Lee²⁸³, Sangil Lee²⁸⁴, Edward P. Lemay, Jr.²⁸⁵, Daniel Lempert²⁸⁶, Andrea
63 Leo^{287,288}, Elise Lesage²⁸⁹, Joel M. Levin^{290,291}, Peng Li²⁹², Jing Lin²⁹³, Luke Lindsay²⁹⁴,
64 Daria Lisovoj²⁰², Meng Liu²⁹⁵, Sihong Liu²⁹⁶, Tingshu Liu²⁹⁷, Sergio Lo Iacono²⁹⁸, Paul
65 Lodder²⁹⁹, Rubén López-Bueno³⁰⁰, Ruben Lopez-Nicolas³⁰¹, Katharina Loter²⁹⁹, Nigel
66 Mantou Lou³⁰², Andrey Lovakov³⁰³, Jackson G. Lu³⁰⁴, Jonas Ludwig³⁰⁵, Finn
67 Luebber^{306,307}, Jiří Lukavský¹²⁰, Charles Luo³⁰⁸, Xuanyu Lyu³⁰⁹, Esther Maassen²⁹⁹,
68 Martin Máčel³¹⁰, Michael L. Mack³¹¹, Christopher R. Madan³¹², Andreas Mädebach³¹³,
69 Joseph Maffly-Kipp^{314,315}, Daniel J. Mallinson³¹⁶, Igor Marchetti³¹⁷, Tyler Marghetis³¹⁸,
70 Matteo M. Marini³¹⁹, Diego Marino Fages³²⁰, Mayte Martínez^{321,322}, Mario Martinoli³²³,
71 Aidas Masiliunas³²⁴, Sébastien Massoni³²⁵, Kaleb C. Mathieu³²⁶, Stefan Mayer³²⁷,
72 Duncan Mayer³²⁸, Maren Mayer³²⁹, Ethan M. McCormick^{330,331}, Ian M.
73 McDonough^{332,333}, Amanda L. McGowan^{334,335}, Miranda M. McIntyre³³⁶, Paul
74 McKee^{112,337}, Armando N. Meier^{338,339}, Pascal F. Meier³⁴⁰, Helena Melero³⁴¹, Christoph
75 Merkle^{342,343}, Raphael Merz²²⁸, Michalis P. Michaelides³⁴⁴, Patrik Michaelsen^{345,346},
76 Gosia Mikolajczak³⁴⁷, Wladislaw Mill³⁴⁸, Philip Millroth³⁴⁹, Kirill G. Miroshnik^{317,350},
77 Michal Misiak^{351,352}, Yuri L. Mora³⁵³, David Moreau^{354,355}, Chris Moreh^{356,357}, Coby
78 Morvinski³⁵⁸, Faisal Mushtaq^{359,360}, Tamás Nagy¹, Christa Nater³⁶¹, Elias Naumann³⁶²,
79 Gorka Navarrete³⁶³, Stephan Nebe³⁶⁴, Andre Nedderhoff³⁶⁵, Richard Nennstiel³⁶⁶, Martin
80 Neugebauer³⁶⁷, Eliana Nicolaisen-Sobesky^{368,369}, Yngwie A. Nielsen³⁷⁰, Guiomar
81 Niso³⁷¹, Benjamin Nowak³⁷², Mehmet Okan³⁷³, Kenneth Ong³⁷⁴, Adrian I. Onicas³⁷⁵,
82 Christian Oswald³⁷⁶, Kasper Otten³⁷⁷, Shubham Pandey^{378,379}, Myrto Pantazi^{380,381}, Paolo
83 Papale³⁸², Philip Pärnamets⁸, Shiva Pauer^{383,384}, Yuri G. Pavlov³⁸⁵, Samuel Pawel³⁸⁶,
84 Jonathan E. Peelle³⁸⁷, Hannah K. Peetz¹⁸, Anton Peez^{388,389}, Francesca Pesciarelli³⁹⁰,
85 Brenton D. Peterson³⁹¹, Benjamin Petruželka³⁹², Jonas Petter¹⁵⁸, Jan Pfänder³⁹³, Gerit

86 Pfuhl³⁹⁴, Joseph Phillips³⁹⁵, Matthew T. Pietryka³⁹⁶, Angelo Pirrone^{397,398}, Ilse L.
87 Pit^{399,400,401}, Anna Placht⁴⁰², Irene Sophia Plank⁴⁰³, Matteo Ploner²⁵⁶, Russell A.
88 Poldrack⁴⁰⁴, Monique M. H. Pollmann⁴⁰⁵, Simon Porcher⁴⁰⁶, Patrick Präg⁴⁰⁷, Andrew
89 Adrian Y. Pua^{408,409}, Jessica Pugel⁴¹⁰, Rohan Puri^{411,412}, Marcell Püski¹, Setayesh
90 Radkani⁴¹³, Louis Raes⁴¹⁴, Ismaël Rafai⁴¹⁵, Klara Raiber⁴¹⁶, Steve Rathje⁴¹⁷, Mikhail
91 Reshetnikov⁴¹⁸, Caleb J. Reynolds⁴¹⁹, James P. Reynolds⁴²⁰, Kévin Rigaud⁴²¹, Charlie
92 Rioux⁴²², Sebastian Rivera^{423,185}, Olly Robertson⁴²⁴, Rafael Román-Caballero^{425,426,427,428},
93 Raphael Rehms⁷⁸, Ivan Ropovik^{429,430}, Lukas Röseler^{431,244}, Robert M. Ross⁴³², Amanda
94 Rotella²⁰⁴, Franziska F. Ruffer⁴³³, Felix Rusche^{348,434}, Massimo Rusconi⁴³⁵, Irene
95 Russo⁴³⁶, Alexander H. J. Sahn⁴³⁷, Janos Salamon^{438,421}, Margaret Samahita¹⁷⁶, Ali
96 Sanaei⁴³⁹, Arshiya Sangchooli⁴⁴⁰, Alexandra Sarafoglou¹⁵⁸, Michele Scandola⁴⁴¹,
97 Henning Schaak⁴⁴², Michael Schaerer⁴⁴³, Eric Schares⁴⁴⁴, Hayden T. Schilling⁴⁴⁵, Xenia
98 Schmalz⁴⁴⁶, Kathleen Schmidt⁴⁴⁷, Tom Schonberg⁴⁴⁸, Marcel R. Schreiner^{449,450}, Joris M.
99 Schröder⁴⁵¹, Anna-Lena Schubert⁴⁵², Brendan Schuetze⁴⁵³, Douglas H. Schultz⁴⁵⁴, Lars
100 Schulze⁴⁵⁵, Shawn T. Schwartz^{404,456}, Nicole Schwitter^{457,458}, Bermond Scoggins⁴⁵⁹,
101 Yashvin Seetahul⁴⁶⁰, Raffaello Seri⁴³⁵, David R. Shanks⁴⁶¹, Stacy T. Shaw⁴⁶², Joseph
102 Shaw⁴⁶³, Qiang Shen⁴⁶⁴, Christoph Siemroth⁴⁶⁵, Martina Sladekova⁴⁶⁶, Angela Somo⁴⁶⁷,
103 Arjun Sondhi⁴⁶⁸, Burak Sonmez⁴⁶⁹, Lisa Spantig^{470,465}, Maarten Speekenbrink⁴⁷¹,
104 Angelos Stamos⁴⁷², Lukasz Stasielowicz⁴⁷³, Leonie C. Steckermeier^{474,475}, Simon R.
105 Steinkamp⁴⁰², Andrea H. Stoevenbelt⁴⁷⁶, Chris N. H. Street⁴⁷⁷, Jordan W. Suchow⁴⁷⁸,
106 Hans Fredrik Sunde⁴⁷⁹, James Sundquist^{480,481}, Vsevolod Sushevskiy^{482,483}, Scott D.
107 Swain⁴⁸⁴, Peter Szecsi^{4,1}, Raluca D. Szekely-Copindean^{485,486}, Ewa Szumowska⁴⁸⁷,
108 Alessandro Tacconelli⁴⁸⁸, Eli Talbert⁴⁸⁹, John P. Tang⁴⁹⁰, Jorge N. Tendeiro⁴⁹¹, Martina
109 Testori⁴⁹², Enrico Toffalini⁴⁹³, Aleksandar Tomašević⁴⁹⁴, Selin Topel^{252,495}, Lasse
110 Torkkeli^{496,497,498}, Leonardo Tozzi⁴⁹⁹, Jakub Traczyk¹⁷¹, Alexander Trinidad⁵⁰⁰, Darinka
111 Trübtschek^{501,502}, Konrad Turek⁵⁰³, Maximiliane Uhlich⁵⁰⁴, Eric L. Uhlmann⁵⁰⁵,
112 Karolina Urbanska⁵⁰⁶, Jasper Van Assche^{507,508}, Marcel A. L. M. van Assen^{433,509}, Noah
113 N. N. van Dongen¹⁵⁸, Kenny van Lieshout⁵¹⁰, Roel van Veldhuizen⁵¹¹, Marton A. Varga¹,
114 Leigh Ann Vaughn⁵¹², Fruzsina Venczel¹, Michela Vezzoli⁵¹³, Paul Vierus⁵¹⁴, Antonino
115 Visalli⁵¹⁵, Emily Voldal⁵¹⁶, Fabio Votta⁵¹⁷, Eric-Jan Wagenmakers¹⁵⁸, Anica
116 Waldendorf⁵¹⁸, Matthew J. Walker⁵¹⁹, Matthew B. Wall^{520,521}, Henri Wallen⁵²², Ke
117 Wang⁵²³, Iris Wang⁵²⁴, Y. Andre Wang³¹¹, Markus Weinmann⁵²⁵, Martin Weiss⁵²⁶,
118 Christian Westheide^{527,528}, Aaron Wichman^{529,530}, Juliane C. Wilcke⁷⁸, Benedict J.
119 Williams⁵³¹, David Wisniewski^{532,168,533}, Thomas K. A. Woiczuk⁵³⁴, Mateusz
120 Woźniak^{535,536}, Joshua D. Wright⁵³⁷, Wu Youyou⁵³⁸, Jesper N. Wulff³⁴², Tao Yang⁵³⁹, Siu
121 Kit Yeung⁵⁴⁰, Kenneth S. L. Yuen^{541,542}, Michał Zawistowski⁵⁴³, Rizqy A. Zein^{544,545},
122 Xian Zhao⁵⁴⁶, Zefan Zheng^{547,548}, Steven Zhou⁵⁴⁹, Conrad Ziller²²⁶, David Zimmerman⁵⁵⁰,
123 Cristina Zogmaister⁵⁵¹, Ro'i Zultan⁵⁵², Nicholas Fox⁵⁵³, Timothy M. Errington⁵⁵³, Brian
124 A. Nosek^{553,554}

125 ¹Institute of Psychology, ELTE Eötvös Loránd University, Budapest, Hungary, ²Institute of
126 Operations and Decision Sciences, Corvinus Institute for Advanced Studies (CIAS), Corvinus
127 University of Budapest, Budapest, Hungary, ³Department of Psychology, Edge Hill University,

128 Ormskirk, UK, ⁴Doctoral School of Psychology, ELTE Eötvös Loránd University, Budapest,
129 Hungary, ⁵Department of Economics, University of Innsbruck, Innsbruck, Austria, ⁶Department
130 of Psychometrics & Statistics, University of Groningen, Groningen, The Netherlands,
131 ⁷Department of Statistics, Ludwig Maximilian University Munich, Munich, Germany,
132 ⁸Department of Clinical Neuroscience, Karolinska Institutet, Stockholm, Sweden, ⁹Department
133 of Psychology, Stockholm University, Stockholm, Sweden, ¹⁰Department of Politics, Princeton
134 University, Princeton, NJ, USA, ¹¹The Dissertation Coach, Oak Park, CA, USA, ¹²King's
135 Business School, King's College London, London, United Kingdom, ¹³Department of
136 Psychology, Kadir Has University, Istanbul, Türkiye, ¹⁴Centre of Social and Psychological
137 Sciences, Slovak Academy of Sciences, Bratislava, Slovakia, ¹⁵Faculty of Humanities and Social
138 Sciences, University of Jyväskylä, Jyväskylä, Finland, ¹⁶Faculty of Education, Charles
139 University, Prague, Czechia, ¹⁷Behavioral Genetics Lab, Psychological Institute of the Russian
140 Academy of Education, Moscow, Russia, ¹⁸Behavioural Science Institute, Radboud University,
141 Nijmegen, The Netherlands, ¹⁹Institute of Psychology, Faculty of Humanities and Social
142 Sciences, University of Pécs, Pécs, Hungary, ²⁰Innovation & Technology Transfer Center, Sultan
143 Qaboos University, Al-Khoud, Oman, ²¹Business Communication Department, Sultan Qaboos
144 University, Oman, ²²Jubail English Language and Preparatory Year Institute, Royal Commission
145 for Jubail and Yanbu, Jubail Industrial City, Saudi Arabia, ²³School of Psychological Science,
146 University of Bristol, Bristol, UK, ²⁴Personality Psychology and Psychological Diagnostics,
147 University of Leipzig, Leipzig, Germany, ²⁵Center for Research on Equitable and Open
148 Scholarship, Massachusetts Institute of Technology, Cambridge, MA, USA, ²⁶Graduate School
149 of Business, Stanford University, Stanford, CA, USA, ²⁷Department of Neuroscience, University
150 of Padova, Padova, Italy, ²⁸Department of Technical Education, Faculty of Economic and Social
151 Sciences, Budapest University of Technology and Economics (BME), ²⁹Department of
152 Management, Aarhus BSS (Aarhus University), Aarhus, Denmark, ³⁰Liechtenstein Business
153 School, University of Liechtenstein, Vaduz, Liechtenstein, ³¹Faculty of Psychology and
154 Educational Sciences, Universitat Oberta de Catalunya, Barcelona, Spain, ³²Department of
155 Electronics, Information and Bioengineering, Politecnico di Milano, Milano, Italy, ³³Vienna
156 Cognitive Science Hub, University of Vienna, Vienna, Austria, ³⁴Institut d'Economia de
157 Barcelona, University of Barcelona, Barcelona, Spain, ³⁵Department of Government, Harvard
158 University, Cambridge, MA, USA, ³⁶James M. and Cathleen D. Stone Ph.D. Scholar in
159 Inequality and Wealth Concentration, ³⁷Department of Interdisciplinary Social Science,
160 University of Utrecht, Utrecht, The Netherlands, ³⁸Institute for Media and Communication
161 Studies, Freie Universität Berlin, Berlin, Germany, ³⁹Department of Social Psychology, Tilburg
162 University, Tilburg, The Netherlands, ⁴⁰Department of Management, Prague University of
163 Economics and Business, Faculty of Business Administration, Prague, Czech Republic,
164 ⁴¹Department of Sport, Tourism and Hospitality Management, Temple University, Philadelphia,
165 PA, USA, ⁴²Department of Psychology, Georgia Southern University, Statesboro, GA, USA,
166 ⁴³Banai analitika, Osijek, Croatia, ⁴⁴Usher Institute, University of Edinburgh, Edinburgh, UK,
167 ⁴⁵Department of Psychological Methods, University of Amsterdam, Amsterdam, The
168 Netherlands, ⁴⁶Department of Food, Pharma and Healthcare, Saint Joseph's University,
169 Philadelphia, PA, USA, ⁴⁷Department of Psychiatry, University of Groningen, University
170 Medical Center Groningen, Groningen, The Netherlands, ⁴⁸Brain Research & Imaging Centre,
171 School of Psychology, University of Plymouth, Plymouth, United Kingdom, ⁴⁹Paul Merage
172 School of Business, University of California Irvine, Irvine, CA, USA, ⁵⁰Department of Political
173 Science, Florida State University, Tallahassee, FL, USA, ⁵¹Cognitive Neuroscience Center,

174 Adam Mickiewicz University, Poznan, Poland, ⁵²School of Culture and Society, Aarhus
175 University, Aarhus, Denmark, ⁵³Institute of Sociology, University of Bern, Bern, Switzerland,
176 ⁵⁴CUBE Research Center, Catolica Lisbon School of Business and Economics, Lisbon, Portugal,
177 ⁵⁵CEPE, ETH Zürich, Zürich, Switzerland, ⁵⁶Psychiatry Department, Columbia University, New
178 York, NY, USA, ⁵⁷Institute of Cognitive Neuroscience, University College London, London,
179 United Kingdom, ⁵⁸Chair of Macroeconomics, Technical University Berlin, Berlin, Germany,
180 ⁵⁹Department of Psychology, Hebrew University of Jerusalem, Jerusalem, Israel,
181 ⁶⁰Psychological and Brain Sciences, Dartmouth College, Hanover, USA, ⁶¹Charles and Louise
182 Travers Department of Political Science, UC Berkeley, Berkeley, CA, USA, ⁶²Department of
183 Psychology, Anadolu University, Eskişehir, Türkiye, ⁶³Department of Linguistics, University of
184 California, Santa Barbara, Santa Barbara, CA, USA, ⁶⁴Department of Computational Social
185 Science, GESIS - Leibniz Institute for the Social Sciences, Cologne, Germany, ⁶⁵Guelph Institute
186 of Development Studies, University of Guelph, Guelph, ON, Canada, ⁶⁶Department of Political
187 Science, ⁶⁷Department of Psychology, University of Graz, Graz, Austria, ⁶⁸Analytics, Harrisburg
188 University of Science and Technology, Harrisburg, PA, USA, ⁶⁹Department of Economics,
189 University of Zurich, Zurich, Switzerland, ⁷⁰Department of Technology Education,
190 Technological University of the Shannon: Midlands Midwest, Athlone, Ireland, ⁷¹Experimental
191 and Applied Psychology, Vrije Universiteit Amsterdam, Amsterdam, The Netherlands, ⁷²Institute
192 of Psychology, SWPS University, Sopot, Poland, ⁷³Department of Criminology, Max Planck
193 Institute for the Study of Crime, Security and Law, Freiburg im Breisgau, Germany, ⁷⁴The
194 School of Mathematics and Computer Science (SMCS), Institute of Business Administration,
195 Karachi, Pakistan, ⁷⁵Computer Science Department, DHA Suffa University, Karachi, Pakistan,
196 ⁷⁶School of Humanities, Arts, and Social Sciences, Stevens Institute of Technology, Hoboken,
197 NJ, USA, ⁷⁷Independent Researcher, New York, NY, USA, ⁷⁸Institute for Medical Information
198 Processing, Biometry, and Epidemiology, Ludwig-Maximilians-Universität München, Munich,
199 Germany, ⁷⁹Department of Economics, Swedish University of Agricultural Sciences, Uppsala,
200 Sweden, ⁸⁰Department of Psychology, Stonehill College, Easton, MA, USA, ⁸¹Center for
201 Depression, Anxiety, and Stress Research, McLean Hospital, Belmont, MA, USA, ⁸²Department
202 of Psychiatry, Harvard Medical School, Boston, USA, ⁸³Department of International Relations
203 and Public Policy, São Paulo State University, Franca, Brazil, ⁸⁴Kellogg Institute for
204 International Studies, University of Notre Dame, Notre Dame, USA, ⁸⁵Ted Rogers School of
205 Management, Toronto Metropolitan University, Toronto, ON, Canada, ⁸⁶Institute for Social
206 Research, University of Michigan, Ann Arbor, MI, USA, ⁸⁷Mathematica, 220 Easy Huron Street,
207 Ann Arbor, MI, 48104, ⁸⁸Department of Biological Sciences, National University of Singapore,
208 Singapore, Singapore, ⁸⁹Chinese Open Science Network, Shanghai, China, ⁹⁰Dept. of Intelligent
209 Perception and Interaction Research, OPPO Shanghai Research Institute, Shanghai, China,
210 ⁹¹Lazaridis School of Business and Economics, Wilfrid Laurier University, Waterloo, ON,
211 Canada, ⁹²School of Management, Neuromanagement Laboratory & The State Key Laboratory
212 of Brain-machine Intelligence, Zhejiang University, Hangzhou, China, ⁹³Department of
213 Psychiatry and Psychotherapy, Technische Universität Dresden, Dresden, Germany, ⁹⁴Institute
214 for Theoretical Biology, Department of Biology, Humboldt-Universität zu Berlin, Berlin,
215 Germany, ⁹⁵Science of Intelligence, Research Cluster of Excellence, Berlin, Germany, ⁹⁶Institute
216 for Complex Systems - National Research Council of Italy, Rome, Italy, ⁹⁷Enrico Fermi Research
217 Center, ⁹⁸Melbourne School of Psychological Sciences, The University of Melbourne,
218 Melbourne, VIC, Australia, ⁹⁹Department of Psychology, York University, Toronto, ON,
219 Canada, ¹⁰⁰English Language and Linguistics, School of Critical Studies, University of Glasgow,

220 Glasgow, UK, ¹⁰¹UTS Business School, University of Technology Sydney, Ultimo, NSW,
221 Australia, ¹⁰²Department of Psychological and Brain Sciences, University of Iowa, Iowa City,
222 IA, USA, ¹⁰³Center for Research in Cognition & Neurosciences, Université Libre de Bruxelles,
223 Brussels, Belgium, ¹⁰⁴Department for Psychology of Digitalisation, University of Bern, Bern,
224 Switzerland, ¹⁰⁵Department of Experimental-Clinical and Health Psychology, Ghent University,
225 Ghent, Belgium, ¹⁰⁶Department of Sociology and Anthropology, Tel Aviv University, Tel Aviv,
226 Israel, ¹⁰⁷Department of Sociology, Goethe University Frankfurt, Frankfurt am Main, Germany,
227 ¹⁰⁸Department of Work, Organisation and Society, Ghent University, Gent, Belgium, ¹⁰⁹School
228 of Business and Economics, Vrije Universiteit Amsterdam, Amsterdam, Netherlands,
229 ¹¹⁰Department of Philosophy, Sociology, Education and Applied Psychology, University of
230 Padova, Padua, Italy, ¹¹¹School of Psychology, University of Lincoln, Lincoln, England,
231 ¹¹²Department of Psychology & Neuroscience, Duke University, Durham, NC, USA,
232 ¹¹³Department of Psychology, Memorial University of Newfoundland, St. John's, NL, Canada,
233 ¹¹⁴Erasmus School of History, Culture and Communication, Erasmus University Rotterdam,
234 Rotterdam, The Netherlands, ¹¹⁵Cognitive Science, Vassar College, Poughkeepsie, NY, USA,
235 ¹¹⁶Copenhagen Health Complexity Center, Department of Public Health, University of
236 Copenhagen, Copenhagen, Denmark, ¹¹⁷Department of Health Sciences, University of
237 Groningen, University Medical Center Groningen, Groningen, The Netherlands, ¹¹⁸Collective
238 Intelligence and Design Group, University of Cambridge, Cambridge, UK, ¹¹⁹Climate and Social
239 Intelligence Lab, Caltech, USA, ¹²⁰Department of Cognitive Psychology, Institute of
240 Psychology, Czech Academy of Sciences, Prague, Czech Republic, ¹²¹College of Business and
241 Technology, East Tennessee State University, Johnson City, TN, USA, ¹²²School of Computer
242 Communication Sciences, Ecole Polytechnique Lausanne, Lausanne, Switzerland, ¹²³Institute of
243 Psychology, ELTE Eötvös Loránd University, Budapest, ¹²⁴Department of Neurology and
244 Neurosurgery, Montreal Neurological Institute, Montréal, QC, Canada, ¹²⁵Department of
245 Sociology, University of Copenhagen, Copenhagen, Denmark, ¹²⁶Institute of General Practice
246 and Family Medicine, University Hospital Bonn, University of Bonn, Bonn, Germany,
247 ¹²⁷Department of Psychology, University of Wisconsin-Madison, Madison, WI, USA,
248 ¹²⁸Department of Psychology, University of Washington, Seattle, WA, USA, ¹²⁹Faculty of
249 Psychology, FernUniversität in Hagen, Hagen, Germany, ¹³⁰Department of Psychology,
250 University of Cologne, Germany, ¹³¹Department of Economics, Stockholm School of
251 Economics, Stockholm, Sweden, ¹³²Department of Psychology, Beijing Normal University at
252 Zhuhai, Zhuhai, China, ¹³³Beijing Key Laboratory of Applied Experimental Psychology,
253 National Demonstration Center for Experimental Psychology Education (Beijing Normal
254 University), Faculty of Psychology, Beijing Normal University, Beijing, China, ¹³⁴Department
255 of Psychology, Rochester Institute of Technology, Rochester, NY, USA, ¹³⁵Marketing and
256 supply chain management, Maastricht University, Maastricht, The Netherlands, ¹³⁶Independent
257 Researcher, Philadelphia, PA, USA, ¹³⁷Psychology and Vision Sciences, University of Leicester,
258 Leicester, England, ¹³⁸University of Birmingham, ¹³⁹Institute of Psychology, Department of
259 Clinical Psychology, Psychotherapy and Psychoanalysis, University of Klagenfurt, Klagenfurt
260 am Woerthersee, Austria, ¹⁴⁰Social Psychology and Methodology, Universidad Autónoma de
261 Madrid, Madrid, Spain, ¹⁴¹School of Education and Psychology, University of Navarra,
262 Pamplona, Spain, ¹⁴²School of Human Sciences, University of Greenwich, London, UK,
263 ¹⁴³Institute for Lifecourse Development, ¹⁴⁴Institute of Anthropological Research, National
264 Autonomous University of Mexico, Mexico city, Mexico, ¹⁴⁵Faculty of Economics and
265 Management, Ibn Tofail University, Kénitra, Morocco, ¹⁴⁶Department of Economics, University

266 of Ottawa, Ottawa, ON, Canada, ¹⁴⁷Institute for Replication, University of Ottawa, Ottawa, ON,
267 Canada, ¹⁴⁸Department of Economics, Tilburg University, Netherlands, ¹⁴⁹School of Sociology,
268 HSE University, Moscow, Russia, ¹⁵⁰International Inequalities Institute, London School of
269 Economics and Political Science, London, United Kingdom, ¹⁵¹Air Force Research Laboratory,
270 Wright Patterson AFB, OH, USA, ¹⁵²Department of Public Economics & MUEEL, Masaryk
271 University, Brno, Czech Republic, ¹⁵³Universidad Siglo 21, Córdoba, Argentina, ¹⁵⁴Universidad
272 Católica de Córdoba, Argentina, ¹⁵⁵Discipline of Marketing, J.E. Cairnes School of Business &
273 Economics, University of Galway, Galway, Ireland, ¹⁵⁶School of Psychology, Family, and
274 Community, Seattle Pacific University, Seattle, WA, USA, ¹⁵⁷Postgraduate Program of
275 Psychology, São Francisco University, Campinas, Brazil, ¹⁵⁸Department of Psychology,
276 University of Amsterdam, Amsterdam, The Netherlands, ¹⁵⁹Department of Quantitative Theory
277 and Methods, Emory University, Atlanta, GA, USA, ¹⁶⁰School of Social and Political Sciences,
278 University of Lincoln, Lincoln, United Kingdom, ¹⁶¹Department of Political and Social Sciences,
279 European University Institute, Fiesole, Italy, ¹⁶²Center for Science of Science and Innovation,
280 Evanston, IL, USA, ¹⁶³Kellogg School of Management, Northwestern University, Evanston,
281 Illinois, USA, ¹⁶⁴Department of Statistics, Harvard University, Cambridge, MA, USA, ¹⁶⁵John
282 Glenn College of Public Affairs, The Ohio State University, Columbus, Ohio, USA, ¹⁶⁶Caspian
283 Strategy & Analytics, Dublin, Ohio, USA, ¹⁶⁷Psychology, Humboldt-Universität zu Berlin,
284 Berlin, Germany, ¹⁶⁸Berlin School of Mind and Brain, ¹⁶⁹Education and Child Studies, Leiden
285 University, Leiden, The Netherlands, ¹⁷⁰Department of Psychology, Wake Forest University,
286 Winston-Salem, NC, USA, ¹⁷¹Faculty of Psychology in Wroclaw, SWPS University, Wroclaw,
287 Poland, ¹⁷²Federal Senate of Brazil, Brasília, Brazil, ¹⁷³Department of Economics, Deakin
288 University, Melbourne, VIC, Australia, ¹⁷⁴Department of Brain and Behavioral Sciences,
289 University of Pavia, Pavia, Italy, ¹⁷⁵Champalimaud Foundation, Lisbon, Portugal, ¹⁷⁶School of
290 Economics, University College Dublin, Dublin, Ireland, ¹⁷⁷Geary Institute for Public Policy,
291 University College Dublin, Dublin, Ireland, ¹⁷⁸Dipartimento di Economia, Università degli Studi
292 dell'Insubria, Varese, Italy, ¹⁷⁹Department of Neuroscience and Biomedical Engineering, Aalto
293 University, Espoo, Finland, ¹⁸⁰Department of Psychology, New York University Abu Dhabi,
294 Abu Dhabi, United Arab Emirates, ¹⁸¹Educational Measurement, Leibniz Institute for
295 Educational Trajectories, Bamberg, Germany, ¹⁸²Centre for Research on Peace and
296 Development, KU Leuven, Leuven, Belgium, ¹⁸³IESEG School of Management, Lille, France,
297 ¹⁸⁴Institute of Public Administration, Leiden University, The Hague, The Netherlands,
298 ¹⁸⁵Universidad Diego Portales, ¹⁸⁶Department of Child and Adolescent Psychiatry, Medical
299 University of Vienna, Vienna, Austria, ¹⁸⁷Department of Business and Economics, TU
300 Dortmund University, Dortmund, Germany, ¹⁸⁸ESCP Business School, Paris, France, ¹⁸⁹Faculty
301 of Economics, UniDistance Suisse, Brig, Switzerland, ¹⁹⁰Zurich University of Applied Sciences
302 (ZHAW), School of Management and Law, ¹⁹¹Center for Sociocultural Research, HSE
303 University, Moscow, Russia, ¹⁹²Institute of Psychology, University of Bern, Bern, Switzerland,
304 ¹⁹³Institute of Psychology, University of Graz, Graz, Austria, ¹⁹⁴Center for Adaptive Rationality,
305 Max-Planck Institute, Berlin, Germany, ¹⁹⁵Psychology, University of California, Los Angeles,
306 Los Angeles, CA, USA, ¹⁹⁶University of Wisconsin, Madison, ¹⁹⁷Department of Psychology,
307 University Grenoble Alpes, CNRS UMR 5105 LPNC, Grenoble, France, ¹⁹⁸Département
308 d'études cognitives, Ecole Normale Supérieure Paris, Paris, France, ¹⁹⁹Department of Strategy
309 of Innovation, Institute for Cognition and Behavior, WU Vienna University of Economics and
310 Business, Vienna, Austria, ²⁰⁰Management & Organization, University of Washington, Seattle,
311 WA, USA, ²⁰¹Managing People in Organizations, IESE Business School, Barcelona, Spain,

312 ²⁰²Department of Psychology: Social Psychology, University of Cologne, Cologne, Germany,
313 ²⁰³Data Science, Sueddeutsche Zeitung Digitale Medien, Munich, Germany, ²⁰⁴Department of
314 Psychology, Northumbria University, Newcastle-upon-Tyne, United Kingdom, ²⁰⁵Centre for
315 Social and Behaviour Change, Ashoka University, New Delhi, India, ²⁰⁶Department of
316 Economics, Finance, and Legal Studies, University of Alabama, Tuscaloosa, AL, USA,
317 ²⁰⁷Department of Social and Political Science, European University Institute, San Domenico di
318 Fiesole, Italy, ²⁰⁸Institute of Neuroscience, Catholic University of Louvain, Brussels, Belgium,
319 ²⁰⁹Faculty of Psychology, University of Akureyri, Akureyri, Iceland, ²¹⁰Department of
320 Psychology, Kadir Has University, Istanbul, Turkey, ²¹¹Department of Psychology, University
321 of California, Berkeley, Berkeley, CA, USA, ²¹²Department of Decision Sciences, Corvinus
322 University of Budapest, Budapest, Hungary, ²¹³International Institute for Applied Systems
323 Analysis (IIASA), Austria, Laxenburg, ²¹⁴Institute for Social Sciences, University of Stuttgart,
324 Stuttgart, Germany, ²¹⁵Labour and public policies, Paris school of economics, Paris, France,
325 ²¹⁶Statistics, evaluation and research department, Caisse nationale des allocations familiales,
326 Paris, France, ²¹⁷Economics, Frisch Centre, Oslo, Norway, ²¹⁸Department of Psychiatry &
327 Neuropsychology, Mental Health and NeuroScience (MHeNS) Research Institute, Maastricht
328 University, Maastricht, The Netherlands, ²¹⁹Methodology and Statistics Unit, Institute of
329 Psychology, Leiden University, Leiden, The Netherlands, ²²⁰Psychological and Brain Sciences,
330 Texas A&M University, College Station, TX, USA, ²²¹Department of Psychology, University of
331 New Mexico, Albuquerque, NM, USA, ²²²Department of Brain and Cognitive Sciences,
332 Massachusetts Institute of Technology, Cambridge, MA, USA, ²²³Dartmouth College,
333 ²²⁴Department of Finance, Aalto University, Espoo, Finland, ²²⁵Applied Economics, University
334 of Minnesota, St. Paul, MN, USA, ²²⁶Department of Political Science, University of Duisburg-
335 Essen, Duisburg, Germany, ²²⁷Department of Health Promotion, Care and Public Health
336 Research Institute (CAPHRI), Maastricht University, Maastricht, The Netherlands,
337 ²²⁸Department of Psychology, Ruhr University Bochum, Bochum, Germany, ²²⁹Department of
338 Psychology, Japan Women's University, Bunkyo-ku, Japan, ²³⁰School of Economics, The
339 University of Queensland, St Lucia, Australia, ²³¹Faculty of Psychology in Wrocław, SWPS
340 University, Wrocław, Poland, ²³²Division of Psychology, Communication and Human
341 Neuroscience, University of Manchester, Manchester, UK, ²³³Department of Radiology,
342 University of Michigan, Ann Arbor, MI, USA, ²³⁴Department of Psychology, Eastern Michigan
343 University, Ypsilanti, MI, USA, ²³⁵Yale Law School, Yale University, New Haven, CT, USA,
344 ²³⁶Melbourne Business School, Carlton, Australia, ²³⁷Political Science, University of California,
345 Merced, Merced, CA, USA, ²³⁸Department of Psychology, University of Cologne, Köln,
346 Germany, ²³⁹Department of Psychology, Seton Hall University, South Orange, NJ, USA,
347 ²⁴⁰Department of Psychology, Universidad de los Andes, Bogotá, Colombia, ²⁴¹McCourt School
348 of Public Policy, Georgetown University, Washington, DC, USA, ²⁴²School of Psychology,
349 Swansea University, Swansea, UK, ²⁴³Institute of Political Science and Sociology, University of
350 Bonn, Bonn, Germany, ²⁴⁴University of Bamberg, ²⁴⁵Department of Psychology, Faculty of Arts,
351 Pavol Jozef Šafárik University in Košice, Košice, Slovakia, ²⁴⁶Warwick Business School,
352 University of Warwick, Coventry, United Kingdom, ²⁴⁷University of Oxford, ²⁴⁸Independent
353 Researcher, Manisa, Turkey, ²⁴⁹Institute of Security and Global Affairs, Leiden University, The
354 Hague, The Netherlands, ²⁵⁰MoMiLab, Social and Affective Neuroscience Group, IMT School
355 for Advanced Studies Lucca, Lucca, Italy, ²⁵¹Department of Psychology, University of Essex,
356 Colchester, UK, ²⁵²Institute of Psychology, Leiden University, Leiden, The Netherlands,
357 ²⁵³Mater Research Institute, University of Queensland, Brisbane, Australia, ²⁵⁴School of

358 Psychology, Dublin City University, Dublin, Ireland, ²⁵⁵Victoria University of Wellington, New
359 Zealand, ²⁵⁶Department of Economics and Management, University of Trento, Trento, Italy,
360 ²⁵⁷Flo Health LTU, UAB, Vilnius, Lithuania, ²⁵⁸D'Amore-McKim School of Business,
361 Northeastern University, ²⁵⁹Department of Banking and Finance, University of Innsbruck,
362 Innsbruck, Austria, ²⁶⁰Institute of Operations and Decision Sciences, Corvinus University of
363 Budapest, Budapest, Hungary, ²⁶¹Structural Change and Work Organisation, Federal Institute
364 for Occupational Safety and Health (BAuA), Dortmund, Germany, ²⁶²Chair for
365 Industrial/Organizational Psychology, University of Hamburg, Hamburg, Germany,
366 ²⁶³Department of Psychology, The University of Hong Kong, Hong Kong SAR, China, ²⁶⁴Hong
367 Kong Institute of Asia-Pacific Studies, The Chinese University of Hong Kong, Hong Kong SAR,
368 China, ²⁶⁵Computer Vision Group, Sano Centre for Computational Personalized Medicine,
369 Krakow, Poland, ²⁶⁶Department of Research on Social and Institutional Transformations,
370 Institute of Political Studies of the Polish Academy of Sciences, Warszawa, Poland,
371 ²⁶⁷Department of Mathematics, Aristotle University of Thessaloniki, Thessaloniki, Greece,
372 ²⁶⁸Independent Researcher, Brno, Czechia, ²⁶⁹Department of Health and Functioning, Western
373 Norway University of Applied Sciences, Bergen, Norway, ²⁷⁰Mohn Medical Imaging and
374 Visualisation Center, Bergen, Norway, ²⁷¹Dep. Neurology, Haukeland University Hospital,
375 Faculty of Medicine, University of Bergen, Bergen, Norway, ²⁷²IDN Being Human Lab -
376 Institute of Psychology, University of Wroclaw, Wroclaw, Poland, ²⁷³Department of
377 Psychology, University of Zurich, Zurich, Switzerland, ²⁷⁴CAPE, École Polytechnique Fédérale
378 de Lausanne, Lausanne, Switzerland, ²⁷⁵Department of Clinical Psychology, Utrecht University,
379 Utrecht, The Netherlands, ²⁷⁶Group of Health Psychology, KU Leuven, Leuven, Belgium,
380 ²⁷⁷Department of Physics, Freie Universität Berlin, Berlin, Germany, ²⁷⁸Department of
381 Psychology, Senshu University, Kawasaki, Japan, ²⁷⁹Department of Personality Psychology and
382 Social Psychology, Institute of Psychology, Czech Academy of Sciences, Brno, Czech Republic,
383 ²⁸⁰Department of Marketing, Goethe University Frankfurt, Frankfurt am Main, Germany,
384 ²⁸¹Labour Markets and Social Insurance, ZEW – Leibniz Centre for European Economic
385 Research, Mannheim, Germany, ²⁸²Department of Psychological Sciences, Purdue University,
386 West Lafayette, IN, USA, ²⁸³Lerner College of Business, University of Delaware, Newark, DE,
387 USA, ²⁸⁴Department of Psychological and Brain Sciences, Boston University, Boston, MA,
388 USA, ²⁸⁵Psychology, University of Maryland, College Park, MD, USA, ²⁸⁶Department of
389 Politics, State University of New York, Potsdam, Potsdam, NY, USA, ²⁸⁷Department of
390 translational research and advanced technologies in medicine and surgery, University of Pisa,
391 Pisa, Italy, ²⁸⁸Nuclear Medicine Unit, Department of Diagnostic Imaging, N.O.P. - S. Stefano,
392 U.S.L. Toscana Centro, Prato, Italy, ²⁸⁹Department of Experimental Psychology, University of
393 Ghent, Ghent, Belgium, ²⁹⁰Rady School of Management, University of California San Diego, La
394 Jolla, CA, USA, ²⁹¹Katz Graduate School of Business, University of Pittsburgh, Pittsburgh,
395 USA, ²⁹²School of Management, University of Bath, Bath, UK, ²⁹³Institute of Developmental
396 Psychology, Beijing Normal University, Beijing, China, ²⁹⁴Department of Economics,
397 University of Exeter, Exeter, UK, ²⁹⁵School of English and International Studies, Beijing Foreign
398 Studies University, Beijing, China, ²⁹⁶Stanford Center on Early Childhood, Stanford University,
399 Stanford, CA, USA, ²⁹⁷Department of Social, Personality, and Health Psychology, University of
400 Houston, Houston, TX, USA, ²⁹⁸Department of Sociology, University of Essex, Colchester, UK,
401 ²⁹⁹Department of Methodology and Statistics, Tilburg University, Tilburg, The Netherlands,
402 ³⁰⁰Department of Physical Medicine and Nursing, University of Zaragoza, Zaragoza, Spain,
403 ³⁰¹Department of Basic Psychology and Methodology, University of Murcia, Murcia, Spain,

404 ³⁰²Department of Psychology, University of Victoria, Victoria, BC, Canada, ³⁰³German Centre
405 for Higher Education Research and Science Studies (DZHW), Berlin, Germany, ³⁰⁴MIT Sloan
406 School of Management, Massachusetts Institute of Technology, Cambridge, MA, USA,
407 ³⁰⁵Technische Universität Berlin, Berlin, Germany, ³⁰⁶Social Neuroscience Lab, Department of
408 Psychiatry and Psychotherapy, University of Lübeck, Lübeck, Germany, ³⁰⁷Open Science
409 Initiative, University of Lübeck, Lübeck, Germany, ³⁰⁸Department of Economics, University of
410 Maryland, College Park, College Park, MD, USA, ³⁰⁹Institute for Behavioral Genetics,
411 University of Colorado at Boulder, Boulder, CO, USA, ³¹⁰Department of Psychology, Charles
412 University, Prague, Czech republic, ³¹¹Department of Psychology, University of Toronto,
413 Toronto, ON, Canada, ³¹²School of Psychology, University of Nottingham, Nottingham, UK,
414 ³¹³Department of Translation and Language Sciences, Universitat Pompeu Fabra, Barcelona,
415 Spain, ³¹⁴Department of Psychological & Brain Sciences, Texas A&M University, College
416 Station, TX, USA, ³¹⁵The Ohio State University Medical Center, ³¹⁶School of Public Affairs,
417 Penn State Harrisburg, Middletown, PA, USA, ³¹⁷Department of Life Sciences, University of
418 Trieste, Trieste, Italy, ³¹⁸Cognitive and Information Sciences, University of California, Merced,
419 Merced, CA, USA, ³¹⁹Department of Economics , University of Bologna, Bologna, Italy,
420 ³²⁰Department of Economics, Durham University, Durham, UK, ³²¹Department of
421 Interdisciplinary Life Sciences, Domestication Lab, Konrad Lorenz Institute of Ethology,
422 University of Veterinary Medicine Vienna, Vienna, Austria, ³²²Language Research Center,
423 Georgia State University, Atlanta, Georgia, USA, ³²³Institute of Economics & L'EMbeDS,
424 Sant'Anna School of Advanced Studies, Pisa, Italy, ³²⁴School of Economics, University of
425 Sheffield, Sheffield, UK, ³²⁵Université de Lorraine, Université de Strasbourg, CNRS, BETA,
426 Nancy, France, ³²⁶Department of Psychology and Special Education, East Texas A&M
427 University, Commerce, TX, USA, ³²⁷School of Business and Economics, University of
428 Tübingen, Tübingen, Germany, ³²⁸Independent Researcher, Doylestown, PA, USA, ³²⁹Leibniz-
429 Institut für Wissensmedien (Knowledge Media Research Center), Tübingen, Germany,
430 ³³⁰Methodology & Statistics, Universiteit Leiden, Leiden, The Netherlands, ³³¹Education
431 Statistics and Research Methods, University of Delaware, Newark, USA, ³³²Department of
432 Psychology, Binghamton University, Binghamton, NY, USA, ³³³Center for Cognitive
433 Applications, Binghamton University, Binghamton, USA, ³³⁴Annenberg School for
434 Communication, University of Pennsylvania, Philadelphia, PA, USA, ³³⁵Annenberg School for
435 Communication, University of Pennsylvania, ³³⁶Department of Psychology, California State
436 University San Bernardino, San Bernardino, CA, USA, ³³⁷Department of Psychology, Southern
437 Connecticut State University, New Haven, USA, ³³⁸Faculty of Business and Economics,
438 University of Basel, Basel, Switzerland, ³³⁹University of Lausanne, ³⁴⁰Department of Business
439 Administration, University of Zurich, Zurich, Switzerland, ³⁴¹Departamento de Psicobiología y
440 Metodología en Ciencias del Comportamiento, Universidad Complutense de Madrid, Madrid,
441 Spain, ³⁴²Department of Economics and Business Economics, Aarhus University, Aarhus,
442 Denmark, ³⁴³Danish Finance Institute, Frederiksberg, Denmark, ³⁴⁴Department of Psychology,
443 University of Cyprus, Nicosia, Cyprus, ³⁴⁵Department of Political Science, University of
444 Gothenburg, Gothenburg, Sweden, ³⁴⁶Department of Management and Engineering, Division of
445 Economics, Jedi Lab, Linköping University, 581 83 Linköping, Sweden, ³⁴⁷Global Institute for
446 Women's Leadership, Canberra, ACT, Australia, ³⁴⁸Department of Economics, University of
447 Mannheim, Mannheim, Germany, ³⁴⁹Department of Psychology, Uppsala University, Uppsala,
448 Sweden, ³⁵⁰Faculty of Psychology, Saint Petersburg State University, Saint Petersburg, Russia,
449 ³⁵¹IDN Being Human, University of Wroclaw, Wroclaw, Poland, ³⁵²University of Oxford,

450 School of Anthropology and Museum Ethnography, ³⁵³Center for Social and Cultural
451 Psychology, Université libre de Bruxelles [Free University of Brussels], Brussels, Belgium,
452 ³⁵⁴School of Psychology, University of Auckland, Auckland, New Zealand, ³⁵⁵University of
453 Auckland, Centre for Brain Research, Auckland, NZ, ³⁵⁶School of Geography, Politics and
454 Sociology, Newcastle University, Newcastle upon Tyne, United Kingdom, ³⁵⁷ESRC Centre for
455 Population Change, University of Southampton, UK, ³⁵⁸Department of Management, Ben-
456 Gurion University of the Negev, Be'er Sheva, Israel, ³⁵⁹School of Psychology, University of
457 Leeds, Leeds, United Kingdom, ³⁶⁰Leeds NIHR Biomedical Research Centre, ³⁶¹Department of
458 Psychology, University of Bern, Bern, Switzerland, ³⁶²GESIS Leibniz Institute for the Social
459 Sciences, Mannheim, Germany, ³⁶³School of Psychology, Center for Social and Cognitive
460 Neuroscience, Universidad Adolfo Ibáñez, Santiago de Chile, Chile, ³⁶⁴Zurich Center for
461 Neuroeconomics, Department of Economics, University of Zurich, Zurich, Switzerland,
462 ³⁶⁵Department of Psychological Methods, Helmut Schmidt University Hamburg, Hamburg,
463 Germany, ³⁶⁶Department of Sociology of Education, University of Bern, Bern, Switzerland,
464 ³⁶⁷Karlsruhe University of Education, Karlsruhe, Germany, ³⁶⁸Institute of Neuroscience and
465 Medicine (INM-7: Brain and Behaviour), Research Centre Jülich, Jülich, Germany, ³⁶⁹Institute
466 of Systems Neuroscience, Heinrich Heine University Düsseldorf, Düsseldorf, Germany,
467 ³⁷⁰Department of Linguistics, Cognitive Science and Semiotics, Aarhus University, Aarhus,
468 Denmark, ³⁷¹Neuroimaging Group, Instituto Cajal, CSIC, Madrid, Spain, ³⁷²AgroParisTech,
469 INRAE, VetAgro Sup, Université Clermont Auvergne, Lempdes, France, ³⁷³Department of
470 Marketing, Istanbul Bilgi University, Istanbul, Turkey, ³⁷⁴Department of Economics, University
471 of Maryland College Park, College Park, MD, USA, ³⁷⁵Department of Neurology, University of
472 Utah, Salt Lake City, UT, USA, ³⁷⁶Center for Crisis Early Warning, University of the
473 Bundeswehr Munich, Neubiberg, Germany, ³⁷⁷Asylum and Migration group, WODC - Research
474 and Data Centre, The Hague, The Netherlands, ³⁷⁸Department of Humanities and Social
475 Sciences, Indian Institute of Technology Bombay, Mumbai, India, ³⁷⁹University of Osnabrueck,
476 Germany, ³⁸⁰Center for Social and Cultural Psychology, Université libre de Bruxelles, Brussels,
477 Belgium, ³⁸¹University of Amsterdam, ³⁸²Vision & Cognition, Netherlands Institute for
478 Neuroscience (KNAW), Amsterdam, The Netherlands, ³⁸³Department of Social Psychology,
479 Helmut Schmidt University Hamburg, Hamburg, Germany, ³⁸⁴Department of Social Psychology,
480 University of Amsterdam, Amsterdam, The Netherlands, ³⁸⁵University of Tuebingen,
481 Tuebingen, Germany, ³⁸⁶Department of Biostatistics, University of Zurich, Zurich, Switzerland,
482 ³⁸⁷Department of Communication Sciences and Disorders, Northeastern University, Boston,
483 MA, USA, ³⁸⁸Research Center "Normative Orders", Goethe University Frankfurt, Frankfurt am
484 Main, Germany, ³⁸⁹Peace Research Institute Frankfurt (PRIF), Frankfurt am Main, Germany,
485 ³⁹⁰Department of Biomedical, Metabolic, and Neurological Sciences, University of Modena and
486 Reggio Emilia, Modena, Italy, ³⁹¹The Sababi Institute, Hargeisa, Somaliland, ³⁹²Faculty of Social
487 Studies, Department of Social Work, University of Ostrava, Ostrava, Czech Republic,
488 ³⁹³Département d'études cognitives, ENS, EHESS, PSL University, Institut Jean Nicod, Paris,
489 France, ³⁹⁴Department of Psychology, Norwegian University of Science and Technology,
490 Trondheim, Norway, ³⁹⁵School of Law and Politics, Cardiff University, Cardiff, United
491 Kingdom, ³⁹⁶Political Science, University of Wisconsin–Madison, Madison, WI, USA,
492 ³⁹⁷Department of Psychology, University of Liverpool, Liverpool, UK, ³⁹⁸Centre for Philosophy
493 of Natural and Social Science, London School of Economics, London, UK, ³⁹⁹Institute of Human
494 Sciences, University of Oxford, Oxford, United Kingdom, ⁴⁰⁰Calleva Research Centre for
495 Evolution and Human Sciences, Magdalen College, Oxford, United Kingdom, ⁴⁰¹Center for

496 Adaptive Rationality, Max Planck Institute for Human Development, Berlin, Germany,
497 ⁴⁰²Danish Research Centre for Magnetic Resonance, Department of Radiology and Nuclear
498 Medicine, Copenhagen University Hospital - Amager and Hvidovre, Hvidovre, Denmark,
499 ⁴⁰³Department of Psychiatry and Psychotherapy, Ludwig-Maximilians-Universität München,
500 Munich, Germany, ⁴⁰⁴Department of Psychology, Stanford University, Stanford, CA, USA,
501 ⁴⁰⁵Department of Communication and Cognition, Tilburg University, Tilburg, The Netherlands,
502 ⁴⁰⁶Department of Management, Université Paris Dauphine PSL, Paris, France, ⁴⁰⁷Center for
503 Research in Economics and Statistics (CREST), ENSAE, Institut Polytechnique de Paris,
504 Palaiseau, France, ⁴⁰⁸Carlos L. Tiu School of Economics, De La Salle University - Manila,
505 Philippines, ⁴⁰⁹School of Business, Economics, and Information Systems, University of Passau,
506 Passau, Germany, ⁴¹⁰Evidence-to-Impact Collaborative, Pennsylvania State University,
507 University Park, PA, USA, ⁴¹¹School of Psychological Sciences, University of Tasmania,
508 Hobart, TAS, Australia, ⁴¹²Menzies Institute for Medical Research, University of Tasmania,
509 Hobart, TAS, Australia, ⁴¹³Brain and Cognitive Sciences, Massachusetts Institute of Technology,
510 Cambridge, MA, USA, ⁴¹⁴Department of Economics, Tilburg University, Tilburg, The
511 Netherlands, ⁴¹⁵GREDEG, Université Cote d'Azur, Nice, France, ⁴¹⁶Department of Sociology,
512 Radboud University, Nijmegen, The Netherlands, ⁴¹⁷Department of Psychology, New York
513 University, New York, NY, USA, ⁴¹⁸Department of Psychology, National Research University
514 Higher School of Economics, Moscow, Moscow, Russia, ⁴¹⁹Department of Psychology,
515 Willamette University, Salem, OR, USA, ⁴²⁰School of Psychology, Aston University,
516 Birmingham, United Kingdom, ⁴²¹Psychology, Artois University, Arras, France, ⁴²²Department
517 of Interdisciplinary Human Sciences, Texas Tech University, Lubbock, TX, USA, ⁴²³Escuela de
518 Gobierno y Administración Pública, Universidad Mayor, Santiago, Chile, ⁴²⁴Department of
519 Psychiatry, Department of Experimental Psychology, University of Oxford, Oxford, United
520 Kingdom, ⁴²⁵Department of Experimental Psychology, University of Granada, Granada, Spain,
521 ⁴²⁶Mind, Brain and Behavior Research Center (CIMCYC), University of Granada, Granada,
522 Spain, ⁴²⁷Department of Psychology, Neuroscience and Behaviour, McMaster University,
523 Hamilton, Canada, ⁴²⁸McMaster Institute for the Music and the Mind, McMaster University,
524 Hamilton, Canada, ⁴²⁹Faculty of Education, Charles University, Prague, Czech Republic,
525 ⁴³⁰Institute of Psychology, Czech Academy of Sciences, Brno, Czech Republic, ⁴³¹Münster
526 Center for Open Science, University of Münster, Münster, Germany, ⁴³²Department of
527 Philosophy, Macquarie University, Sydney, NSW, Australia, ⁴³³Methodology and Statistics,
528 Tilburg University, Tilburg, The Netherlands, ⁴³⁴Max Planck Institute for Research on Collective
529 Goods, Bonn, Germany, ⁴³⁵InsIDE Lab, Dipartimento di Economia, Università degli Studi
530 dell'Insubria, Varese, Italy, ⁴³⁶ILC, CNR, Pisa, Italy, ⁴³⁷Department of Psychology, University
531 of Konstanz, Konstanz, Germany, ⁴³⁸Department of Ergonomics and Psychology, Budapest
532 University of Technology and Economics, Budapest, Hungary, ⁴³⁹Computational Social Science,
533 University of Chicago, Chicago, IL, USA, ⁴⁴⁰Melbourne School of Psychological Sciences,
534 University of Melbourne, Melbourne, VIC, Australia, ⁴⁴¹Department of Human Sciences,
535 University of Verona, Verona, Italy, ⁴⁴²Department of Economics and Social Sciences,
536 University of Natural Resources and Life Sciences, Vienna, Austria, ⁴⁴³Lee Kong Chian School
537 of Business, Singapore Management University, Singapore, Singapore, ⁴⁴⁴University Library,
538 Iowa State University, Ames, IA, USA, ⁴⁴⁵Centre for Marine Science and Innovation, UNSW
539 Australia, UNSW, NSW, Australia, ⁴⁴⁶Department of Child and Adolescent Psychiatry,
540 Psychosomatics and Psychotherapy, University Hospital, LMU Munich, Munich, Germany,
541 ⁴⁴⁷Department of Psychology, Ashland University, Ashland, OH, USA, ⁴⁴⁸School of

542 Neurobiology, Biochemistry and Biophysics, Faculty of Life Sciences and Sagol School of
543 Neuroscience, Tel Aviv University, Tel Aviv, Israel, ⁴⁴⁹Department of Psychology III, Julius-
544 Maximilians-Universität Würzburg, Würzburg, Germany, ⁴⁵⁰Department of Psychology,
545 University of Mannheim, Mannheim, Germany, ⁴⁵¹School of Economics, University of
546 Nottingham, Nottingham, United Kingdom, ⁴⁵²Department of Psychology, University of Mainz,
547 Mainz, Germany, ⁴⁵³Education Sciences Department, University of Potsdam, Potsdam,
548 Germany, ⁴⁵⁴Center for Brain, Biology and Behavior, Department of Psychology, University of
549 Nebraska-Lincoln, Lincoln, NE, USA, ⁴⁵⁵Department of Educational Sciences and Psychology,
550 Freie Universität Berlin, Berlin, Germany, ⁴⁵⁶Wu Tsai Neurosciences Institute, Stanford
551 University, ⁴⁵⁷MZES, University of Mannheim, Mannheim, Germany, ⁴⁵⁸Department of
552 Sociology, University of Warwick, Coventry, United Kingdom, ⁴⁵⁹School of Politics and
553 International Relations, Australian National University, Canberra, ACT, Australia,
554 ⁴⁶⁰Department of Psychology, University of Innsbruck, Innsbruck, Austria, ⁴⁶¹Division of
555 Psychology and Language Sciences, University College London, London, United Kingdom,
556 ⁴⁶²Department of Social Sciences and Policy Studies, Worcester Polytechnic Institute,
557 Worcester, MA, USA, ⁴⁶³Faculty of Sport, Technology and Health Sciences, St Mary's
558 University, Twickenham, London, UK, ⁴⁶⁴Key Laboratory of Brain-Machine Intelligence for
559 Information Behavior (Ministry of Education and Shanghai), School of Business and
560 Management, Shanghai International Studies University, Shanghai, China, ⁴⁶⁵Department of
561 Economics, University of Essex, Colchester, United Kingdom, ⁴⁶⁶School of Psychology,
562 University of Sussex, Brighton, United Kingdom, ⁴⁶⁷Department of Psychology, San Diego State
563 University, San Diego, CA, USA, ⁴⁶⁸Department of Quantitative Intelligence, Feinstein Institutes
564 for Medical Research, New York, NY, USA, ⁴⁶⁹Social Research Institute, University College
565 London, London, United Kingdom, ⁴⁷⁰School of Business and Economics, RWTH Aachen
566 University, Aachen, Germany, ⁴⁷¹Department of Experimental Psychology, University College
567 London, London, United Kingdom, ⁴⁷²Department of Management, Society and
568 Communication, Copenhagen Business School, Copenhagen, Denmark, ⁴⁷³Institute of
569 Psychology, Osnabrück University, Osnabrück, Germany, ⁴⁷⁴Department of Social Sciences,
570 Otto von Guericke University Magdeburg, Magdeburg, Germany, ⁴⁷⁵Faculty of Social Science,
571 RPTU University of Kaiserslautern-Landau, Kaiserslautern, Germany, ⁴⁷⁶Department of
572 Educational Science, University of Groningen, Groningen, The Netherlands, ⁴⁷⁷School of
573 Psychology, Keele University, Keele, United Kingdom, ⁴⁷⁸School of Business, Stevens Institute
574 of Technology, Hoboken, NJ, USA, ⁴⁷⁹Centre for Fertility and Health, Norwegian Institute of
575 Public Health, Oslo, Norway, ⁴⁸⁰Jackson School of Global Affairs, Yale University, New Haven,
576 CT, USA, ⁴⁸¹NSI, Inc., ⁴⁸²Department of Communication Studies, Northwestern University,
577 Evanston, IL, USA, ⁴⁸³Department of Computer Science, ⁴⁸⁴Department of Marketing, Clemson
578 University, Clemson, SC, USA, ⁴⁸⁵Department of Social & Human Research, Romanian
579 Academy, Cluj-Napoca, Romania, ⁴⁸⁶Babeş-Bolyai University, ⁴⁸⁷Institute of Psychology,
580 Jagiellonian University, Krakow, Poland, ⁴⁸⁸Center for Law and Economics, ETH Zürich,
581 Zürich, Switzerland, ⁴⁸⁹Education, Research, Statistics, and Evaluation, University of Virginia,
582 Charlottesville, VA, USA, ⁴⁹⁰History and Art History, Utrecht University, Utrecht, The
583 Netherlands, ⁴⁹¹Graduate School of Advanced Science and Engineering, Hiroshima University,
584 Higashihiroshima, Japan, ⁴⁹²Business, Operations and Strategy School, University of
585 Greenwich, London, United Kingdom, ⁴⁹³Department of General Psychology, University of
586 Padova, Padova, Italy, ⁴⁹⁴Department of Sociology, University of Novi Sad, Novi Sad, Serbia,
587 ⁴⁹⁵Leiden Institute of Brain and Cognition, Leiden University, Leiden, The Netherlands,

588 ⁴⁹⁶Business, LAB University of Applied Sciences, Lappeenranta, Finland, ⁴⁹⁷Visiting
589 Researcher, LUT University, Business School, Finland, ⁴⁹⁸Turku School of Economics,
590 University of Turku, ⁴⁹⁹Department of Psychiatry and Behavioral Sciences, Stanford University,
591 Palo Alto, CA, USA, ⁵⁰⁰Department of Sociology and Social Psychology, University of Cologne,
592 Cologne, Germany, ⁵⁰¹Research Group Neural Circuits, Consciousness and Cognition, Max
593 Planck Institute for Empirical Aesthetics, Frankfurt am Main, Germany, ⁵⁰²Department of
594 Cognitive Neuroscience, Faculty of Psychology and Neuroscience, Maastricht University,
595 Maastricht, The Netherlands, ⁵⁰³Human Resource Studies, Tilburg University, Tilburg, The
596 Netherlands, ⁵⁰⁴Department of Psychology, University of Basel, Basel, Switzerland,
597 ⁵⁰⁵Department of Organizational Behavior, INSEAD, Singapore, Singapore, ⁵⁰⁶Independent
598 Researcher, , United Kingdom, ⁵⁰⁷Center for Social and Cultural Psychology, Université Libre
599 de Bruxelles, Brussels, Belgium, ⁵⁰⁸Optentia Research Unit, North-West University,
600 Vanderbijlpark, South Africa, ⁵⁰⁹Department of Sociology, Utrecht University, Utrecht, the
601 Netherlands, ⁵¹⁰Methodology and Statistics unit, Institute of Child and Education Studies, Leiden
602 University, Leiden, The Netherlands, ⁵¹¹Department of Economics, Lund University, Lund,
603 Sweden, ⁵¹²Department of Psychology, Ithaca College, Ithaca, NY, USA, ⁵¹³Department of
604 Humanistic Studies, University of Urbino, Urbino, Italy, ⁵¹⁴Department of Political Science,
605 University Duisburg-Essen, Duisburg, Germany, ⁵¹⁵IRCCS San Camillo Hospital, Lido Venice,
606 Italy, ⁵¹⁶Vaccine and Infectious Disease Division, Fred Hutchinson Cancer Center, Seattle, WA,
607 USA, ⁵¹⁷Amsterdam School of Communication Research, University of Amsterdam,
608 Amsterdam, The Netherlands, ⁵¹⁸Nuffield College, University of Oxford, Oxford, United
609 Kingdom, ⁵¹⁹Economics, Newcastle University Business School, Newcastle upon Tyne, United
610 Kingdom, ⁵²⁰Faculty of Medicine, Imperial College London, London, United Kingdom,
611 ⁵²¹Perceptive Discovery, Perceptive, London, UK., ⁵²²Arctic Centre, University of Lapland,
612 Rovaniemi, Finland, ⁵²³Darden School of Business, University of Virginia, Charlottesville, VA,
613 USA, ⁵²⁴Psychology, University of Michigan, Ann Arbor, MI, USA, ⁵²⁵Cologne Institute for
614 Information Systems, University of Cologne, Cologne, Germany, ⁵²⁶Department of Psychology
615 I, Clinical Psychology and Psychotherapy, University of Würzburg, Institute of Psychology,
616 Würzburg, Germany, ⁵²⁷Accounting and Finance Group, University of Edinburgh Business
617 School, Edinburgh, United Kingdom, ⁵²⁸Leibniz Institute for Financial Research SAFE,
618 Frankfurt, Germany, ⁵²⁹Psychological Sciences, Western Kentucky University, Bowling Green,
619 KY, USA, ⁵³⁰University of Kentucky Medical School, ⁵³¹Department of Psychological Sciences,
620 Swinburne University of Technology, Hawthorn, Victoria, Australia, ⁵³²Department of
621 Experimental Psychology, Ghent University, Ghent, Belgium, ⁵³³Department of Psychology,
622 Humboldt University of Berlin, Germany, ⁵³⁴Department of Business Economics, University of
623 the Balearic Islands, Palma, Spain, ⁵³⁵Cognitive Science Department, Central European
624 University, Vienna, Austria, ⁵³⁶Italian Institute of Technology, Genoa, Italy, ⁵³⁷Data and
625 Analytics, Texas State Technical College, Waco, TX, USA, ⁵³⁸IOE, UCL's Faculty of Education
626 and Society, Psychology and Human Development, University College London, London, United
627 Kingdom, ⁵³⁹Cameron School of Business, University of North Carolina Wilmington,
628 Wilmington, NC, USA, ⁵⁴⁰Department of Psychology, Chinese University of Hong Kong, Hong
629 Kong SAR, China, ⁵⁴¹Neuroimaging Center (NIC), Focus Program Translational Neuroscience
630 (FTN), Johannes Gutenberg University Medical Center, Mainz, Germany, ⁵⁴²Leibniz Institute
631 for Resilience Research (LIR), Mainz, Germany, ⁵⁴³Department of General and Transplantation
632 Surgery, Medical University of Warsaw, Warsaw, Poland, ⁵⁴⁴Department of Psychology,
633 Ludwig-Maximilians-Universität München, München, Germany, ⁵⁴⁵Universitas Airlangga,

634 ⁵⁴⁶Department of Psychology, Ohio University, Athens, OH, USA, ⁵⁴⁷Research Group Neural
635 Circuits, Consciousness, and Cognition, Max Planck Institute for Empirical Aesthetics, Frankfurt
636 am Main, Germany, ⁵⁴⁸Institute of Psychology, Goethe University of Frankfurt, Frankfurt am
637 Main, Germany, ⁵⁴⁹Department of Psychology, Claremont McKenna College, Claremont, CA,
638 USA, ⁵⁵⁰Independent Researcher, Washington, DC, USA, ⁵⁵¹Department of Psychology,
639 University of Milano-Bicocca, Milan, Italy, ⁵⁵²Department of Economics, Ben-Gurion
640 University of the Negev, Beer Sheva, Israel, ⁵⁵³Center for Open Science, Charlottesville, VA,
641 USA, ⁵⁵⁴University of Virginia

642 *Balazs Aczel and Barnabas Szasz are shared first and corresponding authors.

643 **Abstract**

644 The same dataset can be analysed in different justifiable ways to answer the same research
645 question, potentially challenging the robustness of empirical science¹⁻³. In this crowd initiative,
646 we investigated the degree to which research findings in the social and behavioural sciences are
647 contingent on analysts' choices. We examined a stratified random sample of 100 studies published
648 between 2009 and 2018, where for one claim per study, at least five re-analysts independently re-
649 analysed the original data. The statistical appropriateness of the re-analyses was assessed in peer
650 evaluations, and the robustness indicators were inspected along a range of research characteristics
651 and study designs. We found that 34% of the independent re-analyses yielded the same result
652 (within a tolerance region of +/- 0.05 Cohen's *d*) as the original report; with a four times broader
653 tolerance region, this indicator rose to 57%. Regarding the conclusions drawn, 74% of analyses
654 were reported to arrive at the same conclusion as in the original investigation; 24% to no
655 effects/inconclusive result, and 2% to the opposite effect as in the original investigation. This
656 exploratory study suggests that the common single-path analyses in social and behavioural
657 research should not simply be assumed to be robust to alternative analyses⁴. Therefore, we
658 recommend the development and use of practices to explore and communicate this neglected
659 source of uncertainty.

660 **Keywords:** analytical variability, crowdsourcing science, data analysis, research credibility, robustness,
661 scientific transparency

662 **Introduction**

663 Over the past decade, social and behavioural scientists have been striving to enhance the
664 robustness, objectivity, and replicability of their findings through systemic reforms in the conduct
665 and communication of empirical research. Practices such as preregistration⁵, registered reports⁶,
666 multisite replications⁷, analytical reproducibility checks^{8,9}, and automated result validation
667 techniques¹⁰ have been investigated and recommended to produce robust and replicable findings.
668 An important aspect of robustness has yet to be systematically charted across these sciences: the
669 contingency of the results on researchers' analytical choices.

670 In a typical research pipeline, the collected empirical data are analysed by a single analyst or team,
671 and the published report presents a conclusion based on one analytical path, occasionally
672 accompanied by a few robustness tests. The peer review process aims to ensure that the analysis
673 approach meets the statistical and field-specific standards. However, this procedure does not

674 systematically ascertain whether justifiable alternative analytical choices could have led to
675 different results.

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

686 One way to explore analytical variability is to employ a *multiverse* methodology^{2,11} in which the
687 analyst conducts all combinations of analytic choices they are able to generate across a wide range
688 of reasonable scenarios. Alternatively, in the *multi-analyst* approach, multiple analysts analyse the
689 data following their best judgement. The latter approach requires more organisation, but it takes
690 advantage of alternative expert perspectives without the combinatorial expansion of the number of
691 results. A multi-analyst approach also examines naturally occurring variation, empirically
692 answering the counterfactual question of what might have happened if another investigator had
693 considered the same research question using the same data.

694 Multi-analyst projects^{3,12-24} have provided some evidence of the extent to which analysts'
695 individual choices influence the results and conclusions. From economics to neuroscience, these
696 explorations demonstrated that the robustness of empirical findings can be compromised by
697 researcher degrees of freedom²⁵. The estimates of previous multi-analyst studies suggest that the
698 variability in effect-size estimates attributable to analytical heterogeneity can exceed the
699 variability one would expect due to sampling error²⁶.

700 Do we know how robust published findings are to analytical choices across the social and
701 behavioural sciences? One could argue that multi-analyst projects so far have been purposefully
702 conducted in research areas with little consensus on the best analytical approach or were motivated
703 to demonstrate the potential effect of analytical choices and, therefore, may represent rare cases
704 where alternative analyses produce important differences in results. For example, perhaps the
705 datasets selected afforded greater researcher degrees of freedom than is typical, raising issues of
706 the generalizability of the findings to scientific research more broadly. Differences between
707 academic methodologies and fields also seem plausible - for example, the relatively simple
708 experiments sometimes used in social psychology and behavioural economics may contain fewer
709 analytic decisions than the complex longitudinal observational datasets used in macroeconomics
710 and finance, and thus be more analytically robust in general²². To the extent that this is the case,
711 the findings from the existing multi-analyst projects could be biased towards worst-case scenarios,
712 and the traditional analytical practice and review system may not require fundamental adjustments.
713 If, on the other hand, observed results are contingent on the analyst's choices across fields,
714 methodologies, and types of datasets, then the scientific literature could be less robust than is often
715 assumed. If so, the general practices of how we conduct, report, and review empirical analyses
716 should be reformed to address this source of uncertainty.

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

723 *Variability of the results*

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

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

736 A challenge in any multi-analyst project is to find a common metric that allows the results of the
737 different analyses to be compared. A practical solution is to transform the reported point estimates
738 into a standard effect size measure. Although these transformations have limitations and their
739 calculation relies on assumptions that may not hold in all considered analysis settings^{25,27-29}, for
740 the sake of comparability, we decided to compute Cohen's *d* for each re-analysis, wherever it was
741 feasible. (For an alternative approach, see Supplementary Fig. 1) The methods, materials, analysis
742 plan, peer evaluation, and data management strategy of the project were preregistered on the OSF
743 repository of the project (Deviations from the registered plan are reported and explained in the
744 'Deviations from preregistration' supplementary document). In our preregistration, we defined that
745 we consider two results qualitatively the same when their effect sizes are within the tolerance
746 region of ± 0.05 Cohen's *d*. However, below, we also report analyses with alternative tolerance
747 regions. Our results revealed how far the new estimates were from the original ones (Fig. 2a) and
748 how often the effect sizes of the re-analyses fell within this tolerance region (Fig. 2i).

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

757 Alternatively, we could define the tolerance region as the percentage of the given effect size. As
758 an additional robustness test, we varied the tolerance region between $\pm 5\%$ and $\pm 20\%$ but it
759 barely made any difference regarding the percentage of robust studies (Extended Data Fig. 1b).

760 We next considered whether these robustness results vary by the disciplines of the studies, the
761 study designs, the expertise of the analysts, their prior familiarity with the data, and the sample
762 size in the data. Fig. 2b and Fig. 2c show the results for the major disciplines in our sample (≥ 10
763 studies). For Fig. 2c, we created an effect-size estimate range for each study as the numerical
764 difference between the highest and lowest estimate of re-analysis effect sizes. In our reading, the
765 listed disciplines do not yield large differences in the robustness of the results. Still, it is reasonable
766 to think that the level of analytical robustness in different disciplines can be influenced by the type
767 of studies that are commonly conducted there. For example, one could conjecture that empirical
768 claims based on observational data show lower robustness of the conclusions since they likely
769 involve more researcher degrees of freedom in terms of viable analysis paths than experimental
770 research settings. Fig. 2d and Fig. 2e explore this question and indicate that the results of studies
771 with observational study designs have lower analytical robustness in our sample, relative to
772 experimental designs (also see Tables S5 and S6).

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

781 Re-analyzing published studies entails a potential risk of bias if the re-analysts' familiarity with a
782 given study influences their choice of analysis. Re-analysts reported that they were familiar with
783 the original study in only 8% of cases. Moreover, there was no more than 3% difference in
784 robustness between those who were and those who were not familiar with the original study (Fig.
785 2g). For both groups, around two-thirds of the estimates fell outside our tolerance region. Finally,
786 we were interested to see whether these robustness results would show a different pattern when
787 considering sample size, as one could assume that studies with larger sample sizes could offer
788 more robust results. Fig. 2h does not support this assumption as the density distributions of the
789 sample sizes for results that are within and outside of the tolerance region are virtually the same.
790 Therefore, studies with large sample sizes are not immune to analytical variability.

791 We next asked whether the re-analyses show a trend or shift in effect sizes compared to the results
792 of the original studies. If the re-analysis effect sizes randomly vary around the original effect size,
793 we would expect that they are larger or smaller than the original ones with an equal chance. Fig.
794 3a (re-analysis data trimmed at Cohen's $d \leq 5$) and 3b (Cohen's $d \leq 1$) indicate that re-analysis
795 effect sizes show a tendency to be smaller than the original effect sizes as reflected in their best-
796 fitting (least squares) line. The distribution of original and re-analysis effect sizes also supports
797 this, as the peak of the density distribution of the latter is markedly lower. The mean effect size of
798 the original results is 0.73 (Median = 0.43), whereas for the re-analysis it is 0.49 (Median = 0.35),
799 Cohen's d , computed on $d_s \leq 5$. This result is consistent with the possibilities that original authors
800 were biased to report larger effects than re-analysts, that re-analysts were biased to report smaller
801 effects than original analysts, or both.

802 *Variability of the conclusions*

803 Another focal question of our study was whether the re-analysts reached the same qualitative
804 conclusions as the original study analysts. To answer this question, we asked the re-analysts to

805 implement any statistical re-analysis they deemed most appropriate to test the original claim using
806 the original data, with the goal of arriving at a single conclusion. Across all individual re-analyses
807 ($n = 504$), 74% of analyses were reported to arrive at the same conclusion as in the original
808 investigation; 24% to no effects/inconclusive result, and 2% to an effect in the opposite direction
809 as in the original investigation (Fig. 4a).

810 Out of 100 re-analysed claims, 34% were robust to independent re-analysis, such that all re-
811 analysts reported that they found evidence for the originally reported claim. It is important to note,
812 however, that this result is contingent on the level of agreement we use to define analytically robust
813 findings. With a more liberal definition of analytical robustness, this value was 39% when
814 analytical robustness was defined as >80% re-analysis agreement with the original conclusion, and
815 it was 80% when this definition was >50% (the results with alternative levels of agreement are
816 displayed on Fig. 4j).

817 We examined whether these results show a different pattern when inspecting them along the
818 earlier-mentioned aspects of the analyses. Fig. 4b and Fig. 4c present the proportions of
819 conclusions that were robust in each of the listed disciplines. Just as in the case of analyses of
820 robustness of the statistical results, the listed disciplines do not manifest large differences in
821 robustness of the conclusions, whereas their robustness may be influenced by the study designs
822 most common in a given field or subfield. Fig. 4d supports this notion as it indicates that nearly
823 half of the conclusions from experimental studies remained robust upon independent re-analysis,
824 whereas less than one-third of observational studies yielded robust conclusions. Moreover, Fig. 4e
825 indicates that, although the majority of re-analyses for both study designs reached the same
826 conclusions as the original study, the figure was 13% higher for experimental studies than for
827 observational studies. Just as for the robustness of the results, we can ask whether the deviation
828 from the originally reported claim in terms of conclusions is explained by the re-analysts' lack of
829 analytical expertise. Fig. 4f shows no support for this conjecture when evaluating the pattern of
830 results as a function of self-reported statistical expertise. The same conjecture can be assessed by
831 considering the quality of the submitted statistical analyses that were evaluated by peer evaluators
832 on a subset of the analyses (see Methods). Fig. 4g shows that the proportion of inferentially robust
833 conclusions is numerically larger for analyses that were rated as medium-quality by peer evaluators
834 than for analyses that were rated as high-quality. Whether this pattern was a result of noise or
835 whether more sophisticated analyses are characterized by greater heterogeneity in approaches and
836 results should be the topic of future metascientific projects.

837 Just as for the analyses of the robustness of the statistical results, we were interested to see whether
838 these results show a different pattern when inspecting them as a function of analysts' prior
839 familiarity with the dataset. Although those familiar with the original study did report the same
840 conclusion in a higher proportion than those who were not familiar, 17% of their re-analyses still
841 indicated a conclusion different from the original one (Fig. 4h).

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

847 For descriptive information about the re-analysts, peer evaluators, and additional robustness
848 analyses, see Extended Data Figs. 2, 3, 4, and Supplementary Information's General descriptives,
849 Demographics of the re-analysts, Peer evaluation, and Robustness analyses sections.

850 **Limitations**

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

858 Although we conducted more than 500 analyses, our project included only five independent
859 analyses for most datasets, therefore, we do not know to what degree these analyses capture the
860 full variability of analyses and results for the given research question and dataset. Also, since we
861 re-analysed already published studies and the re-analysts were provided with these studies, the
862 original analysis pipeline could have anchored some of the choices of the re-analysts. On the other
863 hand, some analysts could have been motivated to produce alternative results, as it is a basic
864 incentive of scientists to say something new.

865 While Cohen's d has the advantage of being easy to compute and comparable across different
866 analyses, Kumpel and Hoffmann³⁹ recently proposed the concept of generalised marginal effects
867 (gMEs), an effect size metric that is both formally applicable and comparable across different
868 statistical models. We had not originally planned to calculate standardised gMEs, and, accordingly,
869 did not collect all required analysis outputs to compute them across the board. Still, we calculated
870 gMEs for a sample of our studies to showcase their potential for future multi-analyst studies
871 (Supplementary Fig. 1).

872
873 We presented some exploratory analyses, but there are many other factors to explore that could
874 contribute to analytical variability (e.g., topical expertise). Finally, despite our best efforts to
875 conduct quality checks on the re-analyses to ensure the soundness of the analytic strategies¹⁶, it is
876 possible that some of the discrepancies between the original and the new results are due to
877 weaknesses in the re-analysts' approach rather than equally justifiable alternative analysis
878 decisions. It is likewise possible that there are weaknesses in the original analysts' approaches. It
879 is unknown whether the quality control processes for the re-analysts resulted in better, worse, or
880 similar overall quality of analysis decisions as compared with the quality control processes for
881 original analysts' decisions. The declared statistical expertise of the re-analysts makes us believe
882 that the observed heterogeneity in analyses and outcomes is a good representation of variation in
883 informed analysis decision-making in social-behavioural research.

884 **Discussion**

885 Are published results in the social and behavioural sciences robust to independent re-analyses?
886 The present exploration shows considerable variability due to researcher degrees of freedom in
887 statistical choices. Overall, when independent researchers analysed the same research question on
888 the original data, 34% of studies remained robust to independent re-analysis in the sense that all

889 re-analysts arrived at the same conclusion as the original analyst or analyst team. Notably, the new
890 conclusions converged with the original ones in 74% of the individual re-analyses. Our descriptive
891 results suggest a number of hypotheses concerning the circumstances in which we could expect
892 greater analytical variability.

893 *Why can there be multiple answers?*

894 Faced with the variability in the analysts' effect-size estimates and conclusions, one intuitive
895 hypothesis is that the variation must be due to researcher characteristics, such as statistical or field-
896 specific knowledge. Previous multi-analyst studies found little to no effect of researcher-specific
897 characteristics, such as experience in the field or statistical expertise^{16,19,28}. Instead, they suggest
898 that analytic results are dependent on the particular choices that the analysts make among similarly
899 acceptable data processing and analysis choices²⁸. For example, when 46 independent analyst
900 teams analysed the same speech dataset to answer the same research question, the authors
901 concluded that "depending on the choice of how the speech signal is operationalised, researchers
902 might find evidence for or against a theoretically relevant prediction" (p. 21)²⁸.

903 In line with previous findings, our results showed no strikingly different patterns across self-
904 reported statistical expertise and experience in a matching field (see Fig. 2f, Extended Data Fig. 4,
905 Supplementary Table 5, 6, 7, 8, and 9). At the same time, the few analysts who reported that they
906 were familiar with the original article produced alternative results and conclusions at a comparable
907 rate. More importantly, our peer evaluation process did not indicate that the analytical variability
908 of the re-analyses was due to inadequate statistical practices. These results are in line with
909 Menkveld et al.²² in which the quality assessment of the proposed analysis pipelines did not
910 statistically explain the results.

911 Another line of thought would suggest that the lack of robustness in the original published results
912 reflects some conceptual ambiguity in the theories or methodology³⁰. Research hypotheses are
913 often short verbal expressions that do not force the specifications of the analyses. The
914 underspecification of claims³¹ could represent a major source of ambiguity in analytic decisions.
915 We could not test the role of hypothesis ambiguity in a controlled manner, but it is a plausible
916 contributor considering that social science theories often make general claims across many
917 variables, creating theory-laden choice points regarding how constructs are operationalised, and
918 how hypotheses are tested³².

919 Regarding methods, we explored our results by separating them by experimental and observational
920 study designs, and observed that the proportions of results and conclusions that were analytically
921 robust were 15-20% higher for the experimental studies. The estimated range of effect sizes was
922 also apparently wider for observational studies compared to experimental ones. This exploratory
923 finding motivates the hypothesis that the increased control over data collection circumstances and
924 the reduced number of variables in experimental versus observational research translate to more
925 limited analytic flexibility. Notably, however, there was still substantial statistical variability
926 among findings from experimental studies.

927 *Why do these findings matter?*

928 Where multiple acceptable analytical paths exist, researchers can use this freedom
929 opportunistically^{33,34} and bias the results towards desired findings ("myside bias"³⁵). The much-

930 discussed credibility challenges in the social and behavioural sciences stem partly from the
931 suspicion that the prevailing incentive systems for publication encourage researchers to report and
932 interpret empirical data to serve non-epistemic goals such as storytelling³⁶. Reform initiatives, such
933 as the preregistration of research and analysis plans, aim to decrease researcher degrees of freedom
934 to tweak the analytic method or the research question to the observed data. Would results in these
935 fields become markedly more credible if every study was preregistered? Since preregistration is a
936 protection against overfitting, we hypothesize that it would reduce or eliminate the observed
937 finding that original analyses showed stronger evidence for positive results than re-analyses.
938 However, we also hypothesize that preregistration would have little impact on the observed
939 heterogeneity across alternative analysis strategies since registering and following a single analytic
940 path constrains the analysts only from choosing opportunistically from the alternative analytical
941 paths. Still, it does not confer any unique statistical or epistemic status to the pre-selected analytic
942 path²⁷. Unexplored but alternative justifiable analyses applied to the same data could still lead to
943 very different results. The present exploration is clear about the presence of this variability in
944 approaches, results, and inferences in the social and behavioural sciences. Without exploring this
945 variability, authors cannot guarantee consumers of their research that the reported conclusions hold
946 a privileged status over alternative conclusions.

947 *What can we do?*

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

952 Two main types of solutions are (1) multi-analyst studies, such as our own, where multiple
953 investigators independently follow their own approach, and (2) the multiverse^{2,11,37} approach,
954 where one investigator or team performs numerous analyses across the set of reasonable pipelines.
955 Conducting exploratory studies to identify analytical uncertainties and holding out samples are
956 further advisable practices to tackle analytical variability.

957 Project leaders aiming to conduct multi-analyst studies can consult various tutorial papers and
958 guidelines. Aczel et al.³⁸ provide an expert consensus guideline on the entire life-cycle of multi-
959 analyst projects from recruiting suitable analysts, through conducting the project to the reporting
960 of the outcomes. Kümpel & Hoffmann³⁹ offer a framework for synthesising objective outcome
961 metrics. The Subjective Evidence Evaluation Survey⁴⁰ is a tool for systematically exploring and
962 quantifying subjective measures of evidence in multi-analyst studies, allowing analysis teams to
963 subjectively reflect on various aspects of evidence, such as coherence, robustness, and relevance,
964 as well as the quality of the research design and data.

965 Multiverse analysis is also useful, especially when the dataset cannot be shared with other research
966 groups due to confidentiality reasons or when there are insufficient human resources to recruit
967 several independent analysts. Several guideline papers help researchers conduct and interpret such
968 analyses^{2,37,41-43}.

969 Recently, many scholars have called for a stronger focus on replication in science⁴⁴. Similar to
970 preregistration, however, replications are unlikely to help address the robustness of results to
971 multiple analysis strategies as they intentionally repeat the same (or at least a very similar) analysis
972 path. In this sense, replications can help detect bodies of work in which authors may have leveraged
973 their researcher degrees of freedom to generate results that are in line with their own or the
974 journal's expectations. All other things being equal, a severely *p*-hacked literature should contain
975 fewer replicable findings. And yet, replicability does not eliminate analytical variability itself.
976 Nevertheless, having multiple studies creates an opportunity to observe if analytical variability is,
977 itself, replicable. For example, imagine that Study A provides evidence for a claim with Analysis
978 1 but not with Analysis 2. If several replications also find evidence for the claim with Analysis 1
979 but not with Analysis 2, then the analytic choices are directly implicated in how evidence for the
980 phenomenon is observed. However, if it is random across replications whether Analysis 1 or
981 Analysis 2 provides evidence for the claim, then the implications of the analytic variability are
982 very different. The combination of replications and robustness investigations will facilitate the
983 advancement of stronger theoretical underpinnings of the topics of study, and could reduce
984 analytical variability in the long run by creating a more direct mapping between theory and
985 measurement^{30,11}.

986 All in all, we argue that the scholarly communication system could foster more engagement with
987 systematic and transparent robustness testing. As a starting point, the research data shared openly
988 alongside codebooks and analysis scripts is a prerequisite for any assessment of analytical
989 robustness. Research findings of particular scientific or societal importance could be accompanied
990 by robustness reports⁴⁶ that summarise the results of alternative theory-motivated analytic choices
991 by independent analysts. This publication format already provides a platform for analysts to
992 scrutinise the fragility of the findings before they have a major impact on scholarship and policy(
993 see <https://scipost-staging.org/JRobustRep>).

994 ***What did we learn about the robustness?***

995 Our results support the view that the results in social and behavioural science studies are contingent
996 on the analyst's choices, and if an analyst reports a single result from a single analytical path, they
997 have not exhausted the possible answers that the dataset can provide. This finding aligns with the
998 conclusions drawn by Wagenmakers, Sarafoglou, and Aczel⁴, that the belief that "for any dataset,
999 there exists a single, uniquely appropriate analysis procedure" and "multiple plausible analyses
1000 would reliably yield similar conclusions" (p. 424) are no more than statistical myths. Without
1001 multi-analyst and multiverse approaches, the fragility of empirical findings remains.

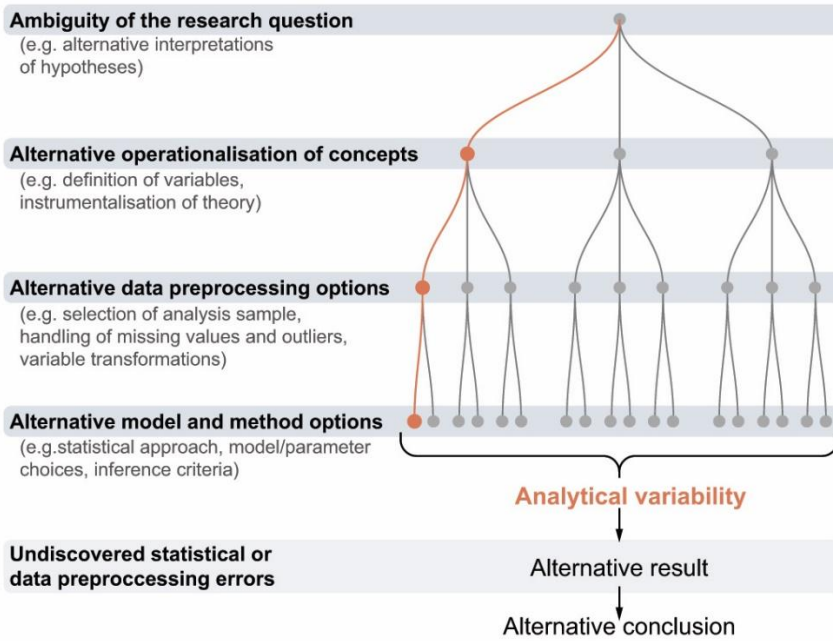
1002 Nonetheless, we emphasise that an optimistic or pessimistic interpretation is a matter of
1003 perspective and greatly depends on what evidential support we expect from a given study.
1004 Therefore, whether a result is satisfyingly robust will always depend on our epistemic needs and
1005 the precision we expect from our results. We caution against using blanket rules in aggregating or
1006 interpreting results across different analytical approaches within the same investigation.

1007 Objectivity is a fundamental ideal of science, implying that claims about the world should not be
1008 contingent on the predispositions of the claimant. What our results reveal is not that we must
1009 distrust or reject the results of the past, including the studies we analysed. Instead, they suggest
1010 that we should adopt greater caution about the evidence that single analytical paths can offer to

1011 support social and behavioural science claims. We believe that the limitations of "single-shot"
1012 analyses cut across numerous scientific disciplines. Methodological innovations, such as multi-lab
1013 collaborations, multi-analyst approaches, or multiverse methods, could increase the robustness of
1014 the social and behavioural sciences, and perhaps more broadly, in other empirical fields.

1015 **Figure legends**

1016 **Fig. 1**
1017 *Major sources of analytical variability.*



1018

1019 *Note.*

1020 Analytical variability can arise from the ambiguity of the research question, the alternative
1021 operationalisations of the concepts, variations in data preprocessing options, or model and
1022 method choices, as well as from undiscovered statistical or data processing errors.

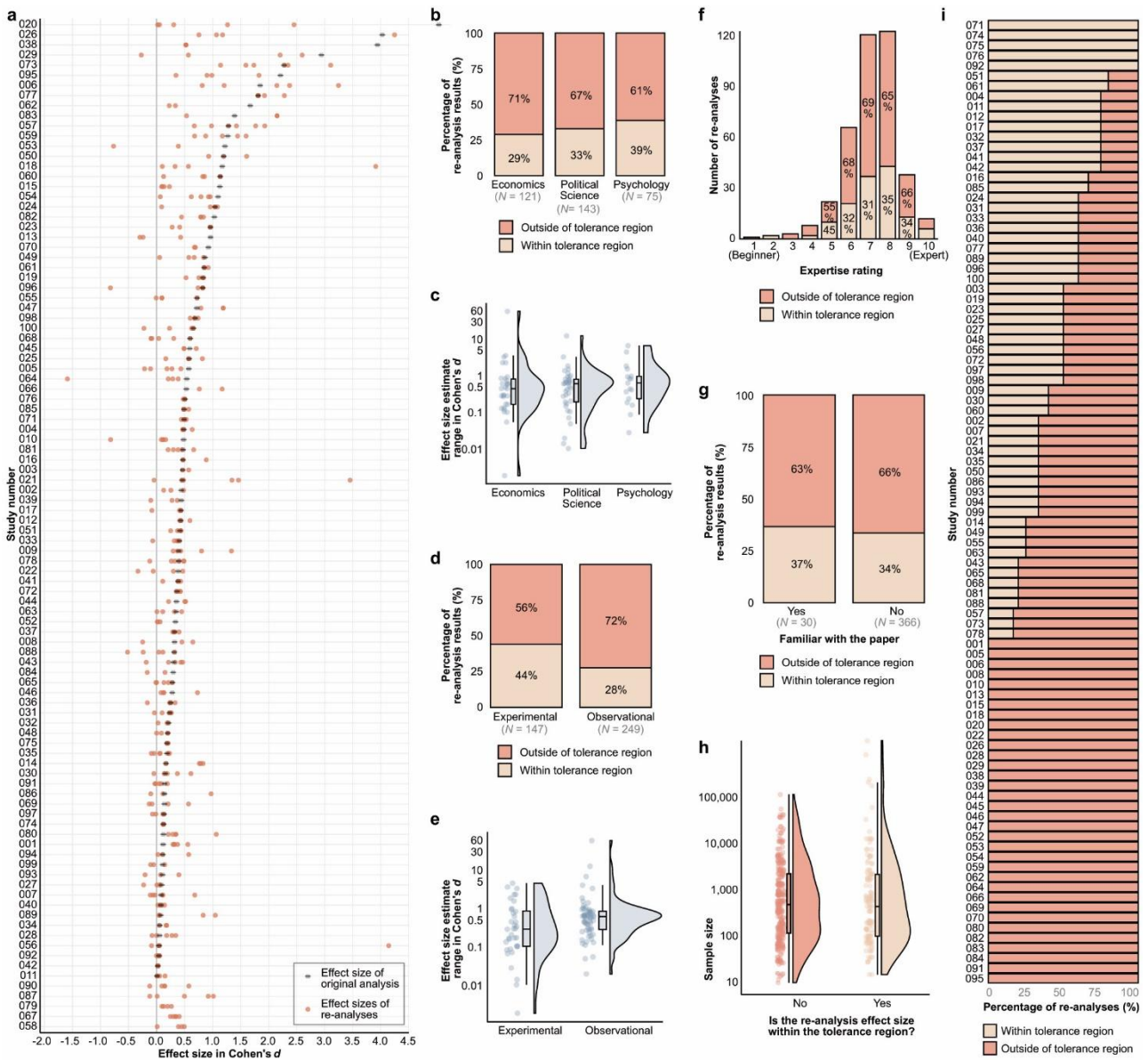
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Fig. 2
Analytical robustness of the statistical results.



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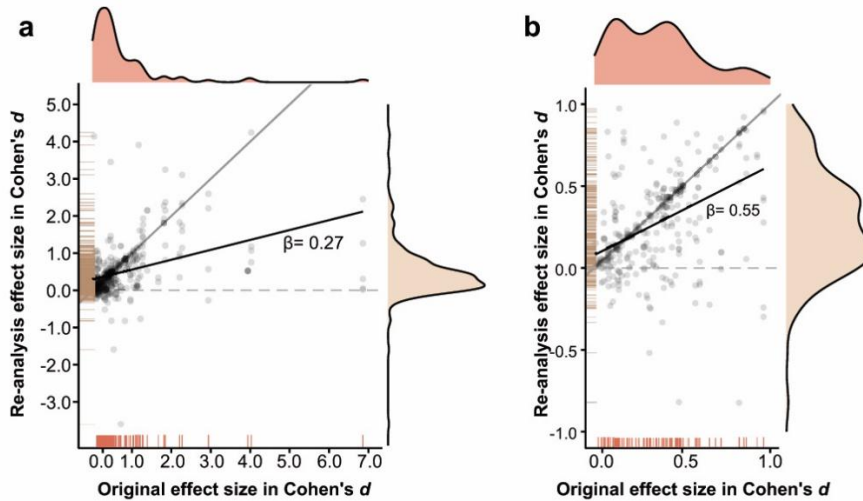
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Note.

a, Effect size of the original analysis (gray square; all represented as positive values) and the effect sizes of the re-analyses (red dot) for each study. The figure displays 415 re-analysis 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 due to missing information. Study numbers correspond to studies listed in <https://osf.io/mkwwhn>. The studies are ordered by the size of the original effect size. **b**, Percentage of re-analysis results falling within or outside of the tolerance region of the original results of the studies by major disciplines. The figure displays the count of re-analyses next to each discipline name. **c**, Distributions of effect-size estimate ranges calculated per study for each major discipline. **d**, Proportion of re-analysis results falling within or outside of the tolerance region of the original results of the studies by study type. The figure displays the count of re-analyses next to each discipline name. **e**, Distribution of effect-size estimate ranges calculated per study for observational and experimental studies. **f**, Percentage of re-analysis results falling within or outside of the tolerance region of the original results of the studies by self-rated

1042 expertise (1= beginner, 10 = expert). **g**, Percentage of re-analysis results falling within or outside of the
1043 tolerance region of the original results of the studies by declared familiarity with the study. **h**,
1044 Distribution of sample sizes separately for re-analysis effect sizes falling within or outside of the
1045 tolerance region of the original results. **i**, Proportion of effect sizes falling within the preset tolerance
1046 range (± 0.05 Cohen's d) for each study.

1047 **Fig. 3**
1048 *Original study effect size versus re-analysis effect size.*



1049 *Note.*
1050 The thin diagonal line represents an ideal case when the re-analysis effect sizes are equal to original
1051 effect size, the thick line shows the best-fitting (least squares) line of the displayed dots. Density plots
1052 of original ($n = 95$) and re-analysis ($n = 504$) effect sizes are parallel to their respective axis. β refers to
1053 the regression slope. Figure **a** shows effect sizes Cohen's $d \leq 5$, Figure **b** displays the same for effect
1054 sizes Cohen's $d \leq 1$.
1055

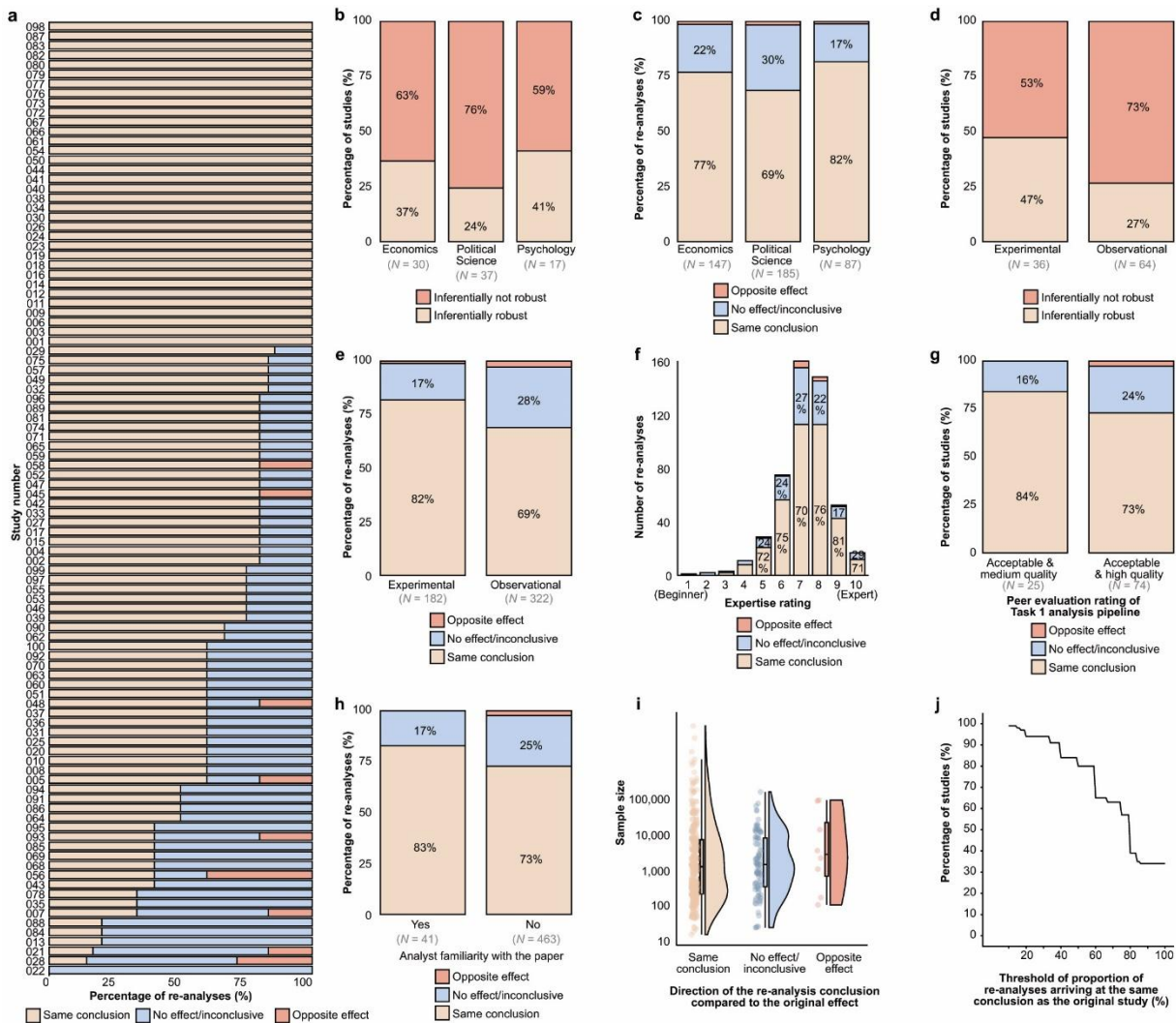
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Fig. 4
Analytical robustness of the conclusions.



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Note.

a, Proportion of same conclusion, no effect/inconclusive results, and opposite direction conclusions for each study. Study numbers correspond to studies listed in <https://osf.io/mkwhn>. **b**, Proportion of inferentially robust results (i.e., all re-analyses arrived at the same conclusion for the given study) by major disciplines (more than 10 studies in our collection: Economics, Political Science, and Psychology). **c**, Proportion of same effect, no effect/inconclusive results, and conclusions in the opposite direction of the original studies by major discipline. The number of re-analyses is displayed below each discipline. **d**, Proportion of inferentially robust results by study design (experimental vs. observational). The number of re-analyses is given below each study design. **e**, Proportion of same conclusion, no effect/inconclusive, and opposite effect of the re-analyses by study type (experimental, observational). **f**, Proportion of same conclusion, no effect/inconclusive, and opposite effect of the re-analyses 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 figure, we included only studies with more than one peer evaluation and where the peer evaluators agreed on their rating. The figure shows only the rating options with 5 or more re-analyses in that category. **h**, Proportion of same conclusion, no effect/inconclusive, and opposite effect of the re-analyses by declared familiarity with the study. **i**, Distribution of the sample size of the re-analyses resulting in the same conclusion, no effect/inconclusive, and opposite effects. Sample size values were

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1079 available for 345 re-analyses. **j**, Percentage of studies with robust conclusions above different levels of
1080 re-analysis consensus. Re-analysis consensus refers to the agreement among the conclusions drawn by
1081 the original study and the independent re-analyses.

1082 **Methods**

1083 All methods and procedures in this study were vetted by a panel of experts with prior experience
1084 in multi-analyst studies or who are specialists in the relevant methodology (see Additional Details
1085 of Method within Supplementary Information).

1086 **Preregistration**

1087 The methods, materials, analysis plan, peer evaluation, and data management strategy of the
1088 project were preregistered on the OSF. Deviations from the registered plan are reported and
1089 explained in the “Deviations from preregistration” supplementary document.

1090 *Ethical considerations*

1091 The datasets resulting from this project were not considered human subject research and are
1092 covered under an umbrella ethics protocol that was managed by the Center for Open Science
1093 (COS) (BRANY SBER IRB protocol #21-056-749), with concurrence from the United States
1094 Naval Information Warfare Center Pacific, HRPO. The institutional ethics board of the Faculty of
1095 Education and Psychology at Eötvös Loránd University, Budapest, Hungary, determined that the
1096 re-analysts are not considered research participants and that the project raises no ethical concerns.

1097 **Materials**

1098 *Selection of studies*

1099 The selection of studies was completed in two stages. In the first stage, the SCORE team created
1100 an initial study and claim collection. From this collection, we selected our sample using additional
1101 criteria.

1102 In the SCORE project, a stratified random sample of 600 articles was identified from a larger pool
1103 of randomly stratified ~30,000 articles from 62 journals, published between 2009 and 2018. The
1104 journals covered the main branches of social and behavioural sciences (criminology, economics,
1105 education, health-related, marketing/organisational behaviour, management, political science,
1106 psychology, public administration, sociology). To obtain the original studies, the following steps
1107 were taken: First, the paper was reviewed. If data and/or code were available, they were
1108 downloaded and saved into a project on OSF. If data and/or code were not available, the SCORE
1109 team attempted to contact the corresponding author to request that they share the data and code
1110 used for the original publication. Studies were excluded from the sample if they did not contain at
1111 least one inferential test using non-simulated, human data, where human data are defined at any
1112 level of human organisation (e.g., the individual person, family, political entity, firm, economic
1113 unit). The majority of the studies were tested for analytic *reproducibility* using the original
1114 specification, which is to be distinguished from *robustness* to alternative specifications. Analytic
1115 reproducibility was tested in cases when both original data and code were available ($n = 63$), or
1116 when the original data were available but the original code had to be adapted by the SCORE team
1117 in order to successfully reproduce the result ($n = 7$). If data were available but the original code

1118 was not, SCORE sourced a collaborating lab to generate new analytic code for the reproduction (n
1119 = 10). If data and code were not available, the collaborating lab used the secondary source data,
1120 which were shared upon request (acquired by SCORE), alongside newly generated analytical code
1121 for the reproduction (n = 11). Some reproductions were never attempted (n = 9). If the analytic
1122 reproduction failed, the paper was removed from the pool. Therefore, the present project focused
1123 solely on robustness to alternative specifications and did not conduct direct reproducibility checks
1124 using the original specification, as these had already been carried out by SCORE. Further details
1125 of the SCORE methodology (list of journals, selection process, etc.) are available in the original
1126 report⁴⁵.

1127 In the present work, a further requirement of the selected studies was to contain a single inferential
1128 statistical test result that corresponded to the claim with our instructions. Thus, we ensured that
1129 given the claim and the instructions, no other statistical result could correspond to the claim in the
1130 original article. If all potential claims from the study were too ambiguous and, therefore, could not
1131 be linked with a single inferential test statistic with the specification instructions, the study was
1132 excluded from our sample. The above-described study selection process was continued until we
1133 reached our target number of 100 studies, corresponding claims, and datasets.

1134 The selected studies and all available corresponding data and materials were made available to the
1135 re-analysts so that they could fully understand the selected claim and approach. There are trade-
1136 offs for how much information to give to the re-analysts to conduct re-analyses. Complete blinding
1137 of the original analysis strategy would ensure an entirely independent decision-making process
1138 about how to analyse the data. However, in much scientific writing, there is insufficient clarity in
1139 the description of the theoretical background, rationale, and specification of the conceptual model
1140 to be tested. In some papers, there is a clean break between these and clear hypotheses to test. In
1141 other papers, the narrative intermixes theoretical statements and analysis decisions and may not
1142 clearly state hypotheses or how they correspond with observed results. As a consequence, attempts
1143 to blind papers inevitably lead to variation in what is blinded across papers and many subjective
1144 decisions about what should be blinded (because it provides information about analysis strategy)
1145 and what can remain unblinded (because it provides information about theory and rationale). A
1146 major risk of those blinding decisions is that important information could be removed, which
1147 would weaken the re-analysts' ability to conduct a fair re-analysis of the original claim. As such,
1148 we opted for complete transparency of the original article so that no potentially important
1149 information was missing for the re-analysts, and we instructed re-analysts that they should create
1150 an analysis plan based on their own decisions for how best to assess the study's claim. On balance,
1151 this increases the risk of dependent decision-making but reduces the risk of misspecification of the
1152 hypothesis and rationale of the original research. In this context, we judged the latter to be a more
1153 important precondition for conducting an informative study.

1154 ***Claim selection***

1155 Claim selection was built on Phase 1 of the SCORE project effort. The claims identified for Phase
1156 1 of SCORE were executed according to a "single trace" approach, where only a single claim trace
1157 was extracted from the article, which corresponded to one statistically significant inferential test
1158 result (see https://docs.google.com/document/d/1yKvjMFaIcwCLq1k-02m_Y1G937yMIc_1JK4OfMay40/edit?usp=sharing). Within the current project, first, the lead
1159 team ensured that the extractions (i) are understandable, (ii) contain only one claim, (iii) indicate
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1161 the direction of the effect, (iv) there is a statistical hypothesis test-based result provided in the
1162 article that corresponds to the claim; and (v) the claim was phrased on a conceptual and not
1163 statistical level. If not, then they extracted the part of the claim that is relevant, or if this could not
1164 be achieved, they selected another more suitable sentence from the abstract, or if this could not be
1165 achieved, they searched for another suitable sentence from other parts of the article that could
1166 satisfy all of our criteria. When none of these steps presented a claim that satisfied the expectations,
1167 then the given article was not used in our study (for their list and explanation of dropping, see our
1168 data table). Where an expression of a claim has been judged by the lead team as ambiguous or
1169 rhetorical, they substituted the expression with an ellipsis mark (e.g., “dramatically increased” to
1170 “... increased”) while preserving the original wording and the meaning of the claim. Only in cases
1171 where, due to the selection, the wording of the claim became complicated, ungrammatical, or
1172 contained an ambiguous definition or an unexplained abbreviation, did the core team make
1173 necessary (and marked) adjustments in the grammar or wording of the claim while preserving the
1174 original meaning of the extraction. For example, the following selection “Three factors increase
1175 the salience of the proliferation threat: (1) prior violent militarized conflict...” was changed to
1176 “[prior violent militarized conflict] increase[s] the salience of the proliferation threat ...”. The list
1177 of claims can be found at <https://osf.io/mkwhn>.

1178 *Analysis instructions*

1179 For the re-analysts’ second task, instructions were needed in cases where the original paper
1180 contained more than one statistical analysis corresponding to the high-level claim, in order to be
1181 able to compare the new result to the one in the original paper. For this, the lead team prepared
1182 certain instructions (e.g., data selection, exclusions) that single out only one statistical result in the
1183 original paper. The instructions always remained circumstantial (e.g., data selection, exclusions,
1184 choice of measurement) and never gave direct instructions to the choice of statistical approach or
1185 full specification of the model.

1186 **Procedures**

1187 *Re-analyst recruitment*

1188 Our preregistered aim was to have at least five independent re-analyses carried out for each of the
1189 100 selected studies (Extended Data Fig. 5). Our choice of 5 analyses per study was led by practical
1190 considerations, as we judged that recruiting 500 analysts for a project is the limit of our capacity.

1191 Participation in the project was advertised on social media, at conferences, in mailing lists (e.g.,
1192 SCORE collaborator list), via personal networks, and in research newsletters. As a response to our
1193 recruitment call, 1141 researchers signed up to participate in our study. Out of these volunteers,
1194 459 signed up to analyse at least one dataset and submitted their work by the deadline or an
1195 extended deadline. From all the eligible volunteers, we selected re-analysts and peer evaluators on
1196 a first-come, first-served basis. The expectation of participation in the study was experience with
1197 conducting statistical analyses, and this was communicated to the volunteers from the start of the
1198 recruitment. Re-analysts were informed that they would qualify as authors on the publication of
1199 this study if (1) they completed their analyses and submitted all required materials and the post-
1200 analysis survey on time; (2) their analyses passed the peer evaluation, and (3) they reviewed and
1201 approved the manuscript in time.

1202 Re-analysts received a flat fee of 100 USD for each of their completed re-analyses (including both
1203 Task 1 and Task 2) if they submitted their work before March 2023, the deadline of the grant
1204 budget, unless they were from an embargoed country, in which case we were unable to transfer
1205 any payment. Peer evaluators received a flat fee of 10 USD per peer evaluation. Any further
1206 volunteers were informed that this payment did not apply to them.

1207 Upon joining the project, the volunteers for re-analysis were required to accept the project
1208 requirements. They were informed about (a) their tasks and responsibilities; (b) the project
1209 confidentiality agreements; (c) the plans for publishing the research report and presenting the data,
1210 analyses, and conclusion; (d) the conditions for an analysis to be included or excluded from the
1211 study; (e) that their names will be publicly linked to the analyses; (f) the re-analysts' rights to
1212 update or revise their analyses; (g) the project time schedule; and (h) the nature and criteria of
1213 compensation. Re-analysts were informed that, whereas they could consult other researchers
1214 during their analyses, they could not work in teams in this project. Before discussing the details of
1215 the analyses with others, the re-analysts were asked to ascertain that the person was not another
1216 re-analyst on that dataset. All communication materials of this study are openly available on the
1217 public repository of the project at <https://osf.io/nvy8a>.

1218 *Assignment of analyses and tasks*

1219 The following procedure was first piloted with two analysts to learn about the practical challenges
1220 and time demands of the following tasks. As the results of those analyses were not of central
1221 interest, we kept no records of them.

1222 First, each re-analyst was asked to assign themselves to one study, but at later rounds of
1223 recruitment, we allowed re-analysts to complete analysis on another paper, other than the one they
1224 completed earlier. They were asked to choose those studies where they saw the greatest relevance
1225 of their expertise. The authors of the original study could not be the re-analysts of that study.

1226 For several practical reasons, the re-analyses were not started at the same time for each study and
1227 each analyst. Firstly, it took us several rounds of recruitment to gather the target number of
1228 analyses for each study, mainly due to dropouts, delays, unplanned personal difficulties, and a
1229 shortage of staff. Secondly, our analysts found it difficult to retrieve, open, or interpret some of
1230 the datasets. In some cases, we had to reach out to the original authors, causing further delays in
1231 the project.

1232 The task of the re-analysts was to reflect on the corresponding claim (see claim selection) by re-
1233 analyzing the corresponding data. The re-analysts were provided with access to the datasets,
1234 extracted claims, the original articles, and all the corresponding materials. They were informed
1235 that their analyses should be conducted preferably with scripts that could reproduce all their results
1236 (including data preprocessing, extraction of test statistics and p-values/Bayes Factors, computing
1237 effect-size measures, etc.), but they could use the statistical software of their choice to produce an
1238 analysis script. Re-analysts were asked to write and structure their code such that others could
1239 understand their analysis scripts (e.g., by annotating the different analysis steps), and they were
1240 also informed that the analysis scripts from all analysts would be made publicly available with
1241 their names linked to the analyses.

1242 Re-analysts received two main tasks for each study, where Task 2 was given after the completion
1243 of Task 1. Once Task 1 was submitted, the analysts could not change the submission of Task 1
1244 unless they were asked by the lead team to provide some missing information from their analysis.

1245 **Task 1** The re-analysts were asked to reflect on the selected claim by re-analyzing the
1246 corresponding data. They could conduct and report as many analyses as they wished, but they had
1247 to draw a single conclusion from their analysis. They were asked to report their analyses and
1248 indicate whether their results provided evidence for the relationship/effect as claimed by the
1249 original study.

1250 **Task 2** For this task, the re-analysts had to produce only one statistical result corresponding to the
1251 claim they studied in Task 1, which would be compared to a statistical result in the original paper.
1252 The lead team provided certain instructions (e.g., data selection, exclusions) for this analysis to be
1253 able to compare the new result to one result in the original paper (see Analysis instructions section).
1254 Re-analysts were asked to report their results in terms of statistical families of r , z -, t -, F -, or χ^2
1255 tests (or their non-parametric versions). In addition, they were asked to report sample sizes (e.g.,
1256 per group) and the corresponding degrees of freedom. By this means, most results could be
1257 translated into standardized coefficients by the coordinators.

1258 The reason for requiring two analyses from the re-analysts was that they served two different aims.
1259 The results of Task 1 aimed to answer our first preregistered project question: “Do different
1260 analysts arrive at the same conclusions as the analyst of the original study?”, whereas the results
1261 of Task 2 aimed to answer our second preregistered project question: “Do different analysts arrive
1262 at the same effect estimates as the analyst of the original study?” We found that asking only one
1263 of the tasks would not have been sufficient to fully address both questions. In Task 1, researchers
1264 were not constrained to one analysis, so they could have produced more than one statistical result
1265 in order to draw a conclusion from the dataset. Therefore, in Task 1, it was not guaranteed that we
1266 would be able to select a single effect size from each analyst in order to answer our second project
1267 question. Another challenge to finding an answer to our second question was that in some of the
1268 original articles, one claim could have had more than one corresponding statistical result listed. In
1269 these cases, we prepared instructions for Task 2 in order to single out only one statistical result in
1270 the original paper. For example, if the original study contained two corresponding regression
1271 models, one with some exclusions and one with no exclusions, then we chose one of them (e.g.,
1272 the latter), and instructed the re-analysts not to apply any exclusions to the analysed data. In all
1273 other regards, re-analysts were free to conduct their calculations according to their best judgment.

1274 After completing the analysis and writing up the methods, results, and conclusion, re-analysts were
1275 expected to upload their analysis code (if available) to the corresponding OSF folder. Their
1276 reported methods, results, and conclusions were collected via an online form (see
1277 <https://osf.io/fjnhz/>). When uploading the materials, they were also asked to fill out a post-analysis
1278 survey. All major communications between the core project team and re-analysts from the study
1279 are openly available on the public repository of the project.

1280 **Peer evaluations**

1281 The goal of peer evaluation in this project was to assess whether the applied analytical choices are
1282 acceptable and whether the reported conclusion follows from the statistical results. By acceptable,

1283 we mean that peer evaluators agree that the analysis pipeline is within the variations that could be
1284 considered appropriate by the scientific community in addressing the given analytical task.

1285 The peer evaluation phase did not address potential errors in translating the description of the
1286 analytic methodology into analysis scripts. To mitigate potential gross errors in the analysis, peer
1287 evaluators were provided with a thorough and standardised description of the results and
1288 conclusions obtained using the described analysis, including sample sizes, the effect size, the test
1289 statistic, and degrees of freedom. From the description of the dataset, the description of the
1290 analysis, and the reported results and conclusions, peer evaluators were able to identify potential
1291 flaws in the implementation of the analysis that could stem from errors and/or mismatches.

1292 *Assignment of the analyses*

1293 When assigning the volunteer peer evaluators to analyses, the initial rule was that they should not
1294 evaluate any re-analyses conducted on datasets they had re-analysed as a re-analyst. In practice,
1295 for logistical reasons, this rule was applied in all but six cases (i.e., 99% of peer evaluations were
1296 carried out on a dataset that was different from the dataset they analysed themselves). They were
1297 asked to choose to evaluate those analyses where they see the greatest relevance of their expertise.
1298 If, after choosing a study to evaluate, a peer evaluator did not feel sufficiently skilled/experienced
1299 to judge whether the proposed analysis was acceptable, he/she was told not to fill out our template
1300 and should return the re-analysis to the pool and choose a new one.

1301 *Peer Evaluation Procedure*

1302 For details, see the corresponding section in the Supplementary Information.

1303 **Analysis methods**

1304 This exploratory study contains no inferential statistics. Besides the frequency- and proportion-
1305 based summary statistics, we calculated only the effect sizes of the results from the original articles
1306 and the re-analyses.

1307 *Cohen's d effect sizes*

1308 Following our preregistration, we converted all results into Cohen's *ds* wherever possible. For a
1309 number of cases, we could not achieve this due to missing information in the original studies or
1310 reported statistics that cannot be converted into Cohen's *d* (e.g., logistic regression). All the
1311 conversions are listed in the R scripts and the data documentation. All the original effect sizes are
1312 listed as positive values, and the re-analysis effect sizes are negative only when they showed an
1313 opposite effect compared to the original study.

1314 For further information on methods, see Supplementary Information.

1315 -----

1316 **Reporting summary**

1317 Further information on research design is available in the Nature Portfolio Reporting Summary
1318 linked to this article.

1319 **Data availability**

1320 Study data and materials are available on the project OSF (<https://osf.io/q5h2c/>) and GitHub
1321 repositories (<https://github.com/marton-balazs-kovacs/multi100/>). Archived data include the
1322 original datasets or a description of how to gain access to them. Our shared materials include
1323 all the survey questions and the general communication texts and instructions that we sent to
1324 the re-analysts and peer-evaluators. We excluded from our data files the email addresses of the
1325 re-analysts, as well as the records of those analysts who did not comply with the instructions
1326 and did not submit all the required analyses by the deadline. For further details about our
1327 exclusion criteria and procedure, see our Supplementary Information document.

1328 **Code availability**

1329 All analysis codes for this project are available at [https://github.com/marton-balazs-](https://github.com/marton-balazs-kovacs/multi100)
1330 [kovacs/multi100](https://github.com/marton-balazs-kovacs/multi100).

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1536 **Contributions**

1537 **Conceptualization:** B.A., B. Szaszi, F. Holzmeister, S. Hoffmann, G. Nilsonne, L.K., Z.A.T.,
1538 E.-J.W., T.M.E., and B.A.N.

1539 **Data curation:** H.T.C., M. Kovacs, M.A.V., and F. Venczel

1540 **Formal analysis:** B.A., B. Szaszi, H.T.C., M. Kovacs, F. Holzmeister, D.v.R., H.S.-K., S.
1541 Hoffmann, G. Nilsonne, Y.A., C.L.A., O.A.A., M. Adamkovic, T. Adamovich, K.A., A.M.A.,
1542 A.S.A.-B., A.H.A.-H., C.J.A., P.J.A., T. Alsalti, M. Altman, S. Alzahawi, E. Ambrosini, S.
1543 Anafinova, R.A., M. Angerer, A.A.-B., A. Antonietti, J.A., A. Arenas, M.M.A., F.A., M. Bachl,
1544 B. Bago, Š.B., B.J.B., E. Balayan, C.L.B., B. Banai, K.B., F.B., E. Baskin, N. Bault, C.W.B.,
1545 Q.H.B., M. Behnke, T.B., S.B., A. Bernard, U.B., P.A.B., A. Boldt, C.B.-R., A. Bouyamourn,
1546 O.B., L.B., J. Breuer, R.B., H.B., E. Buchanan, J. Buckenmaier, J. Buckley, J. Buczny, M.
1547 Burghart, B.H.B., N. Byrd, V. Cafarelli, P.C., T.C., K.C., A.M.C., G. Cepaluni, E.C., J.J.C., C.-
1548 c.C., X.C., S.S.C., F.C., H.C., V. Chirkov, D.C., B.C., S.G.C., C. Cohen, J. Collins, S.W.C., G.
1549 Corlazzoli, J. Cummins, C. Czymara, J.D.h., A.D.R., A.M.B.D., C.P.D., M.V.D., F.D.K.,
1550 J.R.d.L., T.R.d.V., R.D., F.D., E.E.D., M.D., V.D., S.D.-S., S.D., L.D., J.D., A.R.D., H.D.,
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1556 F. Hagemeister, M. Haigh, N. Hajdu, N. Hajimoladarvish, J.D.H., M. Hamjediers, R.M.H., M.
1557 Harma, N.R.H., Á.D.H., R.H.H., A.H., Ø.H., D.H., T. Heyman, J. Hicks, J. Hogeveen, J. Höppler,

1558 S.D.H., C. Huber, C. Hughes, T. Hummler, K.H., M.I., T.I., O.I., K.I., I.R.J., A. Jahn, M. Jain,
1559 A. Jakubow, D.J., J.J., M. Jekel, F.J., W.J.-L., R.J., A. Jones, S.J., P. Kačmár, C. Kaiser, Y.
1560 Kalaycı, J. Kantorowicz, A. Karabulut, J. Karch, H.K.-R., J.A.K., A. Kažemekaitytė, A. Kazlou,
1561 Z.K., J. Kim, B.K.-D., K.N.K., J.W.K., C. Koba, M. Kołczyńska, P. Koliás, M.K.G., M.
1562 Korbmacher, Ž.K., M. Kowal, A. Kretzschmar, V.K., A.-M.K., M. Kubsch, Y. Kunisato, D.
1563 Lacko, J.R.L., M. Lange, H.L., D. Lee, S. Lee, E.P.L.J., D. Lempert, A. Leo, E.L., J.M.L., P. Li,
1564 J. Lin, L.L., D. Lisovoj, M. Liu, S. Liu, T.L., S.L.I., P. Lodder, R.L.-B., R.L.-N., K.L., N.M.L.,
1565 A. Lovakov, J.G.L., J. Ludwig, F.L., J. Lukavský, C.L., X.L., E.M., M. Máčel, M.L.M., C.R.M.,
1566 A. Mädebach, J.M.-K., D.J.M., I.M., T.M., M.M. Marini, D.M.F., M. Martínez, M. Martinoli,
1567 A. Masiliunas, S. Massoni, K.C.M., S. Mayer, D. Mayer, M. Mayer, E.M.M., I.M.M., A.L.M.,
1568 M.M. McIntyre, P. McKee, A.N.M., P.F.M., H.M., C. Merkle, R.M., M.P.M., P. Michaelsen,
1569 G.M., W.M., P. Millroth, K.G.M., M. Misiak, Y.L.M., D. Moreau, C. Moreh, C. Morvinski,
1570 F.M., T.N., C.N., E.N., G. Navarrete, S.N., R.N., M.N., E.N.-S., Y.A.N., G. Niso, B.N., M.O.,
1571 K. Ong, A.I.O., C.O., K. Otten, S. Pandey, M. Pantazi, P. Papale, P. Pärnamets, S. Pauer, Y.G.P.,
1572 S. Pawel, J.E.P., H.K.P., A. Peez, F.P., B.D.P., B.P., J. Petter, J. Pfänder, G.P., J. Phillips, M.T.P.,
1573 A. Pirrone, I.L.P., A. Plachti, I.S.P., M. Ploner, M.M.H.P., S. Porcher, P. Präg, A.A.Y.P., J.
1574 Pugel, R.P., M. Püske, S. Radkani, L. Raes, I. Rafäi, K. Raiber, S. Rathje, M. Reshetnikov, C.J.R.,
1575 J.P.R., K. Rigaud, C.R., S. Rivera, O.R., R.R.-C., I. Ropovik, L. Röseler, R.M.R., A.R., F.F.R.,
1576 F.R., M. Rusconi, I. Russo, A.H.J.S., J. Salamon, M. Samahita, A. Sanaei, A. Sangchooli, M.
1577 Scandola, H.S., M. Schaerer, E. Schares, H.T.S., X.S., K.S., M.R.S., J.M.S., A.-L.S., B.
1578 Schuetze, D.H.S., L. Schulze, S.T. Schwartz, N.S., B. Scoggins, Y.S., R.S., S.T. Shaw, J. Shaw,
1579 Q.S., C.S., M. Sladekova, A. Somo, A. Sondhi, B. Sonmez, L. Spantig, M. Speekenbrink, A.
1580 Stamos, L. Stasielowicz, L.C.S., S.R.S., A.H.S., C.N.H.S., J.W.S., H.F.S., J. Sundquist, V.S.,
1581 S.D.S., P.S., R.D.S.-C., E. Szumowska, A. Tacconelli, E. Talbert, J.P.T., J.N.T., M.T., E.
1582 Toffalini, A. Tomašević, S.T., L. Torkkeli, L. Tozzi, J.T., A. Trinidad, D.T., K.T., M.U., K.U.,
1583 J.V.A., K.v.L., R.v.V., L.A.V., M.V., P.V., A.V., E.V., F. Votta, A. Waldendorf, M.J.W.,
1584 M.B.W., H.W., K.W., I.W., Y.A.W., M. Weinmann, M. Weiss, C.W., A. Wichman, B.J.W.,
1585 D.W., T.K.A.W., M. Woźniak, J.D.W., W.Y., J.N.W., T.Y., S.K.Y., K.S.L.Y., M.Z., R.A.Z.,
1586 X.Z., Z.Z., S.Z., C. Ziller, D.Z., C. Zogmaister, and R.i.Z.

1587 **Funding acquisition:** B.A., B. Szaszi, T.M.E., and B.A.N.

1588 **Investigation:** L.A.V.

1589 **Methodology:** B.A., B. Szaszi, H.T.C., M. Kovacs, F. Holzmeister, D.v.R., H.S.-K., S.
1590 Hoffmann, G. Nilsonne, L.K., Z.A.T., C.J.A., J.A.B., R.B.-N., A.D., M.A.G., M.H.K., R.A.P.,
1591 A. Sarafoglou, T.S., D.R.S., E.L.U., M.A.L.M.v.A., N.N.N.v.D., E.-J.W., T.M.E., and B.A.N.

1592 **Project administration:** B.A., B. Szaszi, H.T.C., M. Kovacs, L.K., Z.A.T., N.F., and T.M.E.

1593 **Resources:** B.A., B. Szaszi, H.T.C., F. Holzmeister, L.K., L.D., N.F., T.M.E., and B.A.N.

1594 **Software:** M. Kovacs

1595 **Supervision:** B.A., B. Szaszi, H.T.C., F. Holzmeister, D.v.R., S. Hoffmann, G. Nilsonne, C.J.A.,
1596 J.A.B., R.B.-N., A.D., M.A.G., M.H.K., A.N., R.A.P., A. Sarafoglou, T.S., D.R.S., E.L.U.,
1597 M.A.L.M.v.A., N.N.N.v.D., E.-J.W., T.M.E., and B.A.N.

1598 **Validation:** X.C., D.D., and S.J.

1599 **Visualization:** B.A., B. Szaszi, H.T.C., and M. Kovacs

1600 **Writing - original draft:** B.A., B. Szaszi, H.T.C., M. Kovacs, and L.K.

1601 **Writing - review & editing:** B.A., B. Szaszi, H.T.C., M. Kovacs, F. Holzmeister, D.v.R., H.S.-

1602 K., S. Hoffmann, G. Nilsonne, L.K., Z.A.T., Y.A., C.L.A., O.A.A., E. Acem, M. Adamkovic, T.

1603 Adamovich, K.A., L.A., A.M.A., A.S.A.-B., A.H.A.-H., C.J.A., P.J.A., T. Alsalti, M. Altman,

1604 S. Alzahawi, E. Ambrosini, S. Anafinova, R.A., M. Angerer, A.A.-B., A. Antonietti, J.A., A.

1605 Arenas, M.M.A., F.A., M. Bachl, B. Bago, Š.B., B.J.B., E. Balayan, C.L.B., B. Banai, K.B., F.B.,

1606 E. Baskin, J.A.B., N. Bault, C.W.B., Q.H.B., M. Behnke, T.B., S.B., A. Bernard, U.B., P.A.B.,

1607 A. Boldt, C.B.-R., R.B.-N., A. Bouyamourn, O.B., L.B., J. Breuer, R.B., H.B., E. Buchanan, J.

1608 Buckenmaier, J. Buckley, J. Buczny, M. Burghart, B.H.B., N. Byrd, V. Cafarelli, P.C., T.C.,

1609 K.C., A.M.C., G. Cepaluni, E.C., J.J.C., C.-c.C., X.C., S.S.C., F.C., H.C., V. Chirkov, D.C.,

1610 B.C., S.G.C., C. Cohen, J. Collins, S.W.C., G. Corlazzoli, J. Cummins, C. Czymara, J.D.h.,

1611 A.D.R., A.M.B.D., C.P.D., M.V.D., F.D.K., J.R.d.L., T.R.d.V., R.D., F.D., E.E.D., M.D., D.D.,

1612 V.D., S.D.-S., S.D., L.D., J.D., A.R.D., A.D., H.D., J.E.E., A.E., E. Efendic, J.E., M.M.E., M.E.,

1613 E. Estrada, L.E., T.R.E., A.F., E.M.F., L.F., F.M.F., J.L. Fiechter, M. Fišar, P.E.F.-K., M.

1614 Folwarczny, J.L. Fossum, V.R.F., R.F., D.F., J.F., A.C.F., J.D.G., L.C.G., C.M.G., B.G., S.M.G.,

1615 A. Gasiorowska, B.G.P., R.G., A. Geminiani, D. Geraldés, M.A.G., C. Giani, E.G., V.G., T.G.,

1616 A. Godefroidt, B.G.-B., A. Goreis, L.G.-V., M.G., D. Grigoryev, S.G., D.J.G., J.F.G.M., C.

1617 Guichet, L.G., H.H., A.C.H., S. Hafenbrädl, C. Häffner, F. Hagemester, M. Haigh, N. Hajdu,

1618 N. Hajimoladarvish, J.D.H., M. Hamjediers, R.M.H., M. Harma, N.R.H., Á.D.H., R.H.H., A.H.,

1619 Ø.H., D.H., T. Heyman, J. Hicks, J. Hogeveen, J. Höppler, S.D.H., C. Huber, C. Hughes, T.

1620 Hummler, K.H., M.I., T.I., O.I., K.I., I.R.J., A. Jahn, M. Jain, A. Jakobow, D.J., J.J., M. Jekel,

1621 F.J., W.J.-L., R.J., A. Jones, S.J., P. Kačmár, C. Kaiser, Y. Kalaycı, J. Kantorowicz, A.

1622 Karabulut, J. Karch, H.K.-R., J.A.K., A. Kažemekaitytė, A. Kazlou, Z.K., J. Kim, M.H.K., B.K.-

1623 D., K.N.K., J.W.K., C. Koba, M. Kołczyńska, P. Koliás, M.K.G., M. Korbmacher, Ž.K., M.

1624 Kowal, A. Kretschmar, V.K., A.-M.K., M. Kubsch, Y. Kunisato, D. Lacko, J.R.L., M. Lange,

1625 H.L., D. Lee, S. Lee, E.P.L.J., D. Lempert, A. Leo, E.L., J.M.L., P. Li, J. Lin, L.L., D. Lisovoj,

1626 M. Liu, S. Liu, T.L., S.L.I., P. Lodder, R.L.-B., R.L.-N., K.L., N.M.L., A. Lovakov, J.G.L., J.

1627 Ludwig, F.L., J. Lukavský, C.L., X.L., E.M., M. Máčel, M.L.M., C.R.M., A. Mädebach, J.M.-

1628 K., D.J.M., I.M., T.M., M.M. Marini, D.M.F., M. Martínez, M. Martinoli, A. Masiliunas, S.

1629 Massoni, K.C.M., S. Mayer, D. Mayer, M. Mayer, E.M.M., I.M.M., A.L.M., M.M. McIntyre, P.

1630 McKee, A.N.M., P.F.M., H.M., C. Merkle, R.M., M.P.M., P. Michaelsen, G.M., W.M., P.

1631 Millroth, K.G.M., M. Misiak, Y.L.M., D. Moreau, C. Moreh, C. Morvinski, F.M., T.N., C.N.,

1632 E.N., G. Navarrete, S.N., A.N., R.N., M.N., E.N.-S., Y.A.N., G. Niso, B.N., M.O., K. Ong,

1633 A.I.O., C.O., K. Otten, S. Pandey, M. Pantazi, P. Papale, P. Pärnamets, S. Pauer, Y.G.P., S.

1634 Pawel, J.E.P., H.K.P., A. Peez, F.P., B.D.P., B.P., J. Petter, J. Pfänder, G.P., J. Phillips, M.T.P.,

1635 A. Pirrone, I.L.P., A. Plachti, I.S.P., M. Ploner, R.A.P., M.M.H.P., S. Porcher, P. Prag, A.A.Y.P.,

1636 J. Pugel, R.P., M. Püski, S. Radkani, L. Raes, I. Rafäi, K. Raiber, S. Rathje, M. Reshetnikov,

1637 C.J.R., J.P.R., K. Rigaud, C.R., S. Rivera, O.R., R.R.-C., I. Ropovik, L. Röseler, R.M.R., A.R.,

1638 F.F.R., F.R., M. Rusconi, I. Russo, A.H.J.S., J. Salamon, M. Samahita, A. Sanaei, A. Sangchooli,

1639 A. Sarafoglou, M. Scandola, H.S., M. Schaerer, E. Schares, H.T.S., X.S., K.S., T.S., M.R.S.,

1640 J.M.S., A.-L.S., B. Schuetze, D.H.S., L. Schulze, S.T. Schwartz, N.S., B. Scoggins, Y.S., R.S.,

1641 D.R.S., S.T. Shaw, J. Shaw, Q.S., C.S., M. Sladekova, A. Somo, A. Sondhi, B. Sonmez, L.

1642 Spantig, M. Speekenbrink, A. Stamos, L. Stasielowicz, L.C.S., S.R.S., A.H.S., C.N.H.S., J.W.S.,
1643 H.F.S., J. Sundquist, V.S., S.D.S., P.S., R.D.S.-C., E. Szumowska, A. Tacconelli, E. Talbert,
1644 J.P.T., J.N.T., M.T., E. Toffalini, A. Tomašević, S.T., L. Torkkeli, L. Tozzi, J.T., A. Trinidad,
1645 D.T., K.T., M.U., E.L.U., K.U., J.V.A., M.A.L.M.v.A., N.N.N.v.D., K.v.L., R.v.V., M.A.V.,
1646 L.A.V., F. Venczel, M.V., P.V., A.V., E.V., F. Votta, E.-J.W., A. Waldendorf, M.J.W., M.B.W.,
1647 H.W., K.W., I.W., Y.A.W., M. Weinmann, M. Weiss, C.W., A. Wichman, B.J.W., D.W.,
1648 T.K.A.W., M. Woźniak, J.D.W., W.Y., J.N.W., T.Y., S.K.Y., K.S.L.Y., M.Z., R.A.Z., X.Z.,
1649 Z.Z., S.Z., C. Ziller, D.Z., C. Zogmaister, R.i.Z., N.F., T.M.E., and B.A.N.

1650 **Corresponding Authors**

1651 Correspondence and requests for materials should be addressed to Balazs Aczel and Barnabas
1652 Szaszi; E-mail: balazs.aczel@gmail.com and szaszi.barnabas@gmail.com.

1653 **Ethics declarations**

1654 *Competing interests*

1655 The authors declare no financial or non-financial competing interests.

1656 **SUPPLEMENTARY INFORMATION**

Content	Location
Project preregistrations	https://osf.io/4ev9t , https://osf.io/y4evp
Deviations from preregistration	https://osf.io/wfdxp
Supplementary information	https://osf.io/4ntua
Project OSF Repository	https://osf.io/q5h2c/
Project GitHub Repository	https://github.com/marton-balazs-kovacs/multi100

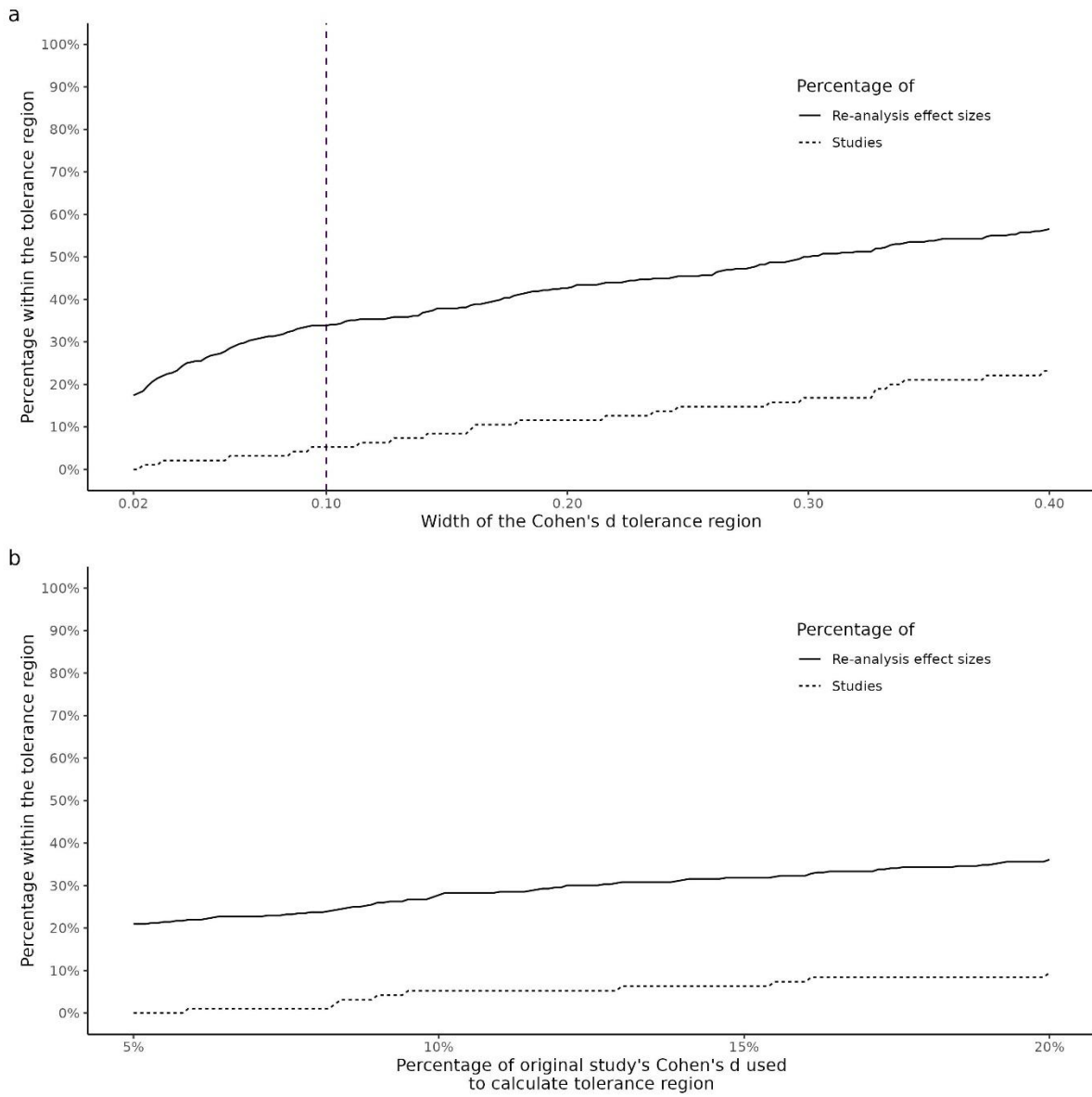
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1659 **Extended data figures**

1660 **Extended Data Fig. 1**

1661 *Robustness of the statistical results.*

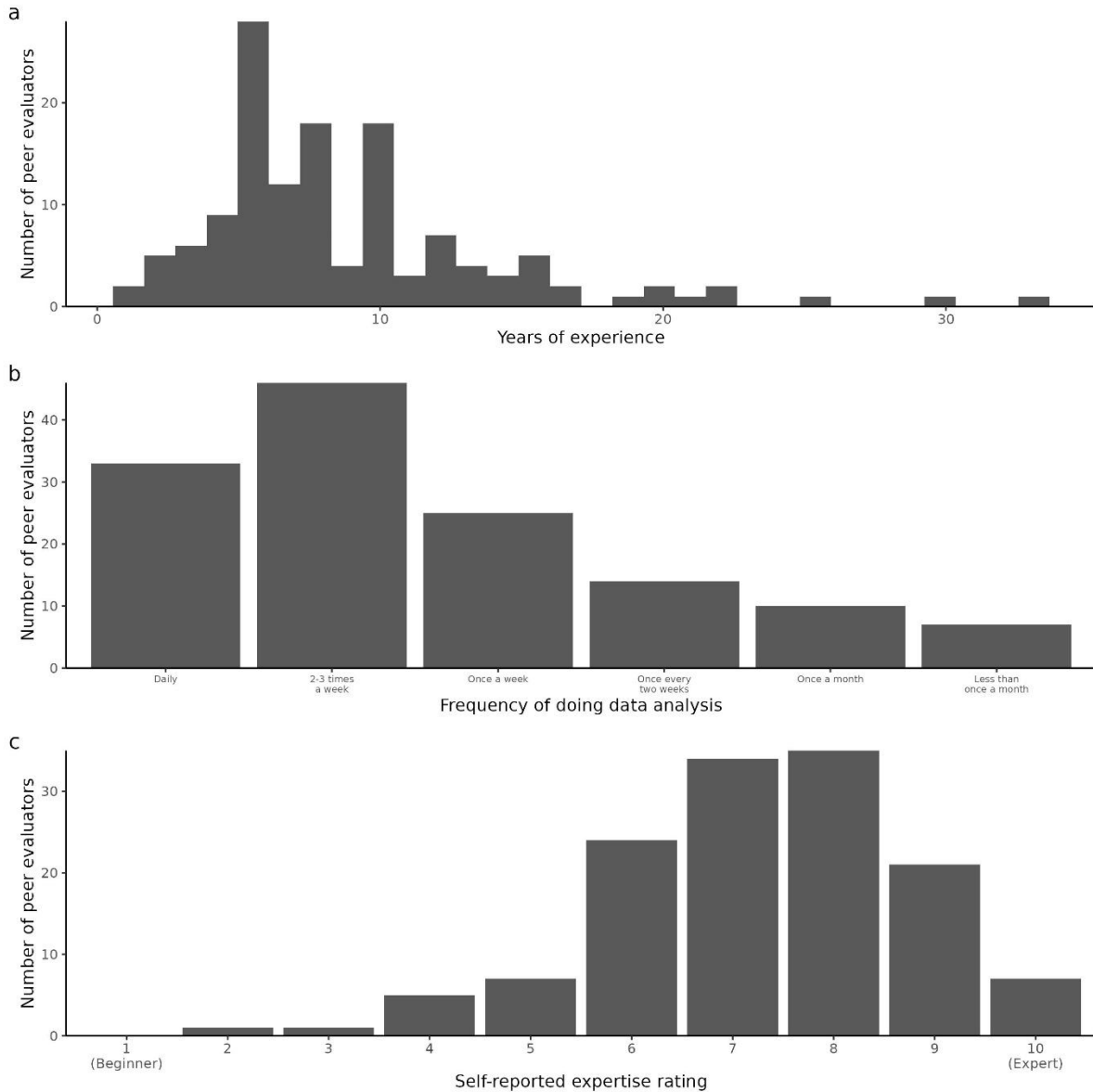


1662

1663 *Note.*

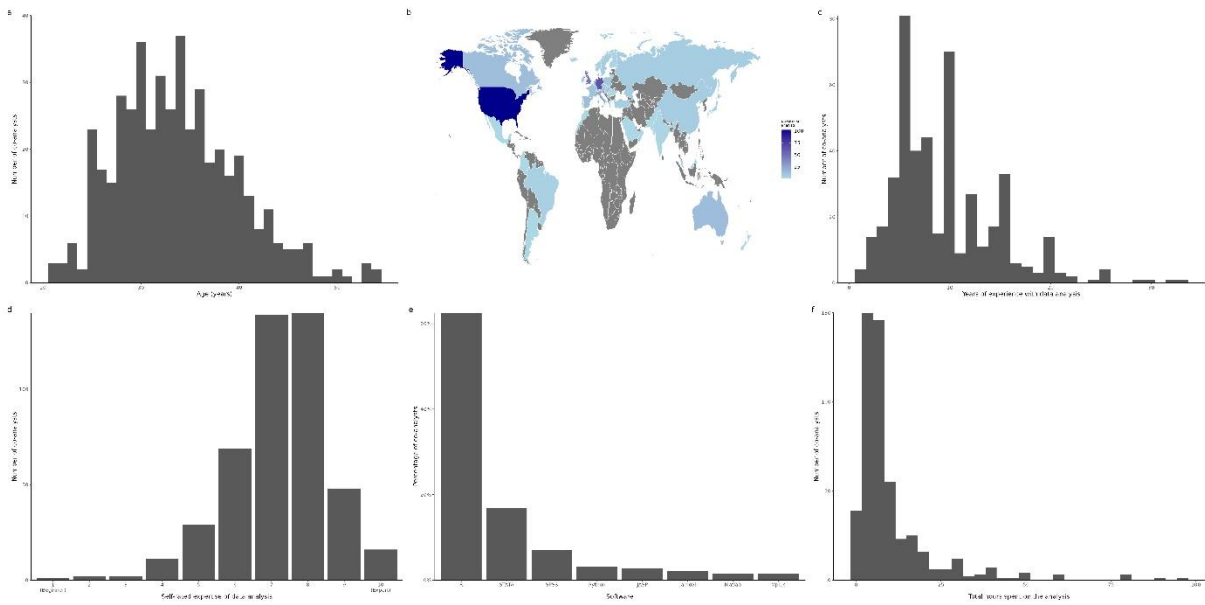
1664 **a**, Robustness of the statistical results with different widths ($\pm[0.01-0.20]$ Cohen's d) of the tolerance
1665 region. **b**, Robustness of the statistical results with different percentages (5-20%) of Cohen's d as a
1666 tolerance region. Calculations on the study and re-analysis levels are shown in different lines.

1667 **Extended Data Fig. 2**
 1668 *Descriptive statistics of the peer evaluators.*



1669
 1670 *Note.*
 1671 **a**, The peer evaluators' years of experience with data analysis. When a peer evaluator submitted more
 1672 than one evaluation and a year passed between the responses, we kept only their first response. **b**, The
 1673 regularity with which peer evaluators perform data analysis. **c**, The peer evaluators' self-rated level of
 1674 expertise in data analysis. When a peer evaluator submitted more than one re-analysis, we kept only their
 1675 first response.
 1676

1677 **Extended Data Fig. 3**
 1678 *Descriptive statistics of the analysts and the analyses.*

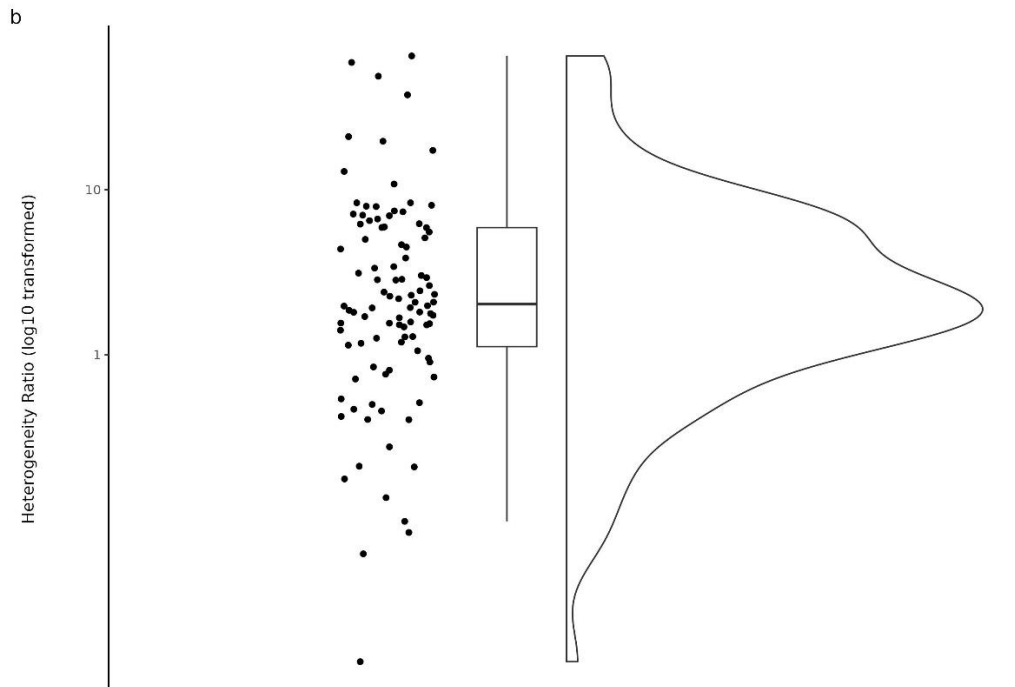
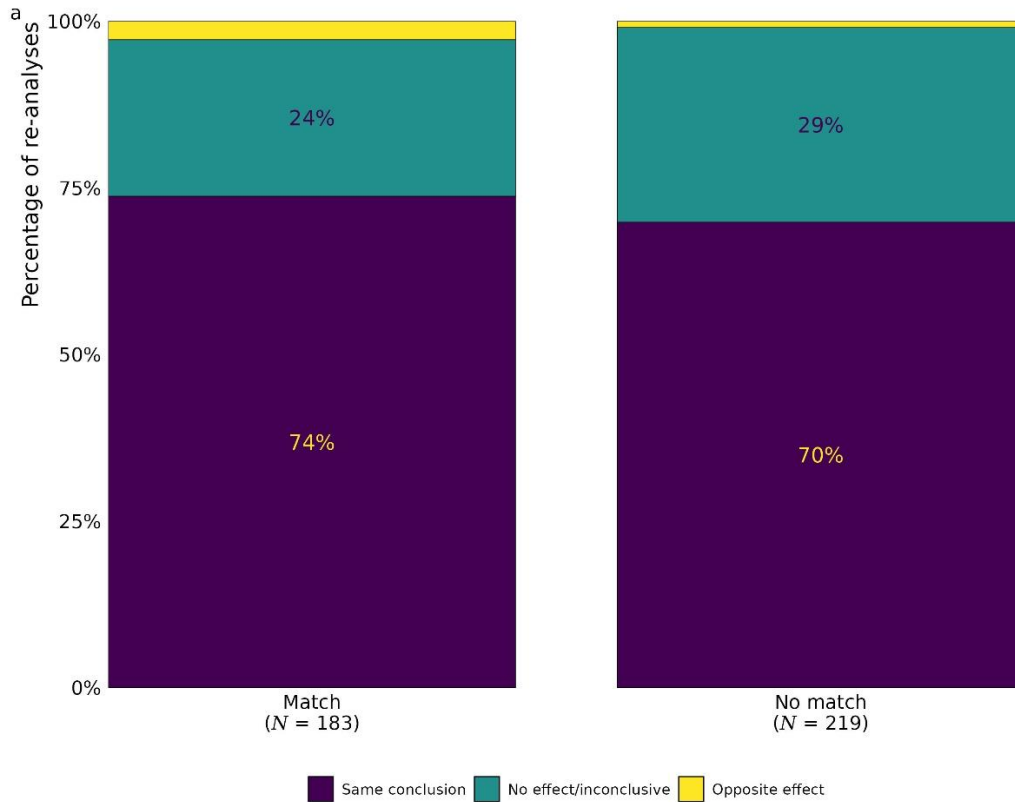


1679

1680 *Note.*

1681 **a**, The distribution of the analysts' age. When an analyst submitted additional re-analyses with a higher
 1682 reported age, we kept only their age at the time of their first submission. Moreover, one analyst is not
 1683 represented in the figure because they did not disclose their age. **b**, The analysts' country of residence.
 1684 When an analyst submitted more than one re-analysis, and they moved between the submissions, we
 1685 only kept their first response. **c**, The analysts' years of experience with data analysis. We only kept their
 1686 first response when an analyst submitted additional re-analyses with a higher reported age. **d**, The
 1687 analysts' self-rated level of expertise in data analysis. When an analyst submitted more than one re-
 1688 analysis, we only kept their first response. **e**, The software the analysts used for their re-analysis tasks.
 1689 In case an analyst completed multiple re-analyses or reported using multiple software applications, we
 1690 kept all their responses for this figure. The figure displays only software applications used by more than
 1691 1% of the analysts. **f**, The reported total hours the analyst spent on Task 1 and Task 2 together. In case
 1692 an analyst completed multiple re-analyses, we kept all their responses for this figure. One response was
 1693 excluded due to being an outlier (999 h), which we assumed was an error.

1694 **Extended Data Fig. 4**
1695 *Additional statistical results requested by the reviewers.*

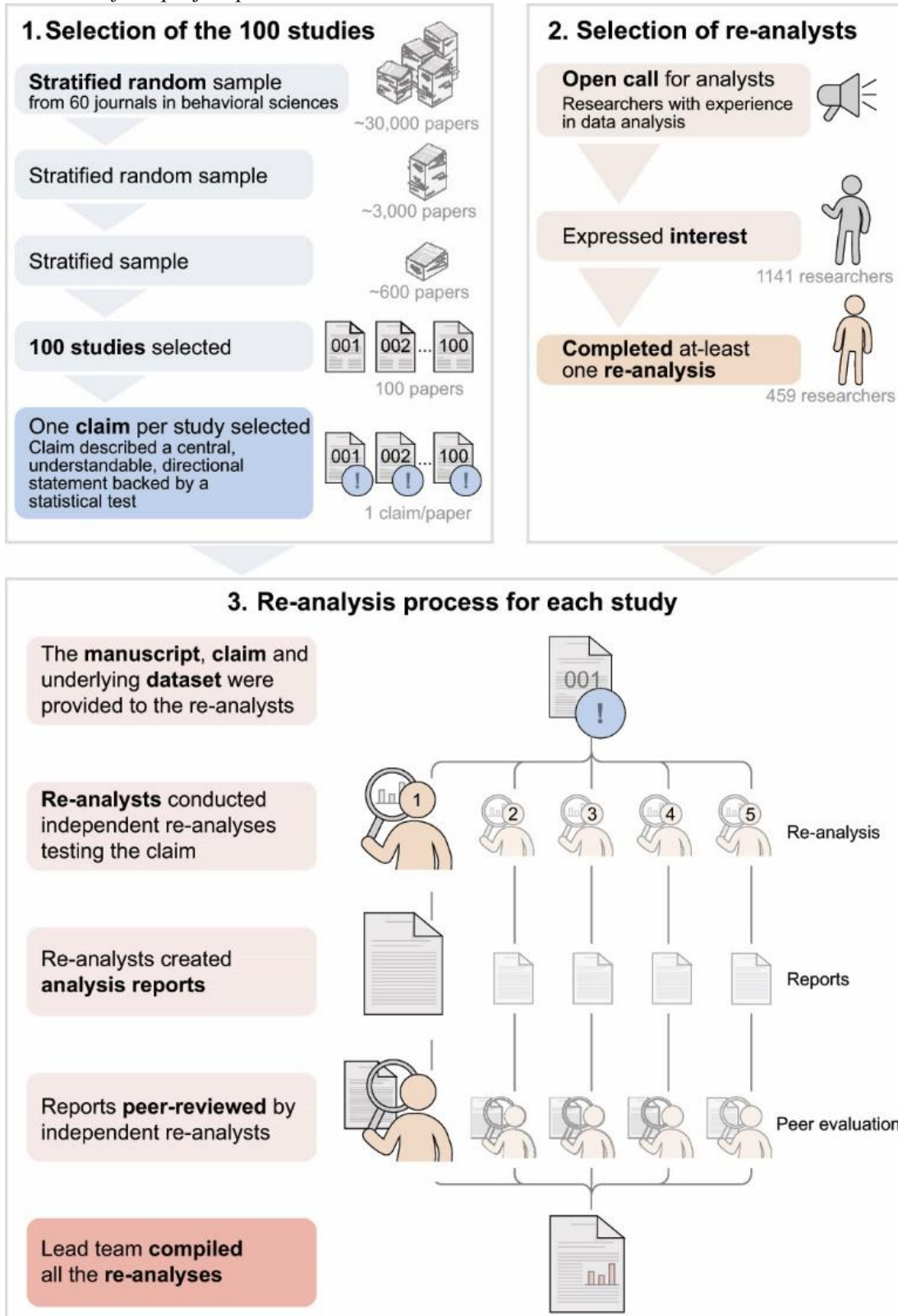


1696
1697 *Note.*
1698 **a**, Proportion of same effect, no effect/inconclusive results, and conclusions in the opposite direction
1699 of the original studies, by matches and nonmatches between the discipline of the re-analyst and the

1700 original study. **b**, The distribution of the heterogeneity ratios calculated between the effect size
 1701 variability over the re-analyses and the sampling variability of the original study effect-size estimates.

1702 **Extended Data Fig. 5**

1703 *Overview of the project procedures.*



1704
 1705 *Note.*

1706 The figure depicts the procedural workflow of the selection of the studies (1); the selection of the re-analysts
1707 (2); and the re-analysis process for each study (3).