RETHINKING REPLICATION IN EMPIRICAL LEGAL RESEARCH

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Abstract

A large number of systematic replication attempts in the social sciences have failed to support the claims in the original studies. These surprising results have inspired a body of metascientific research aimed at understanding these failures to replicate and ensuring future research is credible. In this article, we relate these new insights from metascience to empirical legal research. Specifically, a recent effort to replicate three influential empirical legal studies published in law journals found results that diverged from the originals. The replicators suggested their results were caused by the changing social context and did not explicitly consider whether the original effects were overstated or false positives. We re-analysed the data from the replications to attempt to confirm their results (i.e., computational reproducibility) with mixed success. When possible, we combined the data from the replications (i.e., meta-analysis) to leverage the greater precision that comes with large sample sizes. In one case, we found an effect where the replicators did not. Overall, however, our re-analysis and review of the broader social scientific context suggests that empirical legal research suffers from the same challenges plaguing other fields – small sample sizes and undisclosed flexibility have produced untrustworthy results.

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Part I. Introduction

> What proportion of published research is likely to be false? Low sample size, small effect sizes, data dredging (also known as P-hacking), conflicts of interest, large numbers of scientists working competitively in silos without combining their efforts, and so on, may conspire to dramatically increase the probability that a published finding is incorrect. The field of metascience — the scientific study of science itself — is flourishing and has generated substantial empirical evidence for the existence and prevalence of threats to efficiency in knowledge accumulation…[1]

Replication is fundamental to the experimental sciences.[2] A replication of a study that yields the same findings as the original boosts confidence in those findings, whereas discrepant results raise questions.[3] Irvine, Hoffman, and Wilkinson-Ryan ('IHWR') recently attempted to replicate three empirical legal studies and in each case found results that, in some way, differed from the original findings.[4] These studies had been published in prestigious law journals and been cited over 1,000 times, including many times in the Australian context.[5] In this important work, they explained the failures to replicate as primarily due to social-contextual changes,[6]

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[1] Marcus R. Munafò et al, 'A manifesto for reproducible science' (2017) 1 *Nature Human Behaviour* 1, 1.
[2] Stefan Schmidt, 'Shall we really do it again? The powerful concept of replication is neglected in the social sciences' (2009) 2 *Review of General Psychology* 90.
[3] Brian A Nosek and Timothy M Errington, 'What is replication?' (2020) 18 *PLoS Biology*; Katheryn Zeiler, 'The Future of Empirical Legal Scholarship: Where Might We Go from Here?' (2016) 66 *Journal of Legal Education* 78, 82.
[4] Krin Irvine, David A Hoffman, and Tess Wilkinson-Ryan, 'Law and Psychology Grows Up, Goes Online, and Replicates' (2018) 15 *Journal of Empirical Legal Studies* 320 ('IHWR'), 320: the 'second aim' of their study was to compare online samples with more traditional data collection. While this is a very important component of their study, we will focus on the more general replication issue.
[5] See Robyn Carroll, 'Apologies as a Legal Remedy' (2013) 35(2) *Sydney Law Review* 317; Robyn Carroll, 'You Can't Order Sorriness, So is There Any Value in an Ordered Apology - An Analysis of Ordered Apologies in Anti-Discrimination Cases' (2010) 33(2) *UNSWLJ* 360; Debra Slocum, Alfred Allan, and Maria M Allan, 'An emerging theory of apology' (2011) 63 *Australian Journal of Psychology* 83.
[6] IHWR (n 4) 322, 324-25, 341-5: They discussed the importance of sample sizes but did not connect this issue to their failure to replicate several findings; On the limited explanatory value of the social-contextual changes account (i.e., hidden moderators), see Anton Olsson-Collentine, Jelte M Wicherts, and Marcel ALM van Assen, 'Heterogeneity in direct replications in psychology and its association with effect sizes' (2020) 146(10) *Psychological Bulletin* 922.

without expressly considering that the effects might have been false positives or substantially smaller than suggested by the original reports. In this article, we will discuss these possibilities and provide an update regarding the fast-moving field of reproducibility in the social sciences.

Our overarching goal is to integrate empirical legal scholarship into the larger conversation in science about reproducibility exemplified in this article's opening epigraph. Notably, many recent large multi-lab replication efforts have been unable to replicate seemingly well-established findings.[7] A large proportion have found no significant effect or much smaller effects than originally reported.[8] This has mobilized researchers interested in the study of science (i.e., metascience) to identify and investigate the factors that may explain these surprising failures to replicate. These include: imprecise measurement,[9] sampling error,[10] publication bias,[11] undisclosed researcher degrees of freedom or p-hacking (e.g., excluding outliers or stopping data collection after observing the data),[12] and small sample sizes, which can exacerbate the effects of

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[7] Open Science Collaboration (OSC), 'Estimating the Reproducibility of Psychological Science' (2015) 349(6251) *Science* 943 ('RP:P'); Richard A Klein et al, 'Investigating variation in replicability: A 'many labs' replication project' (2014) 45(3) *Social Psychology* 142 ('ML1'); Richard A Klein et al, 'Many Labs 2: Investigating Variation in Replicability Across Samples and Settings' (2018) 1 *Advances in Methods and Practices in Psychological Science* 443 ('ML2'); Charles Ebersole et al, 'Many Labs 3: Evaluating participant pool quality across the academic semester via replication' (2016) 67 *Journal of Experimental Social Psychology* 68 ('ML3'); Richard A Klein et al, 'Many Labs 4: Failure to Replicate Mortality Salience Effect With and Without Original Author Involvement' <https://psyarxiv.com/vef2c> (advance); Colin F Camerer et al, 'Evaluating replicability of laboratory experiments in economics' (2016) 351 *Science* 1433 ('RP:EE'); Colin F Camerer et al, 'Evaluating the replicability of social science experiments in Nature and Science between 2010 and 2015' (2018) 2 *Nature Human Behaviour* 637 ('Replicability of Social Science in Nature and Science').

[8] RP:P (n 7) 943; Replicability of Social Science in Nature and Science (n 7) 637.

[9] Jessica Kay Flake and Eiko I Fried, 'Measurement Schmeasurement: Questionable Measurement Practices and How to Avoid Them' (2020) 3(4) *Advances in Methods and Practices in Psychological Science* 456.

[10] ML2 (n 7) 446-7.

[11] Daniele Fanelli, 'Negative results are disappearing from most disciplines and countries' (2012) 90 *Scientometrics* 891.

[12] Joseph P Simmons, Leif D Nelson, and Uri Simonsohn, 'False-Positive Psychology: Undisclosed Flexibility in Data Collection and Analysis Allows Presenting Anything as Significant' (2011) 22(11) *Psychological Science* 1359; Hannah Fraser et al, 'Questionable research practices in ecology and evolution' (2018) 13(7) *PloS One* e0200303; Leslie K John, George Loewenstein, and Drazen Prelec, 'Measuring the Prevalence of Questionable Research Practices With Incentives for Truth Telling' (2012) 23(5) *Psychological Science* 524; Matthew C Makel et al, 'Questionable and Open Research Practices in Education Research' <https://edarxiv.org/f7srb/> (advance); Edith Beerdsen 'Litigation Science after the Knowledge Crisis' *Cornell Law Review* (2020) 106 *Cornell Law Review* 529.

the latter three factors.[13] Additionally, within empirical legal research some scholars have raised concerns that U.S. student-edited journals do not vet methodology and statistics very well (all three of the studies in IHWR's project were published in such outlets).[14]

Replication and the trustworthiness of research are especially important for legal studies because they often have direct policy or societal implications. One recent empirical study published in an eminent Australian law journal,[15] widely covered in the media,[16] relied on a small sample (2 years), contained no preregistered (see below) analyses,[17] and used methods that may only support tentative claims.[18] Yet, it stated conclusively that female high court judges are interrupted more than their male counterparts.[19] The article went on to propose substantial reforms to remedy this phenomenon.[20] A subsequent study collected data over a longer timeframe (15 years).[21] It found that the previous study's sample was an outlier and that using all

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[13] John PA Ioannidis, 'Why Most Published Research Findings Are False' (2005) 2(8) *PloS Medicine* 696; Felix Singleton Thorn, 'The Low Statistical Power of Psychological Research: Causes, Consequences and Potential Remedies' (PhD Thesis, University of Melbourne, 2020).

[14] Zeiler (n 3) 87-90; Shari Seidman Diamond, 'Empirical Legal Scholarship: Observations on Moving Forward' (2019) 113 *Northwestern University Law Review* 1229.

[15] Amelia Loughland, 'Female Judges, Interrupted: A Study of Interruption Behaviour during Oral Argument in the High Court of Australia' (2019) 43(2) *Melbourne University Law Review* 822.

[16] Julia Hare, 'Girl Interrupted: Talking over the Top of Female Judges', *BroadAgenda* (Blog Post, 12 February 2020) < http://www.broadagenda.com.au/home/girl-interrupted-if-it-happens-to-high-court-judges-rude >; Michaela Whitbourn, 'Female High Court Judges "Far More Likely" to Be Interrupted than Male Peers: Study', *The Sydney Morning Herald* (online, 5 February 2020) <https://www.smh.com.au/national/female-high-court-judges-far-more-likely-to-be-interrupted-than male-peers-study-20200204-p53xjw.html>; 'Overcoming Disproportionate Interruptions Faced by Female Judges', *The Lawyers Weekly Show* (Lawyers Weekly, 6 March 2020) <https://www.lawyersweekly.com.au/podcast/27645-overcoming-disproportionate-interruptions-facedby-female-judges>.

[17] Brian A Nosek et al, 'The Preregistration Revolution' (2018) 115(11) *PNAS* 2600.

[18] Eric-Jan Wagenmakers, 'An Agenda for Purely Confirmatory Research' (2012) 7(6) *Perspectives on Psychological Science* 632.

[19] Loughland (n 15) 824, 825, 840 842, 846, for example, 'It shows that having women in superior positions of institutional authority is insufficient to transcend the fact that High Court dialogue is embedded in broader social discourses and power relations, meaning that female judges are treated as conversational inferiors and denied full participation in oral argument.'.

[20] Ibid 844-5.

[21] Tonja Jacobi, Zoë Robinson, and Patrick Leslie, 'Querying the Gender Dynamics of Interruptions at Australian Oral Argument' (2020) 4 *UNSWLR* 1.

available data, there was no effect of a judge's gender.[22] Similarly, an empirical study of Victorian judicial decisions failed to be replicated with more rigorous methods.[23] These studies highlight the pattern in social science we discuss in Part II, in which initial studies are often contradicted by more rigorous follow-ups. They also highlight the need for higher quality and more transparently conducted empirical legal research.[24] Despite such failures to replicate, there does not appear to much conversation about the transparency and quality of empirical legal research, and so we hope this work begins such a discussion.

We also hope to provide more specific contributions regarding IHWR's findings. In particular, 'computational reproducibility checks' are too rare in science.[25] That is, there should be checks of the computational steps used in published analyses to make sure the same result is obtained.[26] We performed these analyses on IHWR's data and found that some of their results did reproduce, but we could not reproduce some others.

We also used meta-analysis (we combined their data into a larger dataset) on their data to leverage the greater precision that comes with increased sample sizes. This provided more precise estimates of the important effects (e.g., factors that can promote more settlements and thus take some burden off the courts) that IHWR studied. For example, we found strong

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[22] Ibid 8-9: '…although female justices are indeed interrupted by advocates more frequently than male justices between 2015 and 2017 – the period of Loughland's study – it is not the case that female justices are interrupted more than male justices in most years. In fact, there are more years in which male justices are interrupted at higher rates than female justices…'.

[23] Gavin Silbert, 'The First 24 Years of the Victorian Court of Appeal in Crime' (2020) 94(6) *Australian Law Journal* 455; Brian Opeskin and Gabrielle Appleby, 'Responsible Jurimetrics: A Reply to Silbert's Critique of the Victorian Court of Appeal' (2020) 94 *Australian Law Journal* (forthcoming).

[24] Zeiler (n 3).

[25] See Victoria Stodden, Jennifer Seiler, and Zhaokun Ma, 'An empirical analysis of journal policy effectiveness for computational reproducibility' (2018) 115(11) *Proceedings of the National Academy of Sciences* 2584; Simine Vazire and Alex O Holcombe, 'Where Are The Self-Correcting Mechanisms In Science?' (2022) 26(2) *Review of General Psychology* 212.

[26] This sometimes happens in the context of journal special issues, but should not be relegated to that context, see Jukka Savolainen and Matthew VanEseltine, 'Replication and Research Integrity in Criminology: Introduction to the Special Issue' (2018) 34(3) *Journal of Contemporary Criminal Justice* 236.

evidence for one effect that IHWR did not indicate they had strong evidence for. Mostly, however, we found that effects were smaller than in the original studies. This is consistent with many other replication studies, conducted mostly in psychology, that find that studies conducted years ago and with less rigorous methods appear to have overestimated the size of the underlying effect (e.g., how much an apology affects one's inclination to settle). This matters because empirical legal research is applied research and policy-makers need precise estimates to guide legal change. We are grateful to IHWR for providing us with their data and materials, which they have made public at our request.[27]

In Part II we provide some background on social science replication research and the practices that replication research indicates have reduced the credibility of published findings. Part III delves into reforms that some fields are using to tackle the issues highlighted in Part II. Then, Part IV provides our analysis of the three effects IHWR studied and our overall evaluation of the strength of the evidence for those effects. Part V concludes with reflections on the importance improving empirical legal research practices.

Part II. What to make of failed replications?

Interpreting the results of a replication study (i.e., collecting new data to confirm a previous study's finding)[28] is not a simple exercise.[29] There are many reasons why a replication may find different results than the original. For instance, the precise context of an original study can never be completely recreated.[30] This is especially challenging in research affected by social

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[27] Krin Irvine, David A Hoffman, and Tess Wilkinson-Ryan, 'Publicly Available Data' <https://www.law.upenn.edu/faculty/twilkins/publicly-available-data.php> ('IHWR Data').
[28] With that definition of 'replication', we will use the term 'reproduction' and 'reproducibility' to refer to recomputing the findings of previous studies based on their data, materials, and code. See Lorena A Barba, 'Terminologies for Reproducible Research' <https://arxiv.org/pdf/1802.03311> (advance).
[29] Nosek and Errington (n 3).
[30] IHWR (n 4) 341-2; ML2 (n 7) 482.

processes, which is especially relevant when it comes to empirical legal research.[31] The impossibility of exact replication produces the interpretive difficulty of identifying the reasons why a subsequent study did or did not replicate the original's result. Unfortunately, this interpretive task is rife with opportunities for motivated reasoning: 'Deciding whether a study is a replication after observing the outcomes can leverage post hoc reasoning biases to dismiss 'failures' as non-replications, and 'successes' as diagnostic tests of the claims, or the reverse.'[32] Before we explore the reasons a study's results may or may not replicate, we will briefly review the findings of large-scale replication projects in social science.

A. Large-scale replication studies in social science

In their empirical legal replication project, IHWR discussed the two large-scale replication efforts in psychology that had been published at the time.[33] The first, the Reproducibility Project: Psychology ('RP:P'), conducted replications of 97 published psychology studies (i.e., they collected data from new participants following the methods of the originals as closely as was feasible).[34] This project drew headlines for its finding that only 35 of the 97 originally positive effects replicated at the same level of statistical significance as the original (see Table 1). Although the RP:P replications appeared to be well-powered (i.e., they used large enough sample sizes to detect the original effects) by psychological standards at the time,[35] commentators suggested that larger samples would produce more successful

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[31] Jay J Van Bavel et al, 'Contextual Sensitivity in Scientific Reproducibility' (2016) 113 *PNAS* 6454; IHWR (n 4) 341-2.
[32] Nosek and Errington (n 3) 3; see also Nicholas A Coles et al, 'The costs and benefits of replication studies' (2018) 41 *Behavioral and Brain Sciences* e124.
[33] IHWR (n 4) 325.
[34] RP:P (n 7).
[35] The average power for the studies, based on the size of the effect in the replicated study, was 92%, see <https://osf.io/k9rnd/>. Most published psychology studies have lower power than this (~50% chance of detecting effects of medium size), see Thorn (n 13).

replications.[36] For this reason, IHWR and others cited the other extant large replication attempt, a multi-site project in psychology called 'Many Labs' ('ML').

[Table 1 about here]

In the ML model, several participating laboratories (instead of one per study, as in RP:P) attempt to replicate a finding, with their estimates then combined to determine the overall effect. The first ML, Many Labs 1 ('ML1'),[37] found that 14 of the 16 effects replicated. It should be noted that ML1 did not attempt a representative sample of studies, unlike RP:P, but IHWR and others suggested its greater replication success had to do with the increased sensitivity, relative to RP:P, that comes with larger sample sizes (i.e., statistical power).[38]

However, the findings of subsequent studies suggest that chalking up the disappointing results of RP:P to low power may have been premature. ML2 and ML3 (which we will describe further below), despite large sample sizes, did not replicate the original findings nearly as consistently as ML1 (see Table 1). In ML2, 15 of 28 studies yielded findings at the same level of statistical significance as the original.[39] In ML3, that figure was 3 of 10.[40] Similarly, two other projects using large sample sizes produced replication findings more in line with RP:P and ML2 and 3. Camerer and colleagues attempted to replicate 21 social scientific studies published in *Nature* and *Science* and found that only 13 replicated.[41] An experimental economics replication

[36] Daniel T Gilbert et al, 'Comment on "Estimating the reproducibility of psychological science"' (2016) 351 *Science* 1037-a, 1037-b; IHWR (n 4) 325.
[37] ML1 (n 7); Gilbert et al (n 36).
[38] IHWR (n 4) 325: 'The Many Labs project replicated 14 of its 16 chosen studies, in part because of the dramatic increase in power.'; Gilbert et al (n 36) 1037-b.
[39] ML2 (n 7) 470.
[40] ML3 (n 7) 73.
[41] Evaluating the replicability of social science experiments in Nature and Science between 2010 and 2015 (n 7).

project yielded similar results, with significant effects in the same direction found for 11 of 18 studies (see Table 1).[42]

These large-scale studies, both the failures to replicate and the successes tended to find substantially smaller effect sizes than the original. For example, both RP:P and the replication study of *Nature* and *Science* social science papers found effect sizes approximately 50% smaller than the original studies.[43] A recent meta-analysis of large-scale replication studies found that the average effect size decrease in replications is 34% (95% CI [51%, 17%]).[44]

RP:P also came under fire for the imperfect fidelity of its methodologies to those of the original studies. Six differences between the methodology of the replications and the original studies were highlighted by Gilbert et al.[45] IHWR described this as a 'salient' critique and the differences as 'egregious'.[46] These differences included one described as follows:[47]

> An original study that asked Israelis to imagine the consequences of military service was replicated by asking Americans to imagine the consequences of a honeymoon…

The Gilbert et al. critique provided an incomplete view, however, of what RP:P did. For instance, three of those six differences were endorsed by the original authors as appropriate (as was discussed in a published response to Gilbert et al that IHWR did not cite).[48] In another case, the replication actually yielded the same statistically significant finding as the original.[49]

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[42] RP:EE (n 7).
[43] RP: P (n 7) 943; Evaluating the replicability of social science experiments in Nature and Science between 2010 and 2015 (n 7).
[44] Thorn (n 13) 209.
[45] Gilbert at al (n 36) 1037-b.
[46] IHWR (n 4) 325.
[47] Gilbert et al (n 36) 1037-b [references omitted].
[48] Christopher J Anderson et al, 'Response to Comment on "Estimating the reproducibility of psychological science' (2016) 351 *Science* 1037-c, 1037-c.
[49] Ibid.

Moreover, the differences with original study methodologies in the RP:P may not be as severe as IHWR imply. For example, based on the description by Gilbert et al quoted above, readers may be surprised that that study was not actually about military service or honeymoons, but *reconciliation after conflicts*.[50] In the original study, the experimenters presented participants with a scenario in which they were to imagine a workplace conflict in which a co-worker took credit for their work while they were on leave. That leave was described as either for military reserve duty (if a male) or for maternity leave (for females). About 80% of participants in the original study were females, so few (n = 19) even saw the military condition. The replicators used just one reason for both genders being on leave– for a wedding and honeymoon. So, while the critique of a military service study changed to one about honeymoons is arguably not as damning as readers may have been led to believe.

Beyond these methodological differences (which were transparently reported in RP:P, so that they can be discussed and debated), replication engages other difficult issues. First, there is the challenge of trying to replicate studies in different times and places, and with different subjects (e.g., does forgiveness work differently in Israel and the United States?). This issue, among other methodological differences, is often invoked to explain why a replication study did not yield the same result as the original, and they are sometimes referred to as 'hidden moderators'.[51] IHWR suggest that these sorts of factors are the reason that their replications failed to yield the same results as the original.[52]

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[50] Nurit Shnabel and Arie Nadler, 'A needs-based model of reconciliation: Satisfying the differential emotional needs of victim and perpetrator as a key to promoting reconciliation' (2008) 94 *Journal of Personality and Social Psychology* 116, 127; See Brian Nosek and Elizabeth Gilbert, 'Mischaracterizing replication studies leads to erroneous conclusions' <https://psyarxiv.com/nt4d3/> (advance).
[51] ML2 (n 7) 482; Olsson-Collentine et al (n 6).
[52] IHWR (n 4) 322, 341-5.

B. Hidden moderators

> It is practically a truism that the human behavior observed in psychological studies is contingent on the cultural and personal characteristics of the participants under study and the setting in which they are studied.[53]

Replication is especially challenging in psychology studies (and this extends to legal studies) because people and the situations that impact them vary in many ways. To investigate the possible presence of hidden moderators, the Many Labs studies (among others) varied the context of replications.[54] Olsson-Collentine and colleagues' 2020 combined the ML studies and similar projects into a large analysis (i.e., meta-analysis) to assess how these proposed moderators affected outcome (sometimes called effect size heterogeneity). Their results suggested that hidden moderators are not the dominant factor in the failures to replicate:[55]

> Our finding that heterogeneity appears to be generally small or non-existent is an argument against so called 'hidden moderators', or unexpected contextual sensitivity. Indeed, our results imply that effects cannot simply be assumed to vary 'across time, situations and persons'…

While these results suggest hidden moderators cannot account for *all* failures to replicate, some evidence for hidden moderators can be found in a re-analysis of RP:P performed by Van Bavel and colleagues.[56] In that study, trained coders rated each RP:P study for the degree to which it seemed sensitive to the context in which it was run (e.g., temporal, cultural).[57] They

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[53] ML2 (n 7) 482.
[54] E.g., ML2 varied culture by having labs in various countries recruit participants; ML3 varied the time of semester moderator, or in other words, anecdotal observation that students are less focused later in the term and so they may provide poorer data; ML4 attempted to tackle the potential moderator of replicator expertise. To investigate this, half of the replicating labs conducted their replication in consultation with the original authors and half did not. This attempt was thwarted because even the labs with expertise could not replicate the effect under study.
[55] Olsson-Collentine et al (n 6).
[56] IHWR (n 4) 344-5; Van Bavel et al (n 31); Jay J Van Bavel et al, 'Reply to Inbar: Contextual sensitivity helps explain the reproducibility gap between social and cognitive psychology' (2016) 113 *PNAS* E4935 ('Contextual sensitivity reanalysis').
[57] Contextual sensitivity reanalysis (n 56) 6455-6.

found that context-dependent studies were less likely to have replicated in RP:P. While this is an intriguing re-analysis and one IHWR cited with approval, they did not cite a contemporaneous critique of Van Bavel and colleagues' study published in the same journal, which found that once the study's subfield was controlled for (e.g., social or cognitive psychology), the context-dependence effect disappeared.[58] This matters because psychological subfields use different methods. For example, social psychology may be more context-dependent, but it may also use less powerful designs (e.g., few within-subjects studies). So, it is possible that Van Bavel and colleagues' finding was not about context-dependence, but the methodological rigor of two fields.[59] Moreover, even studies seemingly quite dependent on the cultural moment (e.g., about politics and morality) regularly replicate.[60]

In light of all that we have reviewed in this subsection, it is inappropriate to conclude that context is the sole (or even primary) driver of non-replication in social science. The weight of the evidence today is not consistent with the strength of IHWR's hidden moderators explanation. We will now explore another theory.

C. Type 1 and Type M errors

Another reason for some failures to replicate an original finding is that the original finding was a false positive (i.e., Type 1 error) or reflected a smaller effect than the replication was powered to detect (i.e., Type M error).[61] Although this possibility was not discussed by IHWR, there are reasons to believe that Type 1 and Type M errors are quite prevalent in the

[58] Yoel Inbar, 'Association between contextual dependence and replicability in psychology may be spurious' (2016) 113 *PNAS* E4933.
[59] And, even while cognitive studies did better in RP:P, they were not beyond reproach (replicating in 21 of 42 studies).
[60] See the 'Moral Foundations' study in ML2 (n 7) 455-6.
[61] Andrew Gelman and John Carlin, 'Beyond Power Calculations: Assessing Type S (Sign) and Type M (Magnitude) Errors' (2014) 9 *Perspectives on Psychological Science* 641.

social science literature. The factors that produce these errors may be especially common in empirical legal research.[62]

1. Researcher Degrees of Freedom (i.e., P-hacking, questionable research practices)

Certain research practices – sometimes referred to as researcher degrees of freedom – substantially inflate the risk of Type 1 and M errors.[63] Many fields endorse a maximum false positive (Type 1) rate of 5% (i.e., $\alpha = .05$).[64] Common research practices, however, violate the assumptions underlying that rate. In an influential article, Simmons and colleagues ran simulations to determine the effect of various practices.[65] They found that the use of four of the practices that constitute researcher "degrees of freedom" (choosing between reporting two dependent variables, adding ten more observations based on the results already collected, controlling for a covariate or interaction, dropping conditions) increased the Type 1 error rate to 60.7%.

How commonly do researchers use those problematic degrees of freedom? Anonymous surveys suggest high prevalence rates. For instance, in psychology, about 65% of respondents in one survey had not reported all dependent measures, and about 55% had decided to collect more data after looking at the results, which are all practices that inflate the Type 1 error rate.[66] Anonymous surveys of researchers in ecology and evolution and education have found similar results.[67] Non-anonymous admissions of researcher degrees of freedom are rare. However, in the

[62] Zeiler (n 3).
[63] See Beerdsen (n 12).
[64] See Regina Nuzzo, 'Statistical errors: P values, the 'gold standard' of statistical validity, are not as reliable as many scientists assume' (2014) 505 *Nature* 150.
[65] Simmons et al (n 12).
[66] John et al (n 12) 525.
[67] See sources at n 12; In criminology, perhaps the closest field to law, in that both interface with the legal system, researcher degrees of freedom also appear to be rampant, Jason M Chin et al, 'Questionable Research Practices and Open Science in Quantitative Criminology' <https://osf.io/preprints/socarxiv/bwm7s/> (advance).

controversy surrounding the ‘power pose’ effect (i.e., standing a certain way has a variety of purported psychological and physiological effects), the lead author of one influential article publicly stated she no longer endorsed the study’s main finding. She wrote: ‘The self-report DV was p-hacked in that many different power questions were asked and those chosen were the ones that “worked”’.[68]

2. Publication bias, underpowered studies, and statistical mistakes and misuse

Several other factors contribute to an outsized rate of Type 1 and M errors: publication bias, underpowered research, and misapplication of statistical tests and statistical errors.

Publication bias is the disproportionate publication of statistically significant effects over null effects.[69] In many fields, negative results are very rarely published.[70] O’Boyle and colleagues, for instance, tracked research projects from student dissertations to their publication.[71] They found twice as many unsupported hypotheses in the dissertation than in the publication. They called this the ‘Chrysalis Effect’ whereby messy results (likely reflecting the reality of the research) transform into beautiful publications. Because in many fields, journals prefer positive findings over null results, the latter are often culled when it comes time to publish. This finding is consistent with a 2020 study comparing meta-analyses (of studies using standard protocols akin those used by the studies replicated by IHWR) and large preregistered replication studies, which showed that the standard studies found stronger effects.[72]

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68 Dana Carney, ‘My position on power poses’ <https://faculty.haas.berkeley.edu/dana_carney/pdf_My%20position%20on%20power%20poses.pdf>.
69 Fanelli (n 11).
70 Ibid.
71 Ernest H O’Boyle Jr, George C Banks, and Erik Gonzalez-Mulé, ‘The Chrysalis Effect: How Ugly Initial Results Metamorphosize Into Beautiful Articles’ (2014) 43 Journal of Management 376.
72 Amanda Kvarven, Eirik Strømland, and Magnus Johannesson, ‘Comparing meta-analyses and preregistered multiple-laboratory replication projects (2020) 4 *Nature Human Behaviour* 423.

Underpowered studies amplify the effects of publication bias. In many fields, most studies do not have enough power to detect the small effects that are typical of the field.[73] This problem is especially well-documented in psychology (with at least 46 studies on the topic).[74] These studies indicate that typical psychology studies will only detect true small effects about 20% of the time and medium effects about 60% of the time, where small and medium were defined as in Cohen's pioneering publication. This means that those studies of real effects that do turn out statistically significant will, on average, report that effects are bigger than they actually are. This is because low-powered research is only likely to yield statistically significant results when, due to chance fluctuations, the study yields an effect larger than the true effect. Several studies support this relationship between low power and Type M errors.[75] For example, in a random sample of 1000 published psychology studies, Kühberger and colleagues found that smaller studies were more likely to report larger effects.[76]

Both small sample sizes and researcher degrees of freedom likely contributed to the findings of a 2020 study comparing meta-analyses of studies using standard protocols akin to those used by the studies replicated by IHWR and replication studies that used large samples and the more rigorous practices we review below in Part III. It found that the standard studies showed larger effects than the larger, more tightly controlled studies.[77] Based on all we have reviewed so far, it is likely that published studies using standard methods show inflated effects.

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[73] In medicine, see David Moher, Corinne S Dulberg, and George A Wells, 'Statistical Power, Sample Size, and Their Reporting in Randomized Controlled Trials' (2004) 272 *JAMA* 122; in neuroscience, see Denes Szucs and John PA Ioannidis, 'Empirical assessment of published effect sizes and power in the recent cognitive neuroscience and psychology literature' (2017) 15(3) *PLoS Biology* 1.

[74] See Thorn (n 13) 84.

[75] Ibid 53-7.

[76] Anton Kühberger, Astrid Fritz, and Thomas Scherndl, 'Publication bias in psychology: A diagnosis based on the correlation between effect size and sample size' (2014) 9(9) *PLoS ONE* e105825.

[77] Amanda Kvarven, Eirik Strømland, and Magnus Johannesson, 'Comparing meta-analyses and preregistered multiple-laboratory replication projects (2020) 4 *Nature Human Behaviour* 423.

Finally, metascientific studies have documented many statistical errors and improper practices in the literature.[78] In neuroscience, for instance, one study found that 79 of 157 neuroscience articles in its sample claimed to have found an interaction without actually reporting a statistical test to support that claim.[79]

There is reason to believe that empirical legal research is not immune from any of the issues mentioned in this section. First, all of these issues have been well documented in psychology, the source of law and psychology's methodology. Moreover, several legal scholars have noted substantial deficiencies in the peer review process in law that may exacerbate the biases we have discussed.[80] Most notably, student-run law journals in the US, like those that published the studies considered by IHWR, often do not rely on peer review. Moreover, methodological training may also be lacking among those with primarily a legal background. By way of analogy, one challenge in medical research has been that many studies are conducted by individuals primarily trained to be practitioners.[81] Based on these factors, we expect that methodological quality may be low in empirical legal research.

Part III. Reforms following failed replication attempts

The work reviewed above prompted a great deal of reforms to research practices in psychology. Empirical legal researchers and law journal editorial boards should be aware of

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[78] See Michèle B Nuijten et al, 'The prevalence of statistical reporting errors in psychology (1985–2013)' (2016) 48 Behavior Research Methods 1205; Zeiler (n 3) 85.
[79] Sander Nieuwenhuis, Birte U Forstmann, and Eric-Jan Wagenmakers, 'Erroneous analyses of interactions in neuroscience: A problem of significance' 14 *Nature Neuroscience* 1105.
[80] Zeiler (n 3); Diamond (n 14); Lee Epstein and Gary King 'The Rules of Inference' (2002) 69 *University of Chicago Law Review* 1, 48-49; Zachary J Bass et al, 'Editorial, The need for collective standards: Validating raw data in legal empirical analysis' (2020) *NYU Journal of Intellectual Property and Entertainment Law* 10(1) 40, 41: 'student-edited legal journals have largely failed to adapt their editorial systems to empirical works'.
[81] Douglas G. Altman, 'The scandal of poor medical research: we need less research, better research, and research done for the right reasons' (1994) 308 *BMJ* 283.

these reforms.[82]. We will review some here, but encourage readers to consult other existing work.[83]

A. Preregistration and registered reports

Preregistration, common in clinical medical research since the 1980s, is a useful way of addressing researcher degrees of freedom and publication bias.[84] It involves describing one's research design and hypotheses before conducting the study. Websites such as the Open Science Framework (OSF) and AsPredicted.org allow researchers to register their methods and hypotheses.[85] Preregistrations allow others to better assess the credibility of findings and conclusions by considering differences between the initial research plan and what was actually done and reported. By providing a public record of planned studies, preregistrations also allow researchers to find unpublished studies, and in doing so, mitigate the effects of publication bias. Because of these benefits, we think preregistration is worthwhile.

A related reform that, as of this writing, has been adopted by 256 journals is called registered reports (RRs).[86] In RRs, studies are reviewed based solely on the idea and methods, before the data are collected. This process incentivizes attention to methodology and allows peer reviewers to assist the authors before the data is collected. It disincentivizes researcher degrees of freedom because the author need not distort the results to increase the chance of the article being accepted, since the acceptance decision is made before data is collected. If the pre-data collection manuscript is accepted, the study is then preregistered. We are aware of only two law-

[82] See one editorial board's description of the steps they are taking: Bass et al (n 80).
[83] Munafò et al et al (n 1); Olivier Klein et al, 'A Practical Guide for Transparency in Psychological Science' (2018) 4 *Collabra: Psychology* 20 ('Practical Guide').
[84] Nosek et al (n 17).
[85] Center for Open Science, 'OSF Preregistrations' <https://osf.io/prereg/>; As Predicted <https://aspredicted.org/>.
[86] Chris Chambers, 'What's next for registered reports?' (2019) 573 *Nature* 187.

related journals to have adopted RRs.[87] This is unfortunate, because research is finding that RRs are working as intended in that they are less likely than regular articles to confirm the researcher's hypothesis (studies published under the normal model confirm hypotheses ~95% of the time, an unrealistic figure; RRs are closer to 50%).[88]

B. Open materials, scripts and data

Social scientists are increasingly making their materials, analysis scripts, and data publicly available.[89] The posting of data analysis scripts and data in particular facilitates error correction.[90] Consider, for instance, the recent example of a criminal justice researcher retracting a study in an influential journal mere weeks after it was published because an attentive colleague found coding errors in his data.[91] Open research practices also improve credibility and trust, aligning the field's practices with the public's expectations;[92] the public is more inclined to trust science conducted with open practices.[93] Such practices also promote efficiency by allowing subsequent researchers to reuse and extend existing findings, thus reducing duplication of

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[87] Law and Human Behavior, <https://www.apa.org/pubs/journals/features/lhb-registered-reports.pdf>; Legal and Criminological Psychology, 'LCP Author Guidelines' <https://onlinelibrary.wiley.com/page/journal/20448333/homepage/forauthors.html>.
[88] Anne M Scheel, Mitchell Schijen, and Daniël Lakens, 'An excess of positive results: Comparing the standard Psychology literature with Registered Reports' <https://psyarxiv.com/p6e9c> (advance).
[89] Garret Christensen et al, 'Open Science Practices are on the Rise: The State of Social Science (3S) Survey' <https://osf.io/preprints/metaarxiv/5rksu/>.
[90] John PA Ioannidis, 'Why science is not necessarily self-correcting' (2012) 7(6) Perspectives on Psychological Science 645.
[91] Joscha Legewie, 'Retraction of the Research Article: "Police Violence and the Health of Black Infants"' (2019) 5(12) *Science Advances* eaba5491.
[92] Justin T Pickett and Sean Patrick Roche, 'Questionable, Objectionable or Criminal? Public Opinion on Data Fraud and Selective Reporting in Science' (2018) 24 *Science and Engineering Ethics* 151.
[93] Cary Funk et al, *Trust and Mistrust in Americans' Views of Scientific Experts* (Pew Research Center Report, 2019) 24.

efforts.[94] Because of these benefits, many funders and journals are beginning to require open practices.[95]

Still, there are challenges facing researchers seeking to open up their practices. The challenges include determining what should be shared, the most useful way to do that sharing, and ensuring compliance with institutional review board requirements. Fortunately, these are questions that psychology and other fields have been grappling with for the past several years, and so there is guidance for empirical legal researchers.[96]

IHWR were responsive and helpful (albeit limited by the bounds of what they saved and their memories for these studies) when we requested their data and further information on their methods. Had we waited longer, however, it may not have been as easy to reach them. Indeed, studies have found that most data requests via email are unsuccessful.[97] In the current case, IHWR's sharing of data and materials with us allowed us to review and confirm much of their analysis, and extend them in ways that we hope will contribute to the literature (see Part IV). Further investigation and confirmation are now also possible because IHWR have decided to upload their materials and data online to their personal faculty webpage.[98] More generally, it is worrying that many law journals do not have guidelines for data sharing.[99] We believe they

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[94] Iain Chalmers and Paul Glasziou, 'Avoidable waste in the production and reporting of research evidence' (2009) 374 *Lancet* 86.
[95] Nicole A Vasilevsky et al, 'Reproducible and reusable research: are journal data sharing policies meeting the mark?' (2017) 5 *PeerJ* e3208.
[96] Practical Guide (n 83).
[97] Timothy H Vines et al, 'The Availability of Research Data Declines Rapidly with Article Age' (2014) 24 *Current Biology* 94; Jelte M Wicherts, Marjan Bakker, and Dylan Moolenaar, 'Willingness to Share Research Data is Related to the Strength of the Evidence and the Quality of Reporting of Statistical Results' 6(11) *PLoS ONE* e26828.
[98] IHWR Data (n 27).
[99] Zeiler (n 3) fn 58.

should adopt guidelines similar or identical to that of many journals in psychology and other sciences.[100]

C. Replication

Replication projects like that of IHWR are especially important in contexts like law that have direct applied consequences. Replication by independent parties is crucial to verifying existing findings and producing more realistic estimates of effect sizes.[101] Once an effect has been found to be meaningful in its original context, the field can go on to see if it generalizes to other contexts and stimuli.[102] Unfortunately, replication is all too rare in social science.[103]

Part IV. What do we now know about the three effects studied by IHWR?

In re-evaluating the findings of IHWR and the original studies they replicated, we came to two general conclusions that we will elaborate on in this part. Recall that, across all three studies, IHWR did not replicate some of the effects that they identified as central to the paper, based on the $p < .05$ criterion. They explained these changes as due to hidden moderators in the form of social-contextual changes, which they claimed were especially common in empirical legal research.[104]

[100] Center for Open Science, 'TOP Factor' <https://topfactor.org/>.
[101] Geoff Cumming, 'Replication and p intervals: p values predict the future only vaguely, but confidence intervals do much better' (2008) 3(4) *Perspectives on Psychological Science* 286.
[102] Mark J Brandt et al, 'The Replication Recipe: What Makes for a Convincing Replication?' (2014) 50 Journal of Experimental Social Psychology 217; see generally, Robert Heirene, 'A call for replications of addiction research: Which studies should we replicate & what constitutes a "successful" replication?' <https://psyarxiv.com/xzmn4/> (advance).
[103] We do not have data for the field of law and psychology or empirical legal research, but replication seems to occur rarely in similar fields (e.g., 1.07% in psychology and 0.45% in criminology): Matthew C Makel, Jonathan A Plucker, and Boyd Hegarty, 'Replications in Psychology Research: How Often Do They Really Occur?' (2012) 7(6) *Perspectives on Psychological Science* 537. William Alex Pridemore, Matthew C Makel, and Jonathan A Plucker, 'Replication in Criminology and the Social Sciences' 1 *Annual Review of Criminology* 1.
[104] IHWR (n 4) 342.

First, in combining all of the panels IHWR used (i.e., MTurk, Survey Software International, Prolific Academic, and in-lab participants), we were, for one paper, able to find statistically significant effects where IHWR did not. The sizes of those effects, however, are consistent with the pattern common for replication studies in social science generally: the first studies published on an effect report larger effect sizes than do subsequent replication studies.[105] This effect size inflation is especially problematic in fields like law that can directly inform policy. Second, our tasks were made more difficult (and in some cases impossible) by a lack of transparency, even in the replication studies conducted by IHWR. This surprised us because IHWR themselves prescribed open data and methods.[106] Throughout our review, we have tried to remedy this lack of transparency by providing our analysis code and previously unreported methodological details that IHWR shared with us. Note also that our results should be considered exploratory because, by necessity, we were aware of IHWR's conclusions as we attempted to confirm their work and tested its robustness.

For readability, we provide a short summary of our findings and analysis below. Interested readers can consult the online supplementary materials for a complete (and more technical) description of our methods, as well as consult our analysis code online.[107] This code can be used in conjunction with IHWR's data.[108]

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[105] See the sources at n 8.
[106] IHWR (n 4) 347.
[107] The Authors, 'Supplementary Materials' <https://osf.io/6x9eh/> ('Supplementary Materials'); The Authors, 'Apology Effect analysis code' <https://osf.io/xdhc3/> ('Apology Effect analysis code'); The Authors, 'Gain and Loss Framing analysis code' <https://osf.io/pgbnx/> ('Gain and Loss Framing analysis code'); The Authors, 'Joint and Separate Evaluation analysis code' <https://osf.io/t386e/>. Jennifer Robbennolt also shared her original materials with us, which we have now made publicly available with her permission <https://osf.io/vew69/>.
[108] IHWR Data (n 27).

A. Apologies and settling (replication of Robbennolt, 2003)

The apology effect hypothesized by Robbennolt (2003) is that plaintiffs are more likely to settle when they receive an apology.[109] One reason for studying whether this effect exists, and if so how large it is, is that laws have been proposed to make apologies inadmissible evidence of culpability, which would lead to more apologies.[110] Such laws may be beneficial if apologies truly increase settlement, as settlements reduce the burden on courts. This work has been considered in the Australian context.[111]

Robbennolt's original vignette study (the one replicated by IHWR) has been cited over 400 times.[112] The apology effect is distinctive among the three effects IHWR chose to replicate in that the earlier published evidence was quite weak. The study that inspired Robbennolt's did not find a statistically significant apology effect.[113] And, the results in Robbennolt's study were either not statistically significant or were weak, used very small samples (~20 participants were condition), and did not replicate within the same article.[114] In their replications, IHWR reported

[109] Jennifer K Robbennolt, 'Apologies and Legal Settlement: An Empirical Examination' 102(3) *Michigan Law Review* 460, 484. Participants were asked to imagine they had been hit by a bicyclist. Depending on condition, participants received either no apology, a partial apology (expressing regret, but not accepting full responsibility), or a full apology (expressing regret and accepting full responsibility). Participants then responded to several questions, including one asking their willingness to accept an offer to settle on a scale from would definitely reject the offer (1) to would definitely accept (5): 1 = I would definitely reject the offer; 2 = I would probably reject the offer; 3 = I am unsure about whether I would accept the offer; 4 = I would probably accept the offer; 5 = I would definitely accept the offer. For a fuller description, see Supplementary Materials (n 107).
[110] Ibid 462: 'This Article seeks to fill the gap by providing much-needed data. The studies described here explore the proposition that apologies facilitate the settlement of civil disputes…'.
[111] See the studies at note 5.
[112] Google Scholar, <https://scholar.google.com/scholar?cites=10460982142126739111&as_sdt=2005&sciodt=0,5&hl=en>.
[113] Russell Korobkin and Chris Guthrie, 'Psychological Barriers to Litigation Settlement: An Experimental Approach' (1994) 93(1) *Michigan Law Review* 107, 148: reporting t (117) = 1.33, $p < .1$.
[114] Email from Jennifer Robbennolt to Jason M Chin, 7 January 2020; Robbennolt (n 108) fn 167.

no statistically significant effect in any panel, but did not interpret this as evidence that the original study was a Type 1 or Type M error.[115]

First, while we were not able to computationally reproduce all of IHWR's results, we confirm that that, even with the added power of a meta-analysis, they did not replicate the apology effect. Interpreting Robbennolt's study and the replications was difficult because it contained several different measurements of settlement acceptance. One was the average response to a 1-5 measure (where e.g., 1 was definitely reject and 5 was definitely accept). IHWR appeared to treat that 1-5 measure as the primary effect of interest and reported that they failed to replicate it (another measure was whether or not chose 'definitely accept' or any other response). We were not able to confirm all of IHWR's results because their article did not provide the details of the statistical test they used. However, we were able to reproduce the percentages in IHWR's Table 3 and have added confidence intervals for them. We display those results in our Figure 1.[116] The large overlapping 95% confidence intervals reflect high variability in responses. We combined the data from all three of IHWR' panels and found a statistically significant effect for one measure, but not two others reported by Robbennolt.[117] Without *a priori* reasons for preferring one measure over another and given the danger of selectively reporting only measures that work, we conclude that even averaging over all panels, IHWR did not replicate the apology effect.

[Figure 1 about here]

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[115] IHWR (n 4) 336.

[116] Error bars are 95% confidence intervals calculated by the add4ci method implemented in Ralph Scherer, 'PropCIs: Various Confidence Interval' <https://CRAN.R-project.org/package=PropCIs> (2018); Alan Agresti and Brian Caffo, 'Simple and Effective Confidence Intervals for Proportions and Differences of Proportions Result from Adding Two Successes and Two Failures' (2000) 54(4) *American Statistician* 280.

[117] This was the whether or not the participant chose 'definitely accept' or not. IHWR also found a significant effect on this measure. However, they do not say in what panels or what statistical test they performed: IHWR (n 4) 336-7.

IHWR suggested that the reason their replication did not support the initial finding was that modern participants found Robbennolt's scenario of a bicycle accident among neighbours and subsequent settlement offers 'surprising or awkward'.[118] This is possible, but we are sceptical that conceptions of bicycle accidents and neighbours arguing has changed much over the past few decades. We think a critical consideration is that the apology effect was never a large and persistent effect to begin with (recall the equivocal results in the original as well as its predecessor). After all, there are reasons why settling might not increase after an apology. Many people may, for instance, take an apology as a sign that they are in the right and that they will succeed in litigation. Moreover, as we discussed in Part II, there is a great deal of evidence that effects originally found with small sample sizes, *p* values hovering around .05, and analytic flexibility tend to be smaller and non-significant on replication.

B. Gain and loss framing (GLF) and settling (replication of Rachlinski, 1996)

A wider literature has found effects of framing a choice as a gain or loss (GLF) on various (not law-related) tasks, providing good reason for expecting that framing would affect settlement decisions – the topic of Rachlinski's study.[119] In short, research suggests people prefer a sure win over one with risk attached (i.e., a gamble). This is so even when the gamble is structured to pay out more on average than the sure win (i.e., higher expected value).[120] Conversely, people are more risk seeking with losses; they prefer to gamble rather than accept a sure loss.[121] GLF is likely a persistent effect. One of the original studies was recently replicated

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[118] IHWR (n 4) 341.
[119] See Jeffrey J. Rachlinski, 'Gains, Losses, and the Psychology of Litigation' (1996) 70 *Southern California Law Review* 113, 130-5; Amos Tversky and Daniel Kahneman, 'The framing of decisions and the psychology of choice' (1981) 211 *Science* 453.
[120] Ibid.
[121] Although note that this aversion to loss may depend on the context: David Gal and Derek D Rucker, 'The Loss of Loss Aversion: Will It Loom Larger Than Its Gains?' (2018) 28(3) *Journal of Consumer Psychology* 497.

in ML1 with a sample of over 6,000 people (albeit, consistent with the broader literature, the effect was approximately half the size originally reported).[122] Similarly, a 2020 replication study found it replicated robustly across countries and contexts.[123] Rachlinski applied GLF to law, finding it may be deter settlement (and thus burden the legal system): plaintiffs will want to settle because it is less risky, but defendants will be more willing to gamble because they do not like the idea of a sure loss.[124] GLF is important in Australia and abroad because, if it is a sizeable effect, lawyers may wish to take steps to avoid such framing so as to not unduly dissuade their clients from settling. IHWR replicated GLF with what appears to be the most success of the three effects.[125]

Through meta-analysis, we find that GLF in litigation contexts appears to be a robust effect. We diverge from IHWR, however, with our conclusion that the effect is likely not as large as it originally seemed. This is consistent with the general trend whereby larger replications find smaller effects than in the originals. For all but one result (noted below), we were able to computationally reproduce IHWR's results.

In Rachlinski's study, participants imagined advising either plaintiffs (gains) or defendants (losses) in a case in which the plaintiff was in the right and might receive $50 (the value of the land) if they went to trial, but had either a 30% or 70% chance (depending on the condition) of winning a higher damages amount (of $100k or $200k).[126] The other option the participant could choose was to advise the client that they should settle for a more certain

[122] ML2 (n 7) 148-9.
[123] Kai Ruggeri et al, 'Replicating patterns of prospect theory for decision under risk' (2020) 4 *Nature Human Behaviour* 622.
[124] Rachlinski (n 119); Korobkin & Guthrie (n 113) 130.
[125] IHWR (n 4) 337-8, 340-3.
[126] See a full description in the Supplemental Materials (n 107).

amount, which was set to approximate the expected value of going to trial (e.g., if the plaintiff had a 30% chance of winning $100k and 70% chance of just $50, the settlement amount was $30k). Thus, going to trial was a gamble in which the participant could win or lose a lot or very little. The scenario was a property dispute in which the plaintiff was a wealthy executive and the defendant was a chain of bed and breakfast inns (B&Bs). The B&B chain had illegally developed part of the plaintiff's land. The full study design was 2 (plaintiffs vs defendants) X 2 ($100k vs $200k) stakes X 2 (30% vs 70% probability of winning the 100 or $200k, as opposed to the $50 nominal amount) factorial design, resulting in 8 total conditions. Between 30 and 50 participants were assigned to each of these conditions. In a footnote, Rachlinski mentioned that another scenario, involving a copyright dispute, was also used but the data did not 'generate consistent or predictable results either supporting or refuting the framing effect'.[127]

Rachlinski compared participants randomly assigned to be lawyers to the plaintiff (i.e., gains framing) and those assigned to be a lawyer for a defendant (loss framing) on whether they would advise settlement in the four combinations of stakes and probability of winning (i.e., scenarios).[128] He found a statistically significant difference between lawyers for plaintiffs and defendants in 3 of the 4 scenarios. The non-significant comparison had the highest sample size. In two of the scenarios, there was a striking raw difference, with plaintiffs' lawyers 50 percentage points more likely to settle. In those scenarios, there was a great deal of risk seeking among defendants (75% of defendants chose to go to court). However, a look at our Figure 2,

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[127] Rachlinski (n 119) 136.
[128] Ibid 137; Supplementary Materials (n 107).

where we have replotted Rachlinksi's, data shows a great deal of variability between scenarios, which may be expected due to the small sample sizes.[129]

[Figure 2 about here]

IHWR performed GLF replications across three panels, with 2-3 times the sample sizes as the original. As in the original studies, plaintiffs were more willing to settle than defendants, but the differences were smaller (5-17% depending on panel) and only statistically significant for three of the six scenarios (Figure 2).[130]

Then, in what IHWR called a 'judicious replication', they simplified and arguably improved the study by giving the participants the role of the actual litigant, rather than telling them to imagine being the lawyer advising the litigant.[131] They also reduced the stakes to $25,000 and changed the scenario to be a dispute between two residential neighbours. Further, IHWR introduced a second scenario involving a video game trademark dispute between corporations. All of these panels reached statistical significance, with raw differences in settling between plaintiffs and defendants ranging from 25-35% (Figure 2).

We were able to reproduce most of IHWR's means and *p*-values.[132] Our Figure 2 displays the effect sizes and 95% confidence intervals for the original studies, direct replications, and judicious replications. This figure corresponds with IHWR's Figure 2, but with the judicious replications added. This helps visualize the original effects, with their large mean differences

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[129] Error bars are 95% confidence intervals calculated by a proportion difference test: Michael P Fay, Michael A Proschan, and Erica Brittain, 'Combining one sample confidence procedures for inference in the two sample case' (2014) 71 *Biometric Methodology* 146.
[130] IHWR (n 4) 339.
[131] Ibid 351.
[132] Gain and Loss framing analysis code (n 107); Supplementary Materials (n 107): For row 3, column two of IHWR's Table 10, we calculated 80.4%, while they calculated 79.6%.

between plaintiffs and defendants but large associated error terms, and how that contrasts with the direct and judicious replications. As with the apology study, we meta-analysed the three original replications (i.e., not the judicious replications) together, including panel as a fixed effect. Here the results were suggestive, but not as strong as the original studies, $z = 1.87$, $p = .061$ (i.e., marginally significant).[133]

So why did IHWR's replication of the original paradigm produce less convincing results than their judicious replications, and why did both indicate smaller effects than the 1993 study (Figure 2)? As with the apology effect, IHWR suggested hidden moderators in that the original scenario (the B&B) became less relatable for participants since the time of the 1993 study.[134] While that may be the case, it is also possible that the original student sample (undergrads at Stanford in the 1990s) always had some trouble relating to the idea of being lawyers for litigants in a B&B property dispute in Oregon, and that this issue did not get much worse with time. Rather, this may be an instance of a well-documented trend whereby the published studies with small sample sizes in this era overestimated effect sizes. This account also finds support in the large preregistered multi-lab GLF replication (in a non-legal context) in ML1 showing half the effect size of the original.[135] In other words, like GLF generally, GLF as applied to settlement decisions does seem to be a persistent effect, but the size of the effect might have been initially overestimated (perhaps because conditions that did not work were omitted from Rachlinkski's original report and thus could not be averaged in with the conditions that did work).[136] Furthermore, the results of the judicious replication suggest that in a real-life situation in which

[133] Gain and Loss Framing analysis code (n 107).
[134] IHWR (n 4) 342.
[135] ML2 (n 7) 148-9.
[136] Rachlinski (n 119) 136.

the facts are not especially complicated and the framing of a choice is made clear (i.e., mirroring IHWR's short and direct judicious replication scenarios), GLF may impact settlement decisions.

Finally, given the importance of the specific materials and stimuli used in these studies, we want to highlight an important methodological detail that is not mentioned in IHWR's report or its appendix, but is only apparent in the materials they shared with us.[137] In the trademark scenario of their judicious replication, the stakes (i.e., possible win or loss at trial) was $1.2 million. This amount is not mentioned anywhere in the article, so a reader is very likely to assume that they used the amounts from the property scenario ($100-300k) or its judicious replication ($25k). As a result, readers without IHWR's materials would not be able to reproduce their work or appropriately interpret the study's results. Additionally, IHWR slightly changed the payout structures of their Judicious Replications such that settlement offers no longer approximately matched the expected value of going to trial.[138]

<u>C. Joint and separate evaluation (JSE), punitive damages, and willingness to contribute to a public cause (a replication of Sunstein, Kahneman, Schkade & Ritov, 2002, 'SKSR')</u>

As with GLF, the effect of joint and separate evaluation (JSE) in non-legal contexts was fairly well-established prior to the study by SKSR.[139] The general idea of JSE is that people may show different preferences when evaluating something in isolation as compared to when it is a choice among other options because the comparison highlights aspects of the choice that people do not otherwise take into account. For example, you might not realise a speaker has a tinny

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[137] IHWR Data (n 27).
[138] Supplementary Materials (n 107).
[139] Cass R Sunstein et al, 'Predictably Incoherent Judgments' (2002) 54 *Stanford Law Review* 1153 ('SKSR'); Christopher K Hsee et al, 'Preference Reversals Between Joint and Separate Evaluations of Options: A Review and Theoretical Analysis' 125(5) *Psychological Bulletin* 576.

sound when it is the only thing you have been listening to (separate evaluation), but when you compare it to another speaker, the tinniness become more apparent (joint evaluation).

SKSR brought the JSE effect into law by testing whether people respond differently to legal cases (e.g., product liability) and public causes (e.g., pollution) if they were presented separately or in comparison to other cases or causes. More specifically, they presented descriptions of harms that were to humans or instead were financial or done to the environment. They called the human harms the 'prominent' category of harm.[140] SKSR's study involved three pairs of legal cases, each pair including one personal injury case (the prominent harm, e.g., a child who got sick because of a faulty childproof drug cap) and one financial case (the non-prominent harm, e.g., a fraud involving repainting luxury cars). They also used three public cause pairs (e.g., skin cancer among farm workers as the prominent harm vs polluted oceans harming dolphins).[141] Participants viewed cases and causes separately and then were asked to compare them. SKSR measured whether JSE would affect punitive damages in the legal cases and how much the participant would donate to the cause in the public cause scenarios.[142]

SKSR's findings robustly supported the hypothesis that JSE can affect punitive damages and contributions by driving them towards the prominent harm in joint evaluation.[143] This broadly matches SKSR's prior findings, which they published in a different journal, but did not

[140] SKSR (n 139) 1173. SKSR hypothesized that in joint evaluation, participants would react more strongly to the prominent harm because it would be compared to the less prominent harm. But, prominence would play 'no role' in separate evaluation. They called the process by which separate evaluation mutes harm prominence 'normalization'. In a previous study that appears to be identical to the current one, they did not use the term normalization, but rather focused on the role of JE in producing a 'context effect': Daniel Kahneman, Ilana Ritov, and David Schkade, 'Economic Preferences or Attitude Expressions?: An Analysis of Dollar Responses to Public Issues (1999) 19(1) *Journal of Risk and Uncertainty* 203, 219.
[141] SKSR (n 139) 1175.
[142] The replicators selected these dependent variables. The original contained additional measures, see IHWR (n 4) 339; Ibid 1176.
[143] SKSR (n 139) 1176.

acknowledge in the journal article IHWR replicated.[144] The finding is significant because, among other reasons, it suggests damages awarded can be substantially changed simply by raising a comparison.

For JSE, IHWR did not successfully replicate SKSR's findings in any panel.[145] As we will now discuss, using meta-analysis of IHWR's data, we did find a statistically significant JSE effect, although a very small one that may not have real world consequences. We did not, however, manage to computationally reproduce the numbers and statistics that IHWR reported (whereas with the apology effect and GLF, we reproduced most of their work).

Regarding the lack of computational reproducibility, IHWR's primary analysis (in their Table 6) only reports *p*-values and sample sizes. When e-mailed, IHWR indicated that they did not record and could not remember how they produced these p-values.[146] We did not succeed in reproducing them from the raw data they provided (see Supplementary Materials and our analysis scripts). Moreover, IHWR's Table 6 is difficult to interpret because neither it nor their article contains the mean and median values it seems to correspond to (we have plotted those values in Figures 3-5 and describe them below).[147] More generally, neither SKSR nor IHWR provided enough information to reproduce their findings. SKSR provided only some examples of

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[144] Kahneman et al (n 140) 218-20.

[145] IHWR (n 4) 339: '…the effect of comparison was noticeably muted and largely nonsignificant'; More specifically, IHWR did not replicate SKSR's interaction in any individual panel, although in the MTurk panel, the public causes were close to statistically significant, p =.051 (IHWR described this as statistically significant, even though it does not meet the conventional criterion for significance): IHWR (n 4) 340, Table 6.

[146] Email from Tess Wilkinson-Ryan to Jason M Chin, 4 March 2020; Supplementary Materials (n 107); Joint and Separate Evaluation analysis code (n 107).

[147] IHWR went on to report two subsidiary analyses that they report as supporting the initial SKSR finding. We could not reproduce these subsidiary results and, when asked, IHWR could not recall the tests performed: Email from Tess Wilkinson-Ryan, Personal Communication to Jason M Chin, 4 March 2020; As far as we have been able to determine, both subsidiary analyses are consistent with a simple main effect of prominence such that participants react more strongly to the human harm and did not support SKSR's primary claim of an interaction: Supplementary Materials (n 107). Figures 3-5's error bars are 95% confidence intervals, see note 116.

their materials, the total sample size, a general (1 page) description of their methods, and a *p* value. IHWR's report did not include any materials or any description of their methods. They did, however, promptly provide us with their materials.

[Figures 3 and 4 about here]

Despite not reproducing IHWR's figures, we did find evidence for a JSE effect where they did not. First, it is useful to visually display IHWR's data to better understand what is happening with them in a way that reporting *p*-values and sample sizes cannot get across. Starting with legal cases, Figure 3, which displays the mean damages awarded, helps us see that that there is a great deal of variability in responses that can mask any effect of JSE. In fact, in IHWR's data, one participant awarded $10^24 dollars, and many other participants awarded hundreds of millions or billions of dollars. The means demonstrate the original general pattern found by SKSR for the lab and SSI panel, but not for MTurk (putting aside tests of statistical significance for now). The medians do not demonstrate the hypothesized pattern (Figure 4).

Turning to the public causes in Figure 5, the 'winsorized' (setting any response over $500 to $500) means (the metric used by SKSR and IHWR)[148] show the original pattern, but still with a great deal of variation. Note also that many participants gave internally contradictory responses. For instance, 22% of the participants in the public cause scenarios reported that a scenario deserved more contributions than the other but then contributed less to that scenario.[149] This suggests that either they were not paying sufficient attention or the materials were too confusing.

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[148] IHWR (n 4) 341: 'As in the original paper, legal cases are reported as medians, and public cases are reported as means, windsorized [Sic] at 500.'
[149] Supplementary Materials (n 107).

[Figure 5 about here]

As with our previous re-analyses, we combined the data from the panels.[150] We found evidence for the original effects. Beginning with public causes (see Table 2), we found the hypothesized interaction whereby the human (i.e., prominent harm) receives more contributions when evaluated jointly against a non-human harm (p = .031), but it was extremely small (generalized eta-squared, i.e., η^2G, = .001).[151] For litigation scenarios (Table 3), this interaction did not reach statistical significance, however the variability was very large[152] (likely due to the outliers, such as the, participants who awarded damages in the trillions of trillions). For this reason, we decided to additionally analyse the logarithm of the damages (Table 3). The result mirrors the public causes, with a very small effect but a statistically significant result (p = .036, η^2G = .001).

[Tables 2 and 3 about here]

IHWR suggested two reasons for why they did not successfully replicate JSE. First, they suggested a ceiling effect impeded their replication in that participants seeing the personal injury by itself rated it higher than original participants did, and so there was little room for it to move up in joint comparison (i.e., it was at its ceiling).[153] However, because there was no cap on the amount that participants could award, it is unclear why IHWR believed there may be a ceiling (and, as we reported above, participants awarded amounts in the billions and beyond).

[150] While IHWR reported that they performed an ordinal regression, we determined that it was more appropriate to perform an analysis of variance because ordinal regressions are designed for ranked dependent measures.
[151] For general guidance on interpreting eta-squared, see Daniël Lakens, 'Calculating and reporting effect sizes to facilitate cumulative science: a practical primer for t-tests and ANOVAs' (2018) 4 *Frontiers in Psychology* 1.
[152] 273,482,173,934,662,701,710,101,632,079,660,204,198,723,584.00.
[153] IHWR (n 4) 339.

IHWR also implicated hidden moderators: 'We do not talk about saving dolphins the way we used to, for example—it is just not the same salient exemplar of environmental advocacy.'[154] As noted above, we disagree in the sense that our analysis suggests that, using all of the data, there is a significant JSE effect. But as to why IHWR's results were less convincing, we think that hidden moderators do not fully explain what IHWR observed. For example, within their own results, IHWR found that public causes were closest to replicating (p = .051). The seemingly more timeless litigation scenarios (e.g., child safety caps, contracts) failed to replicate in their analysis and did not seem to be any closer to replicating than the possibly-anachronistic causes IHWR singled out (e.g., dolphins). We think IHWR should have at least considered that the original effects were not as large as they initially seemed because this is consistent with the broader literature and, as we saw, there are different ways to deal with the immense variability in responses, such as setting all responses above \$500 to \$500 (e.g., why not \$750 or \$1,000?). Dealing with outliers in an ad hoc way is a researcher degree of freedom that contributes to Type 1 and Type M errors.

Our findings should also be qualified by one important transparency-related issue. IHWR's replications of SKSR deviated from the original in an undisclosed and potentially significant way. In their lab and MTurk panels, IHWR followed the original study in presenting the six pairs of case and causes. However, in their SSI panel, IHWR only used two of the pairs.[155] As a result, the SSI participants received a different set of materials than the other two, potentially affecting the results. There may have been a good reason to make this change, but it

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154 IHWR (n 4) 341.

155 They used the pair of legal cases that compared failed child safety caps to fraudulently repainting cars. For causes, they used the pair that compared detecting skin cancer in farm workers to protecting the dolphins. See SKSR (n 139) 1175.

should have been disclosed. We discovered it by going closely through their non-public data and materials.

We also note that in the litigation context, three of the SKSR authors had previously published studies that, from the descriptions available, appear to have used the same methodology as the studies that appear in SKSR.[156] That is, the studies in SKSR may be exact replications of the previously-published studies. Unfortunately, this was not clear because SKSR did not indicate they were replicating prior work and said there was no previous 'formal evidence' for their findings.[157]

Part V. Conclusion

In this article, we have endeavoured to draw the field of empirical legal research further into a movement that is going on elsewhere in social science. One component of that movement is the study of failures to replicate previous findings. Here, we disagree with IHWR's general contention that the replication crisis in psychology had, at the time, been 'overhyped';[158] this is also a position that appears difficult to sustain in light of research that was published after IHWR's work. And as to IHWR's conclusions about the three effects they replicated, we think that hidden moderators are an incomplete explanation for the weaker and non-significant results they found. Of course, it is only thanks to their efforts that we are able to draw these conclusions about reproducibility in legal research.

The possibility that researcher degrees of freedom, small sample sizes, and publication bias contributed to the results described in the studies that IHWR failed to replicate should

[156] Kahneman et al (n 140) 218-20.
[157] SKSR (n 139) 1173.
[158] IHWR (n 4) 346.

prompt re-evaluation of current practices and guidelines. We end with two such implications of our work.

The first implication is focused on empirical legal research, wherein the practices recommended in Part III appear to be uncommon.[159] We suggest that empirical legal researchers begin using more open and transparent practices when appropriate. Legal research often has legal and policy implications, and so users of this research should be able to scrutinize its data and methods. Moreover, open practices are more efficient in the long run because researchers can more easily stand on each other's shoulders, using extant data and methods to test new questions. Transparency can head off questions about researcher bias, which may be especially relevant in research focused on politicized topics. For instance, the researchers that failed to replicate the finding that female High Court judges are interrupted more noted that had the original study 'chosen at random any two-and a half Terms between 2005 and 2013 to study instead of those coming after 2014, the initial results would have suggested gender bias against male justices'.[160] Preregistration can be valuable in cases like this because the original author can point to the fact that they specified a timeframe before seeing the data, minimizing any researcher degrees of freedom where bias could have an effect.

Journal editors may wish to follow suit and expect more transparency in the research they publish. They could require or encourage preregistration, open materials, and open data. They may also wish to devote issues to replication projects, as one law and economics journal recently began to do.[161] Finally, peer review from methodological experts may improve the quality of the

[159] Zeiler (n 3).
[160] Jacobi, Robinson, and Leslie (n 21).
[161] Program on Empirical Legal Studies, *Call for Papers: Empirical Legal Studies Replication Conference, Spring 2019* <https://www.cmc.edu/robert-day-school/call-for-papers-empirical-legal-studies-replication-conference-spring-2019>.

research they publish. One professor suggested that the original study of judges being interrupted should have been subjected to more thorough peer review.[162] Here, Australian law journals may benefit from the fact that Australia is home to a large concentration of meta-researchers and researchers specifically interested in methodology.[163]

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[162] Jeremy_gans (Twitter), 26 August 2020, <https://twitter.com/jeremy_gans/status/1298572380139220992?s=20>: '(Somewhat biting the hand that feeds me, the real blame here lies with @MelbULRev and its peer reviewers. MULR needs to be much more careful about empirical papers. And it should think twice about reusing whoever reviewed Loughland's article in future.)'.

[163] Australian meta-research groups and organisations are: The MetaMelb research group <https://www.metamelb.org/>, which is at the University of Melbourne; The Deakin Lab for the Meta-Analysis of Research (DeLMAR) <https://www.deakin.edu.au/business/research/delmar>; The Inter-Disciplinary Ecology and Evolution Lab <http://www.i-deel.org/> at the University of New South Wales; The Association for Interdisciplinary Meta-research and Open Science (AIMOS) <https://aimos.community/>, which currently has over 250 members; The Australian Reproducibility Network <https://instituteebh.wixsite.com/website-4>.

Table 1. Results of large-scale replication projects

Table 1. Large-scale replication projects from disciplines with methods similar to empirical law and psychology research.

Study	Field	Number of replication studies	% of results statistically significant in the original direction
RP:P (Open Science Collaboration, 2015)	Psychology	97	36%
EERP (Camerer et al., 2016)	Economics	18	61%
Many Labs 1 (Klein et al., 2014)	Psychology	16	88%
Many Labs 2 (Klein et al., 2018)	Psychology	28	54%
Many Labs 3 (Ebersole et al., 2016)	Psychology	9	33%
Science & Nature (Camerer et al., 2016)	Social Sciences	21	62%

Table 2. ANOVA predicting willingness to contribute to a public cause

Table 2. ANOVA with the following independent variables: panel (in lab, SSI, MTurk), solejoint (joint evaluation, separate evaluation), and harm type (human, environmental). The dependent variable is willingness to contribute to the cause.

Effect	F	df_1	df_2	MSE	p	$\hat{\eta}^2_G$
Panel	257.07	2	3,640	35,810.48	< .001	.124
Solejoint	2.42	1	3,640	35,810.48	.120	.001
Harm	1.50	1	3,640	35,810.48	.220	.000
Panel × Solejoint	0.12	2	3,640	35,810.48	.889	.000
Panel × Harm	0.41	2	3,640	35,810.48	.665	.000
Solejoint × Harm	4.66	1	3,640	35,810.48	.031	.001
Panel × Solejoint × Harm	0.58	2	3,640	35,810.48	.560	.000

Table 3. ANOVA predicting punitive damages for legal cases

Table 3. ANOVA with following independent variables: panel (in lab, SSI, MTurk), solejoint (joint evaluation, separate evaluation), and harm type (human, financial). The dependent variable is punitive damages awarded. Due to the large mean square errors, we present both the raw and log-transformed damages.

Raw

Effect	F	df_1	df_2	MSE	p	$\hat{\eta}^2_G$
Panel	1.57	2	3,640	273,482,173,934,662,701,710,101,632,079,660,204,198,723,584.00	.209	.001
Solejoint	1.81	1	3,640	273,482,173,934,662,701,710,101,632,079,660,204,198,723,584.00	.178	.000
Harm	1.81	1	3,640	273,482,173,934,662,701,710,101,632,079,660,204,198,723,584.00	.178	.000
Panel × Solejoint	1.57	2	3,640	273,482,173,934,662,701,710,101,632,079,660,204,198,723,584.00	.209	.001
Panel × Harm	1.57	2	3,640	273,482,173,934,662,701,710,101,632,079,660,204,198,723,584.00	.209	.001
Solejoint × Harm	1.81	1	3,640	273,482,173,934,662,701,710,101,632,079,660,204,198,723,584.00	.178	.000
Panel × Solejoint × Harm	1.57	2	3,640	273,482,173,934,662,701,710,101,632,079,660,204,198,723,584.00	.209	.001

Log transformed

Effect	F	df_1	df_2	MSE	p	$\hat{\eta}^2_G$
Panel	26.92	2	3,640	17.50	< .001	.015
Solejoint	1.13	1	3,640	17.50	.287	.000
Harm	109.34	1	3,640	17.50	< .001	.029
Panel × Solejoint	1.30	2	3,640	17.50	.273	.001
Panel × Harm	0.94	2	3,640	17.50	.392	.001
Solejoint × Harm	4.39	1	3,640	17.50	.036	.001
Panel × Solejoint × Harm	0.94	2	3,640	17.50	.391	.001

Figure 1. Effect of apology type on settlement acceptance

Figure 1. Proportion of participants who indicated they would accept settlement (by choosing probably or definitely) by condition (full vs none vs partial). The error bars are 95% confidence intervals.

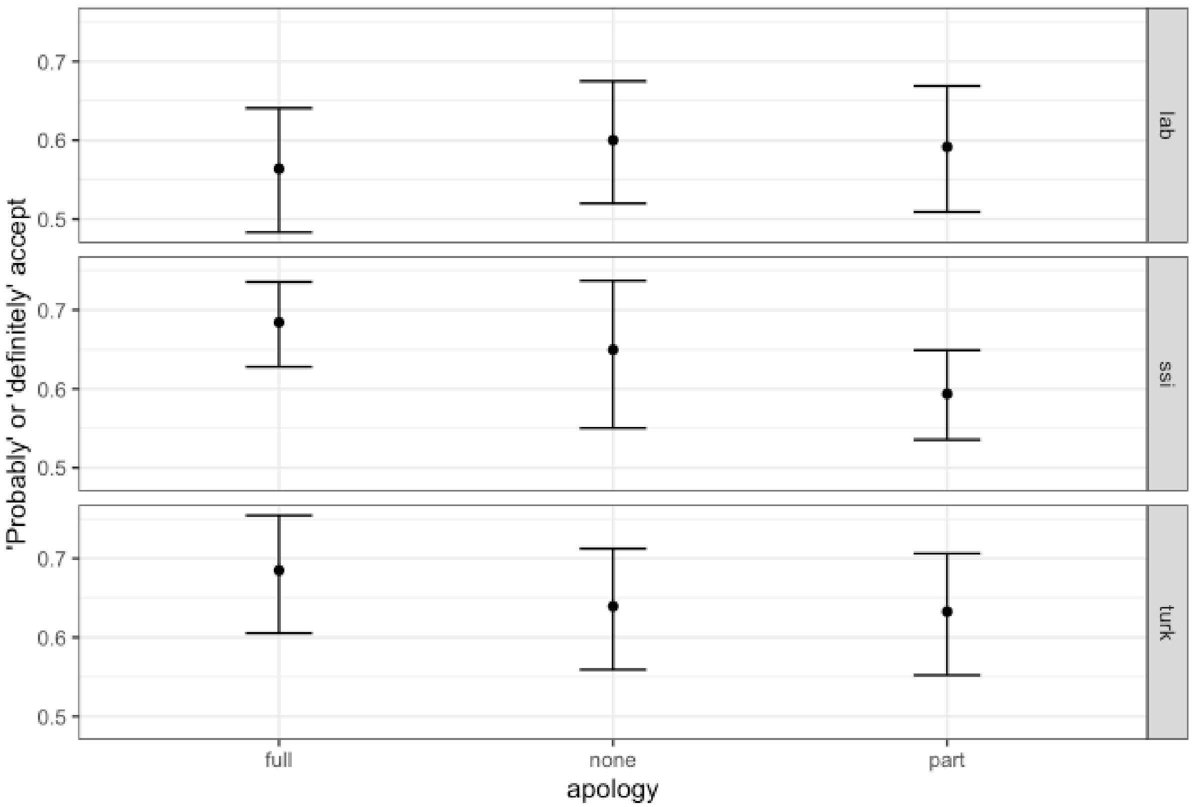

Figure 2. Effect of gain or loss framing on settlement across two original studies, direct replications, and 'judicious replications'

Figure 2. The difference in the proportion of plaintiffs settling and defendants settling. Positive differences indicate plaintiffs prefer settling more than defendants, suggesting greater risk aversion among plaintiffs. Rows are combinations of panel and stakes. The panels are: Rachlinski's original studies in his lab; IHWR's direct replication in three platforms; and IHWR's judicious replications in MTurk and Prolific. Stakes are the amount that could be won or lost if the case went to trial. Red indicates the property scenario and blue is the trademark scenario. The 'losing' column contains scenarios where the odds favour the defendant and 'winning' those favouring the plaintiff. Error bars are 95% confidence intervals.

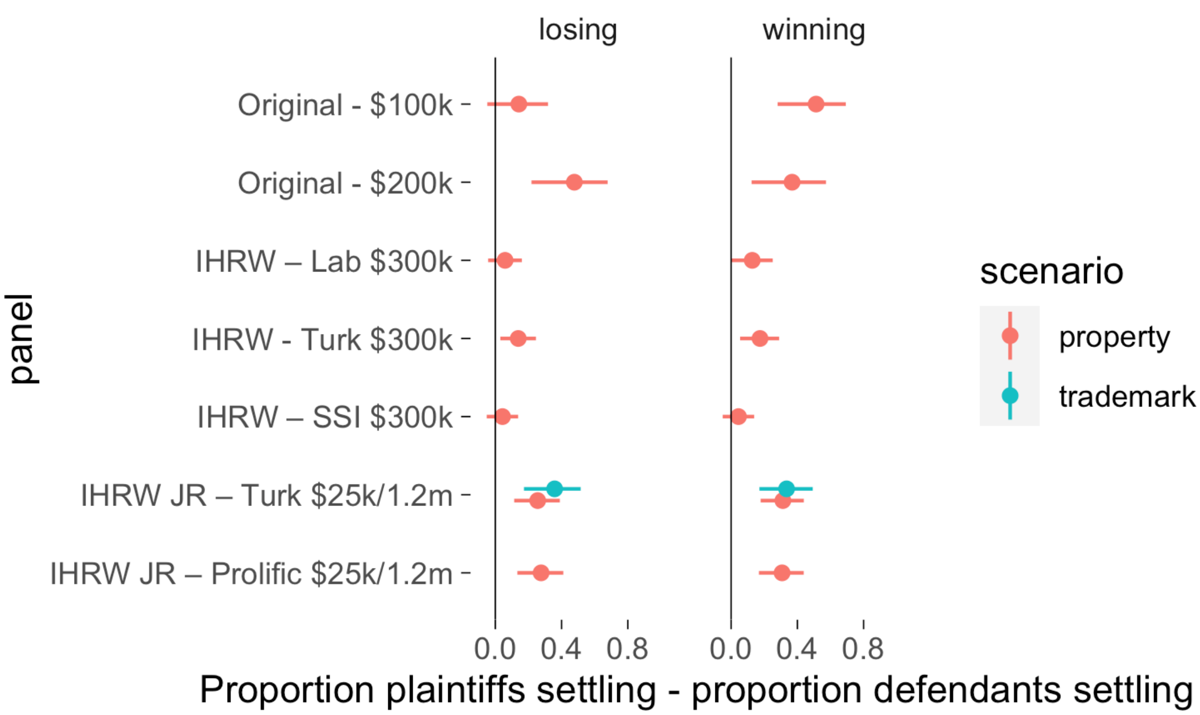

Figure 3. Mean punitive damages in joint and separate evaluation

Figure 3. Our computational reproduction (i.e., re-analysis of IHWR's data) of the mean punitive damages awarded in cases of financial (red lines) and physical (blue lines) harm, assessed either separately or jointly (i.e., in comparison to the case of another type of harm). The panels presented are in lab, SSI, and MTurk. Error bars are 95% confidence intervals.

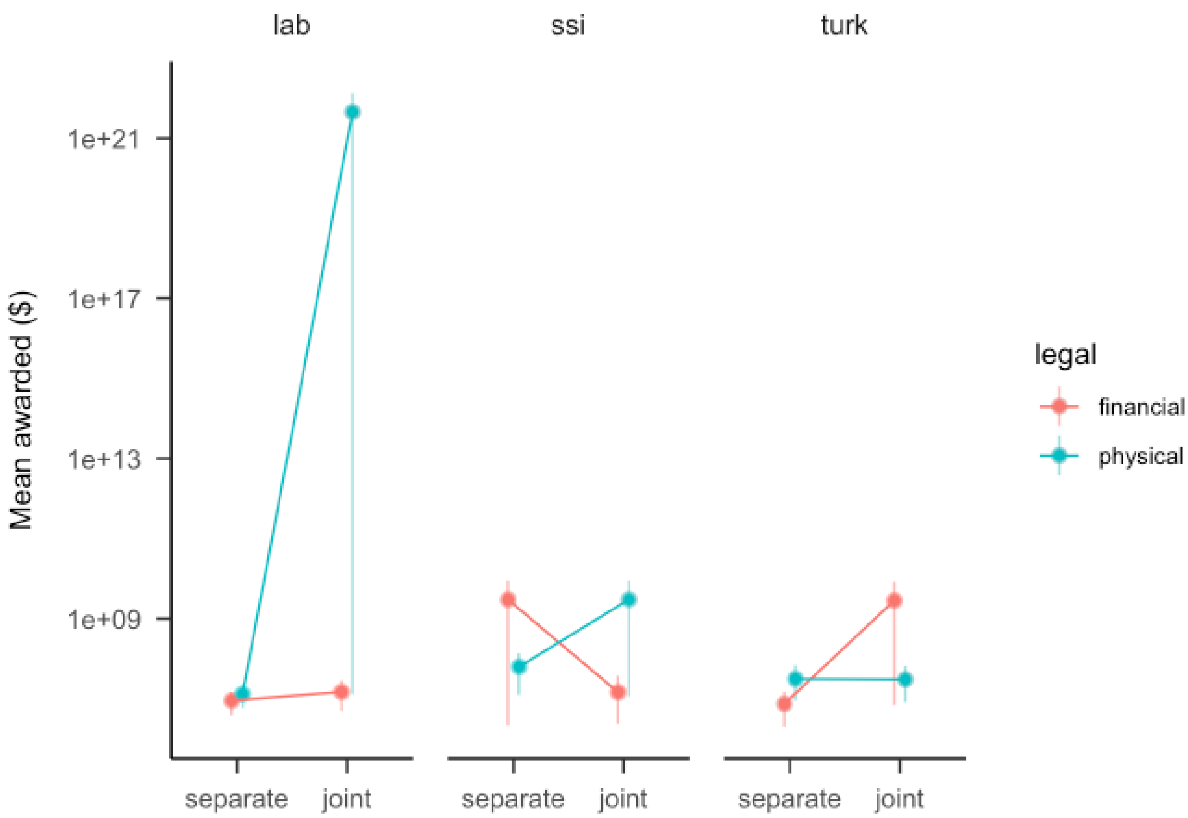

Figure 4. Median punitive damages in joint and separate evaluation

Figure 4. Our computational reproduction (i.e., re-analysis of IHWR's data) of the median punitive damages awarded in cases of financial (red lines) and physical harm (blue lines), assessed either separately or jointly (i.e., in comparison to the case of another type of harm). The panels presented are in lab, SSI, and MTurk. Error bars are 95% confidence intervals.

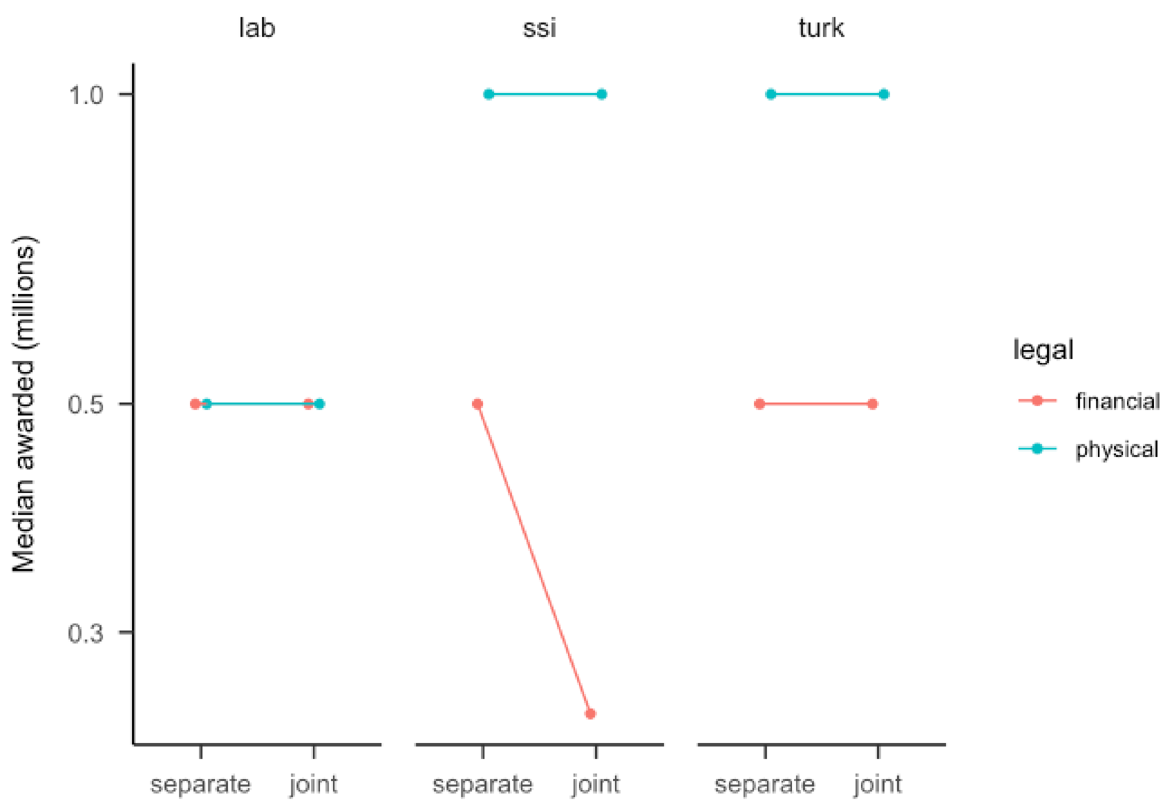

Figure 5. Mean willingness to contribute to a public cause in joint and separate evaluation

Figure 5. Our computational reproduction (i.e., re-analysis of IHWR's data) of the mean willingness to contribute to a public cause helping either the environment (red lines) or humans (blue lines), assessed either separately or jointly (i.e., in comparison to the case of another type of harm), after setting all responses over $500 to $500. The panels presented are in lab, SSI, and MTurk. Error bars are 95% confidence intervals.