



RESEARCH ARTICLE

REVISED Stage 2 Registered Report: Anomalous perception in a Ganzfeld condition - A meta-analysis of more than 40 years investigation [version 4; peer review: 2 approved, 1 not approved]

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Abstract

This meta-analysis is an investigation into anomalous perception (i.e., conscious identification of information without any conventional sensorial means). The technique used for eliciting an effect is the Ganzfeld condition (a form of sensory homogenization that eliminates distracting peripheral noise). The database consists of studies published between January 1974 and December 2020 inclusive.

The overall effect size estimated both with a frequentist and a Bayesian random-effect model, were in close agreement yielding an effect size of approximately .08 (.04 -.12). This result passed four publication bias tests and seems not contaminated by questionable research practices.




Trend analysis carried out with a cumulative meta-analysis and a meta-regression model with year of publication as a covariate, did not indicate sign of decline of this effect size.

The moderators' analyses show that the selected participants' effect size was almost three-times that obtained by non-selected participants and that tasks that simulate telepathic communication show a two-fold effect size for tasks requiring the participants to guess a target.

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Registered Report, meta-analysis, ganzfeld, anomalous perception

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REVISED Amendments from Version 3

Corrected some typos and accepted most of reviewer 2's suggestions related to the publication bias analyses.

Any further responses from the reviewers can be found at the end of the article

Introduction

The possibility of identifying pictures or video clips without conventional (sensorial) means, in a ganzfeld environment, is a decades old controversy, dating back to the pioneering investigation of Charles Honorton, William Braud and Adrian Parker between 1974 and 1975 (Parker, 2017).

In the prototypical procedure, the participant (as percipient) is tested in a room isolated from external sounds and visual information. After he/she is made comfortable in a reclining armchair, he/she receives instructions related to the task during the ganzfeld condition. Even if there are different verbatim versions, the instructions describe what the percipient should do mentally in order to detect the information related to the target and how to filter out the mental contents not related to it. This information is described aloud and recorded for playback before or during the target identification phase. After a relaxation phase, which can range from 5 to 15 minutes, the percipient is exposed to the ganzfeld condition for a period ranging from 15 to 30 minutes.

In the ganzfeld environment, a German term meaning 'whole field', participants are immersed in a homogeneous sensorial field where peripheral visual information is masked out by red light diffused by translucent hemispheres (often split halves of ping-pong balls or special glasses) placed over the eyes, while a relaxing rhythmic sound, or white or pink noise, is fed through headphones to shield out peripheral auditory information.

Once the participant is sensorially isolated from external visual and auditory stimulation, he/she is thought to be in a favorable condition for producing inner mental contents about a randomly-selected target hidden among some decoys, usually three or four.

During this phase, the participant describes verbally all images, feelings, and emotions, they deem related to the target, which is usually a picture or a short video-clip of real objects or events.

The mentation (verbal report) produced by the participant can either be used to guide his/her target selection, or it can be used by the judge to assist in an independent judging process.

A variant of the judgment phase is to send the recording of the information retrieved during the ganzfeld phase to an external judge for independent ratings of the target. In order to prevent voluntary or involuntary leakage of information about the target by the experimenters, the research assistant who interacts with the participants must be blind to the target identity until the participants' rating task is over.

The choice of the target and the decoys is usually made using automatic random procedures, and scores are automatically fed onto a scoring sheet.

There are three different ganzfeld conditions:

- Type 1 (Precognition): the target is chosen after the judgment phase;
- Type 2 (Clairvoyance): the target is chosen before the ganzfeld phase;
- Type 3 (Telepathy): the target is chosen before the ganzfeld phase and presented to a partner of the participant isolated in a separate and distant room. From an historical perspective, this last type is considered the typical condition.

These differences are related to some theoretical and perceptual concepts we will discuss later. It is important to note that type of task makes no difference to the participant who only engages in target identification *after* the ganzfeld phase.

Review of the Ganzfeld Meta-Analyses

It is interesting to note that most of the cumulative findings (meta-analyses) of this line of investigation were periodically published in the journal *Psychological Bulletin*.

Honorton (1985) undertook one of the first meta-analyses of the many ganzfeld studies completed by the mid-1980s. In total, 28 studies yielded a collective hit rate (correct identification) of 38%, where mean chance expectation (MCE) was 25%. Various flaws in his approach were pointed out by Hyman (1985), but in their joint-communicé they agree that “there is an overall significant effect in this database that cannot reasonably be explained by selective reporting or multiple analysis” (Hyman & Honorton, 1986, p. 351).

A second major meta-analysis on a set of ‘autoganzfeld’ studies was performed by Bem & Honorton (1994). These studies followed the guidelines laid down by Hyman & Honorton (1986). Moreover, the autoganzfeld procedure avoided methodological flaws by using a computer-controlled target randomization, selection, and judging technique. The overall reported hit rate of 32.2% exceeded again the mean chance expectation.

Milton & Wiseman (1999) meta-analyzed further 30 studies collected for the period 1987 to 1997, reporting an overall nonsignificant standardized effect size of 0.013. However, Jessica Utts (personal communication, December 11, 2009) using the exact binomial test on trial counts only ($N = 1198$; Hits = 327), found a significant hit rate of 27% ($p = 0.036$).

Storm & Ertel (2001) comparing Milton & Wiseman’s (1999) database with Bem & Honorton’s (1994) one, found the two did not differ significantly. Furthermore, Storm and Ertel went on to compile a 79-study database, which had a statistically significant average standardized effect size of 0.138.

Storm *et al.* (2010), meta-analyzed a database of 29 ganzfeld studies published during the period 1997 to 2008, yielding an average standardized effect size of 0.14. Rouder *et al.* (2013) reassessing Storm *et al.*’s (2010) meta-analysis with a Bayesian approach, found evidence for the existence of an anomalous perception in the original dataset observing a Bayes Factor of 330 in support of the alternative hypothesis (p. 241). However, they contended the effect could be due to “difficulties in randomization” (p. 241), arguing that ganzfeld studies with computerized randomization had smaller effects than those with manual randomization. The reanalysis by Storm *et al.*’s (2013) showed that this conclusion was unconvincing as it was based on Rouder *et al.*’s faulty inclusion of different categories of study.

In the last meta-analysis by Storm & Tressoldi (2020), related to the studies published from 2008 to 2018, the overall standardized effect size was 0.133; 95%CI: 0.06-0.18.

This study

The main aim of this study is to meta-analyze all available ganzfeld studies dating from 1974 up to December 2020 in order to assess the average effect size of the database with the more advanced statistical procedures that should overcome the limitations of the previous meta-analyses, e.g., the use of a common instead of a random model to take in account studies experimental designs heterogeneity, and the controls of publication bias. Furthermore, we aim to identify whether there are moderator variables that affect task performance. In particular, we hypothesize that participant type and type of task are two major moderators of effect size (see Methods section).

Methods

Reporting guidelines

This study follows the guidelines of the APA Meta-Analysis Reporting Standard (Appelbaum *et al.*, 2018) and the Preferred Reporting Items for Systematic Review and Meta-Analysis Protocols (PRISMA-P, Moher *et al.*, 2015).

All following analyses have been approved in the Stage 1 of this Registered Report (Tressoldi & Storm, 2021). Supplementary and new analyses not approved in the Stage 1, are reported in the *Exploratory analyses* section in the Results.

Studies retrieval

Retrieval of studies related to anomalous perception in a Ganzfeld environment is simplified, firstly by the fact that most of these studies have already been retrieved for previous meta-analyses, as cited in the introduction. Secondly, this line of investigation is carried out by a small community of researchers. Thirdly, most of the studies of interest to us are published in specialized journals that adopted the editorial policy of accepting papers with results that are statistically non-significant (according to the frequentist approach). This last condition is particularly relevant because it reduces the publication bias due to non-publication (file drawer effect) of studies with statistically non-significant results often as a consequence of a reduced statistical power.

Furthermore in order to integrate the previous retrieval method, we carried-out an online search with [Google Scholar](#), [PubMed](#) and [Scopus](#) databases of all papers from 1974 to 2020 including in the title and/or the abstract the word “ganzfeld” (e.g., for PubMed: Search: ganzfeld [Title/Abstract] Filters: from 1974 – 2020).

Studies inclusion criteria

The following inclusion criteria were adopted:

- Studies related to anomalous perception in a ganzfeld environment;
- Studies must use human participants only (not animals);
- Total number of participants in a study must be in excess of two to avoid the inherent problems that are typical in single case studies;
- Target selection must be randomized by using a true or a pseudo random algorithm Random Number Generator (RNG) in a computer or similar electronic device, or a table of random numbers.
- Randomization procedures must not be manipulated by the experimenter or participant;
- Studies must provide sufficient information (e.g., number of trials and outcomes) for the authors to calculate the direct hit-rates and effect size values, so that appropriate statistical tests can be conducted.
- Peer reviewed and not peer-reviewed studies e.g., published in proceedings excluding dissertations.

Variables coding

For each included study, one of the authors, expert in meta-analyses, coded the following variables:

- Authors;
- Year of publication;
- Number of trials;
- Number of hits;
- Number of choices of each trial;
- Task type (Type 1,2 or 3);
- Participants type (selected vs. unselected). The authors of the study scored as ‘selected’ all participants that were screened for one or more particular characteristic deemed favourable for the performance in this type of task. All others were coded as ‘non-selected’
- Peer-Review level: Level = 1 for studies published in conference proceedings and *Researches In Parapsychology* (moderate peer-review); Level = 2, for the studies published in scientific journals with full peer-review.

The second author randomly checked all studies, and the data was compared with those extracted by the other author. Discrepancies were corrected by inspecting the original papers.

The complete database with all supporting information is available as *Underlying data* ([Tressoldi & Storm, 2020](#)).

Effect size measures

As standardized measure of effect size, we used the one applied in [Storm, Tressoldi & Di Risio \(2010\)](#) and [Storm & Tressoldi \(2020\)](#): binomial Z score/ $\sqrt{\text{number of trials}}$, using the number of trials, the hits score and the chance probability as raw scores. The exact binomial Z score has been obtained applying the formula implemented online at <http://vassarstats.net/binomialX.html>. When this algorithm did not compute the z value when either number of trials or number of hits were low, we used the one-tailed exact binomial p-value, to find the inverse normal z by using the online app at <http://www.fourmilab.ch/rpkp/experiments/analysis/zCalc.html> where the formula of this conversion is described.

The standardized effect size was computed applying the formula Z/\sqrt{N} of trials.

As standard error, we used the formula: $\sqrt{(\text{hit rate proportion} * (1 - \text{hit rate proportion}) / \text{trials} * \text{chance proportion} * (1 - \text{chance proportion}))}$.

In order to take into account the effect size overestimation bias in small samples, the effect sizes and their standard errors, were transformed into the Hedge's g effect sizes, with the corresponding standard errors by applying the formula presented in [Borenstein et al. \(2009\)](#), pp. 27–28: $g = (1 - (3/(4df - 1))) * d$.

Pooled estimate of the average effect

In order to account for the between-studies heterogeneity, the overall effect size estimation of the whole database has been calculated by applying both a frequentist and a Bayesian random-effect model for testing its robustness.

Frequentist random-effect model

Following the recommendations of [Langan et al. \(2019\)](#), we used the restricted maximum likelihood (REML) approach to estimate the heterogeneity variance with the Knapp and Hartung method for adjustment to the standard errors of the estimated coefficients ([Rubio-Aparicio et al., 2018](#)).

Furthermore, in order to control for possible influence of outliers, we calculated the median and mode of the overall effect size applying the method suggested by [Hartwig et al. \(2020\)](#).

These calculations were implemented in the R statistical environment v.4.0.3 with the [metafor](#) package v. 2.4 ([Viechtbauer, 2017](#)). See syntax details provided as extended data ([Tressoldi & Storm, 2020](#)).

Bayesian random-effect model

As priors for the average effect size we used a normal distribution with Mean = 0.1; $SD = 0.03$, constrained positive, lower bound = 0 ([Haaf & Rouder, 2020](#)), given our expectation of a positive value. As prior for the tau parameter we used an inverse gamma distribution with shape = 1, scale = 0.15.

This Bayesian meta-analysis was conducted using the [MetaBMA](#) package v. 0.6.7 ([Heck et al., 2017](#)).

Publication bias tests

Following the suggestions of [Carter et al. \(2019\)](#), we applied four tests to assess publication bias:

- the 3-parameter selection model (3PSM), as implemented by [Coburn & Vevea \(2019\)](#) with the package 'weightr' v.2.0.2;
- the p -uniform* (star) v. 0.2.5 test as described by [van Aert & van Assen \(2019\)](#), and
- the sensitivity analysis using the [Mathur & VanderWeele \(2020\)](#) package [PublicationBias](#) v.2.4.0.
- The Robust Bayesian meta-analysis test implemented with the RoBMA package v.2.3.1 ([Bartoš & Maier, 2020](#)).

The three parameters model represents the average true underlying effect, δ , the heterogeneity of the random effect sizes, τ^2 and the probability that there is a nonsignificant effect in the pool of effect sizes. The probability parameter is modelled by a step function with a single cut point at $p = 0.025$ (one-tailed), which corresponds to a two-tailed p value of 0.05. This cut point divides the range of possible p values into two bins: significant and nonsignificant. The three parameters are estimated using maximum likelihood ([Carter et al., 2019](#), p. 124).

The p -uniform* test, is an extension and improvement of the p -uniform method. P -uniform* improves upon p -uniform giving a more efficient estimator avoiding the overestimation of effect size in case of between-study variance in true effect sizes, thus enabling estimation and testing for the presence of between-study variance in true effect sizes.

Sensitivity analysis, as implemented by [Mathur & VanderWeele \(2020\)](#), assumes a publication process such that "statistically significant" results are more likely to be published than negative or "nonsignificant" results by an unknown ratio, η (eta). Using inverse-probability weighting and robust estimation that accommodates non-normal true effects, small meta-analyses and clustering, it enables statements such as: "For publication bias to shift the observed point

estimate to the null, ‘significant’ results would need to be at least 30-fold more likely to be published than negative or ‘non-significant’ results” (p. 1). Comparable statements can be made regarding shifting to a chosen non-null value or shifting the confidence interval.

The Robust Bayesian meta-analysis test is an extension of Bayesian meta-analysis obtained by adding selection models to account for publication bias. This allows model-averaging across a larger set of models, ones that assume publication bias and ones that do not. This test allows us to quantify evidence for the absence of publication bias estimated with a Bayes Factor. In our case we compared only two models, a random-effect model assuming no publication bias and a random-model assuming publication bias.

Cumulative meta-analysis

In order to ascertain the overall trend of the cumulative evidence and in particular for testing the presence of a positive or negative trend effect, we performed a cumulative effect size estimation.

Meta-regression

Furthermore, we estimated the overall effect size taking the variable “year of publication” as covariate using a meta-regression model.

Moderators effects

We compared the influence of the following three moderators: (i) Type of participant, (ii) Type of task and (iii) Level of peer-review.

As described in the Variable Coding paragraph, the variable Type of participant, has been coded in a binary way: selected vs unselected. Type of task has been coded as Type 1, Type 2, and Type 3, as described in the Introduction and level of Peer-review as 1 for studies published in conference proceedings or 2, for the studies published in scientific journals with full peer-review.

Statistical power

The overall statistical power was estimated using R package *metameta* v.0.1.1 (Quintana, 2020). Furthermore, we calculated the number of trials necessary to achieve a statistical power of at least .80 with an $\alpha = .05$. With this estimation we examined how many studies in the database reached this threshold.

Results

The search and selection of the studies is presented in the PRISMA flowchart in [Figure 1](#). As shown in the flowchart, our final database comprises 78 studies, for a total of 113 effect sizes carried out by 46 different principal investigators.

The list of all references related to the included and excluded studies is available in the GZMAReference List file and the data used for all the following statistical analyses is available in the GZMADatabase1974_2020 file in the *Underlying data* (Tressoldi & Storm, 2020).

Descriptive statistics

Descriptive statistics related to the variables trials, hits above chance, participants type, task types, peer-review level are presented in [Table 1](#).

Comment: The range of the number of trials as well as the hits percentage is quite wide. The number of task types show that the main types are Type 2: the target is chosen before the ganzfeld phase) and of Type 3: the target is chosen before the ganzfeld phase and presented to a partner of the participant isolated in a separate and distant room. Type 1 studies (target randomly selected after participant makes a choice) are only 5 (4.2%).

The percentage of studies using non-selected participants is greater (62% vs 38%) than that of studies using selected. Most studies (58.4%) were peer-reviewed.

Pooled estimate of the average effect

The estimate of the average effect along with the corresponding 95% Confidence Intervals or Credible Intervals of both the frequentist and the Bayesian random-effect models as described in the Methods section, and values of τ^2 and I^2 (Higgins & Thompson, 2002) with their confidence intervals, as measures of between-study variance, are presented in [Table 2](#).

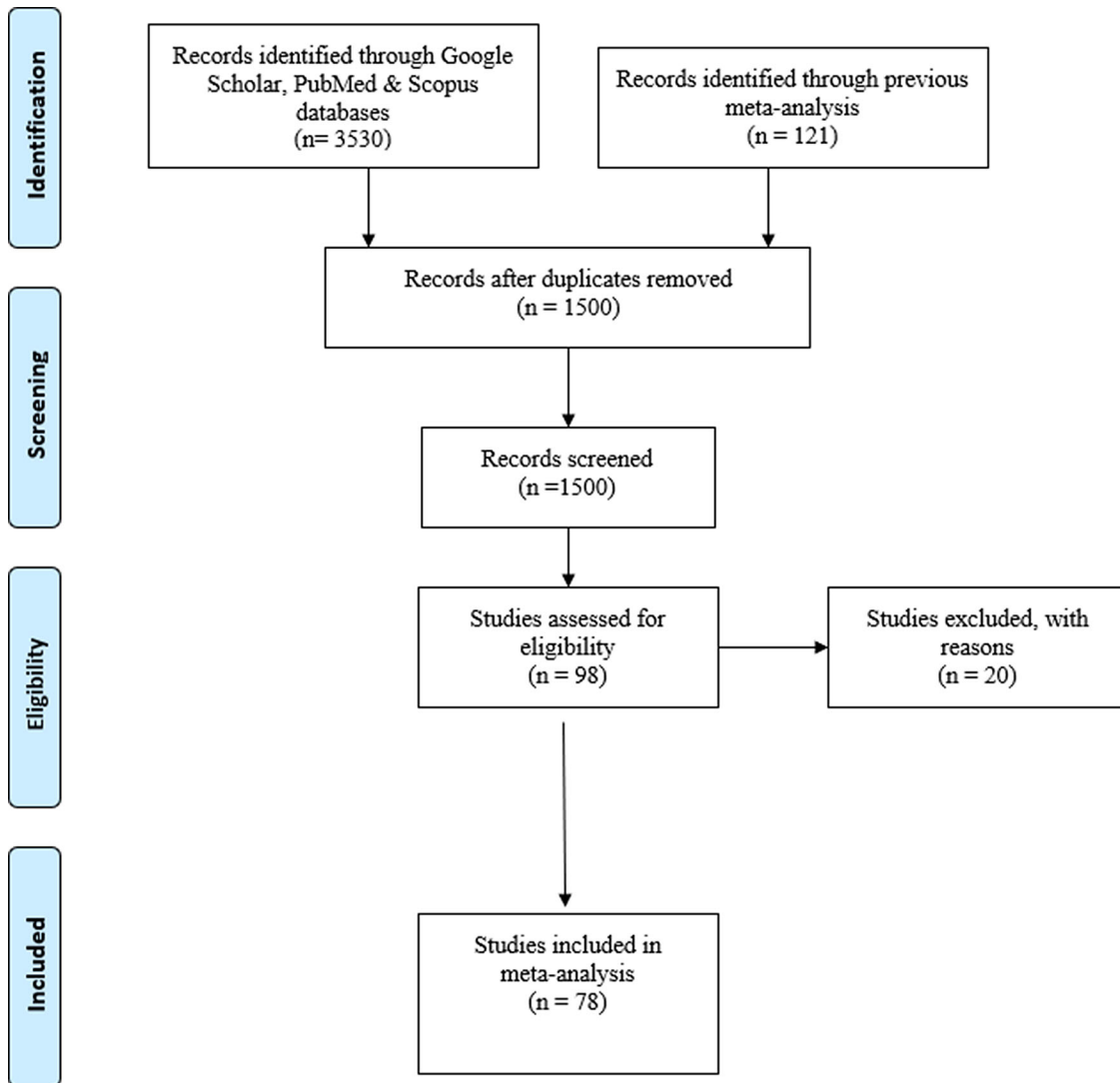


Figure 1. PRISMA flowchart.

Table 1. Descriptive statistics of the main variables.

	Trials	Hits rate above chance	Task Type	Participants Type	Peer-review level
Mean (SD)	42.8 (27.2)	.068 (.11)*			
Range	4-138	-.2 - .60			
Count (%)			Type 1: 5 (4.2) Type 2: 27 (23.8) Type 3: 81 (72)	Non-selected: 70 (62) Selected: 43 (38)	Level 1: 47 (41.6) Level 2: 66 (58.4)

*= this value is purely descriptive because not all studies are 4 free-choice designs.

Comment: The frequentist and the Bayesian random-effect model parameters estimations are in close agreement, and both reject the null (H0) hypothesis with a high probability.

See also the reviewer 2 results based on the weighted least squares model.

In terms of hits percentage above chance, this small effect size corresponds to 6.8% (95%CIs: 4.7 – 8.9).

Table 2. Frequentist and Bayesian random-effect model results.

	Frequentist weighted ES (95% Confidence Intervals)	Bayesian weighted ES (95% Credible Intervals)	τ^2 (95% Confidence Intervals)	I^2 (95% Confidence Intervals)
Mean	.074 (.03 - .12)	.084 (.05 - .12)	.03 (.02 - .04)	63.8 (49.8 - 69.9)
P value or Bayes Factor _(H1/H0)	.0009	89.5		

The level of heterogeneity is medium-large as expected by the influence of the moderators. Given this heterogeneity level, the values of the effect size median = .017 (-.025 - .06) and mode -.01 (-.13 - .10), are uninformative.

The forest plot is available as Figure S1 (*Extended data* (Tressoldi & Storm, 2020)).

Outliers detection and influence

In order to detect the presence of influential outliers, we applied the “influence” function in the metafor package. These procedures identified two influential outliers. The results of the frequentist random-effect model without the influential outliers, are very similar to those with the outliers (mean ES: .078; 95% CIs: .03 - .12).

Cumulative effect size

The results of the cumulative meta-analysis are represented with a cumulative forest plot in Figure S2 (*Extended data* (Tressoldi & Storm, 2020)). From the inspection of the cumulative forest plot, it emerges that the overall effect size stabilized around the cumulative evidence obtained up to 1997. Thus, it appears to be cumulative stable for more than 20 years.

Meta-regression

The results of the meta-regression with “Year” as covariate, show a slope estimate of .0012 (95% CIs: -.002 - .005; $p = .53$).

Comment: These results support the hypothesis that the overall effect size is not affected by the year of publication of the experiments.

Exploratory analyses

Another way to observe the cumulative trend of the overall effect size, is to examine the evolution of the Bayes Factor and of Posterior Probability of H1 as the data accumulate. This information has been obtained using the option “sequential Bayes Factor” within the module Bayesian Meta-Analysis in the software JASP v.0.17.0 (Jasp, 2020) that are presented in Figures S3 (*Extended data* (Tressoldi & Storm, 2020)). From these two plots it is possible to observe how the Bayes Factor started a positive linear trend after approximately 70 experiments. The maximum Posterior probability is achieved after approximately 80 experiments. The JASP file is available as *Underlying data* (Tressoldi & Storm, 2020).

Publication Bias tests

The results of the four publication bias tests described in the Methods section are presented in Table 3 and in the information that follows.

Table 3. Results of the p-uniform*, the 3-parameter selection model (3PSM), and the Robust publication bias tests.

	p-uniform*	3PSM	RoBMA
ES	.12	.15	.074
95% CIs	.04 - .21	.05 - .24	.00 - .12

Table 4. Effect sizes and 95% CIs related to the moderators' categories.

	Selected Participants	Non-selected participants	Task Type 1	Task Type 2	Task Type 3	Peer-review level 1	Peer-review level 2
ES	.13	.04	.12	.04	.08	.073	.076
95% CIs	.06 - .20	-.01 - .09	-.08 - .33	-.04 - .13	.03 - .14	.01 - .13	.01 - .14

The results of the Mathur & VanderWeele (2020) sensitivity analysis publication bias to shift the observed effect size point estimate to the .01 level, considered arbitrarily as the smallest effect size of interest, indicated that for publication bias to attenuate (to “explain away”) the observed overall effect size, affirmative results would need to be at least 4 fold more likely to be published than nonaffirmative results. See also reviewers' 2 for further information about this test.

Comment: The overall effect size estimate passes all four publication bias tests.

Moderators analyses

The weighted effect size along with the corresponding 95% confidence Intervals of the two types of participants, the three task types and the two peer-review level, are presented in Table 4.

Exploratory analysis

After looking at the participants selection and Task Type results, it was interesting to learn that selected participants and Task Type 3 combined, gave: $ES = .14$; 95%CIs: .06 - .22; not different from the results obtained by the selected participants in all three types of tasks.

Comment: Whereas it is clear that the levels of peer-review did not yield differences in the effect sizes, the selection of participants and the Task Types show substantial and statistically significant differences.

Selected participants show an almost three-fold increase in the effect size with respect to the non-selected participants. In terms of hits percentage above chance, this difference corresponds to 9.1%; 95%CI: 5-13 and 5.4%; 95%CI: 2.9 – 7.9, respectively.

Similarly, Tasks Type 1 and 3 show more than two-fold increase in ES compared to Type 2 tasks. However, the effect size observed with tasks Type 1, must be considered with caution given the low number of experiments (5).

Statistical power

The median statistical power related to the observed overall effect size is .088. This result explains the fact that only 30 (22.5%) of the studies reported statistically significant results. For additional analyses related to the statistical power, see the reviewers' 2 review.

Discussion

The main aim of this meta-analysis was to get an overall picture of the evidence accumulated in more than 40 years of investigation related to an anomalous perception in a ganzfeld environment.

The main aim of this meta-analysis was to obtain an overall picture of the evidence accumulated over more than 40 years of investigation related to anomalous perception in a Ganzfeld environment.

The estimate of the average effect from 113 studies carried out from 1974 to June 2020 was small, but it turned out to be robust in both frequentist and Bayesian random-effect models.

As shown by the cumulative analysis and meta-regression with year of publication as covariate meta-analyses, this effect does not show a negative trend from 1974 to 2020 and is quite stable since 1997 and after 70-80 experiments.

Furthermore, the average effect passed four different publication bias tests. This finding suggest that although the meta-analysis is not immune to publication bias (given that a moderate level of bias favoring significant results in the published literature may account for the observed effect size), the meta-analysis results appear relatively robust against the potential impact of this bias. This interpretation is also supported by the low number of studies (22.5%) with statistically significant

results. This outcome is partly due to the practice of publishing statistically non-significant studies in specialized journals and proceedings related to this field of investigation.

Moreover, the similarity of effect size between the two levels of peer-review, add further support to the hypothesis that the “file drawer” is empty, that is that this meta-analysis includes all completed studies.

If we consider the average effect size, the lack of statistically significant results in many experiments is a consequence of their low statistical power, as shown by the very low median statistical power of the meta-analysis.

For those interested in this line of investigation the advice is clear. To achieve a statistical power of at least 0.80 with an alpha value of 0.05, one-tailed, each study must have at least 245 trials (estimated with G*Power, v.3.1.9.7, [Faul, Erdfelder, Lang & Buchner, 2007](#)), to achieve an effect size as observed in this study.

However, this requirement can be reduced considerably if we consider the results of the moderators, in particular, the selection of participants and type of task. With selected participants carrying out a Type 3 task (i.e., with targets chosen before the ganzfeld phase and presented to a partner of the participant isolated in a separate and distant room simulating telepathic communication), the required trials can safely be reduced to 50.

Could the average results be contaminated using some questionable research practices ([John et al., 2012](#)), such as optional stopping, data exclusion, etc.? These practices were difficult to detect after the publication of the study, which is why it is recommended to register all methodological and statistical details before data collection. As far as it concerns this line of investigation, [Wiseman, Watt and Kornbrot \(2019\)](#), documented that preregistration was recommended well before the so-called replication crisis faced by most scientific fields. Furthermore, a simulation of the use of some questionable research practices carried out by [Bierman, Spottiswoode, and Bijl \(2016\)](#) on 78 studies related to anomalous perception in a Ganzfeld environment showed that even if the overall effect size could be inflated by the use of questionable research practices, it was not reduced to zero.

Even if this study is mainly devoted to the statistical analysis of the available evidence, it is important to consider possible theoretical frameworks that could account for such phenomena. Some of these are presented in the review by [Cardena \(2018\)](#) and the book *Transcendent Mind* by [Barušs and Mossbridge \(2017\)](#). As a general theoretical framework, the main assumption is to consider the mind not derived or constrained by their biological correlates but ontologically independent from them, in agreement with some Western and Eastern philosophical interpretations, such as idealism ([Kastrup, 2018](#)), dual-aspect monism ([Walach, 2020](#)), Advaita Vedanta ([Sedlmeier & Srinivas, 2016](#)), etc. If these interpretations of mind and consciousness are valid, what looks impossible or anomalous according to a physicalist or eliminative reductionist interpretation becomes perfectly normal.

Summary and recommendations

The overall picture emerging from this meta-analysis is that there is sufficient evidence to claim that it is possible to observe a non conventional (anomalous) perception in a Ganzfeld environment. The available evidence does not seem to be contaminated by publication bias or questionable research practices. However, to increase the probability of detecting such phenomena, it is recommended to select participants and to use tasks that mimic telepathic communication.

As methodological advice, it is recommended that researchers preregister the methodological and statistical details in open access registries as proposed by [Watt and Kennedy \(2016\)](#) and others, or even better to use a registered report format that makes all procedures more transparent before and during data collection and analysis. One of the best examples to use as a model is the Transparent Psi Project ([Kekecs et al., 2019](#)).

We hope to update the evidence related to anomalous perception in a Ganzfeld environment with a meta-analysis of preregistered studies in the near future.

Data availability

Underlying data

Figshare: Registered Report - Anomalous perception in a Ganzfeld condition: A meta-analysis of more than 40 years investigation, <https://doi.org/10.6084/m9.figshare.12674618.v13> ([Tressoldi & Storm, 2020](#)).

This project contains the following underlying data:

- GZMADatabase1974_2020 (.jasp and.xlsx)
- GZMA Power (.csv)
- GZMA Reference List (.doc)

Extended data

Figshare: Registered Report - Anomalous perception in a Ganzfeld condition: A meta-analysis of more than 40 years investigation, <https://doi.org/10.6084/m9.figshare.12674618.v13> (Tressoldi & Storm, 2020).

This project contains the following extended data:

- Syntax code for Stage 1 and Stage 2 Registered Report
- Figure S1: Forest Plot
- Figure S2: Cumulative Forest Plot
- Figure S3: Sequential Bayes Factor

Reporting guidelines

Figshare: PRISMA checklist for 'Stage 2 Registered Report: Anomalous perception in a Ganzfeld condition - A meta-analysis of more than 40 years investigation', <https://doi.org/10.6084/m9.figshare.12674618.v13> (Tressoldi & Storm, 2020).

Data are available under the terms of the [Creative Commons Attribution 4.0 International license](https://creativecommons.org/licenses/by/4.0/) (CC-BY 4.0).

Acknowledgments

We thank reviewer Dr. Michel-Ange Amorim for his accurate and constructive reviews.

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Open Peer Review

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Version 4

Reviewer Report 28 May 2024

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Michel-Ange Amorim 

Universite Paris-Saclay, Gif-sur-Yvette, Île-de-France, France

The manuscript has been suitably revised and is an important contribution to the scientific literature on anomalous perception in the Ganzfeld.

Competing Interests: No competing interests were disclosed.

Reviewer Expertise: psychophysics, cognitive psychology, cognitive and behavioral neuroscience, psi phenomena

I confirm that I have read this submission and believe that I have an appropriate level of expertise to confirm that it is of an acceptable scientific standard.

Version 3

Reviewer Report 09 May 2024

<https://doi.org/10.5256/f1000research.162122.r245034>

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Michel-Ange Amorim 

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I appreciate the authors' responses and the effort made to revise their manuscript and the

associated syntax code. Nevertheless, there are still some residual typos in the current version 3 of the manuscript, and even more damaging in an analysis (Publication Bias) that has been carried out.

Introduction

"Type 1 (Precogniyion)" should be replaced by "Type 1 (Precognition)";
and "Type 2 (Clayrvoyance)" should be replaced by "Type 2 (Clairvoyance)"

Methods

About the formula, in "Effect size measures" section :

$\sqrt{(\text{hit rate \%} * (1 - \text{hit rate \%}) / \text{trials} * \text{chance proportion} * (1 - \text{chance proportion}))}$

should be replaced by

$\sqrt{(\text{hit rate proportion} * (1 - \text{hit rate proportion}) / \text{trials} * \text{chance proportion} * (1 - \text{chance proportion}))}$.

or by

$\sqrt{(\text{hit rate} * (1 - \text{hit rate}) / \text{trials} * \text{chance proportion} * (1 - \text{chance proportion}))}$.

Results

About the "Publication Bias tests" section of "Exploratory analyses" : "at least 20 fold more likely" should be replaced by "at least 4 fold more likely"

As illustrated in a separate *Jupyter Notebook* (R code and output) file (available through this link: [PDF file2](#)), I explain why the authors show re-run this analysis, possibly together with a complementary fail-safe analysis.

This would allow authors to conclude that the findings suggest that although the meta-analysis is not immune to publication bias (given that a moderate level of bias favoring significant results in the published literature may account for the observed effect size), the meta-analysis results appear relatively robust against the potential impact of this bias as indicated by the high fail-safe N value.

Other Rmd problems :

1) Although I could not test RoBMA in R (due to a problem with jags), the results of this analysis are in the GZMADatabase1974_2020.jasp provided by the authors.

2) The ****Mode-based estimate**** part of the Rmd code returned an error :

```
> MBEMeta(beta.in, se.in, alpha=0.05, n_boot=1e4)
```

```
Error in density(beta.in, bw = bw, weights = weights) :
```

```
Argument 'x' must be an object of class "weightfunct".
```

However, when I ran the code that I previously modified (see my previous review) on the new data file, I obtained roughly similar results as the authors (a median of 0.016 where the authors found 0.017, and similar CIs)

Discussion

It's unclear to me why the "320 trials" estimate (using G*Power) stayed the same as in version 2 of the manuscript, in spite of the fact that the estimated overall ES changed from .099 (.05-.14) to .08 (.04-.12).

I also tried G*Power and found that for a two-tailed "Proportion: Sign test (binomial test)" with a ESg of 0.08, alpha = 0.05, and power = 0.80, the required sample size is N = 312.

Could the authors be more specific about the type of analysis (parameters) they used in G*Power ?

Competing Interests: No competing interests were disclosed.

Reviewer Expertise: psychophysics, cognitive psychology, cognitive and behavioral neuroscience, psi phenomena

I confirm that I have read this submission and believe that I have an appropriate level of expertise to confirm that it is of an acceptable scientific standard, however I have significant reservations, as outlined above.

Author Response 10 May 2024

Patrizio Tressoldi

Thank you for your continuous accurate and constructive reviews we now have acknowledged in this new version.

Here are replies to all your comments and suggestions:

Introduction

"Type 1 (Precognition)" should be replaced by "Type 1 (Precognition)" ;
and "Type 2 (Clairvoyance)" should be replaced by "Type 2 (Clairvoyance)"

Methods

About the formula, in "Effect size measures" section :

$\sqrt{(\text{hit rate \%} * (1 - \text{hit rate \%}) / \text{trials} * \text{chance proportion} * (1 - \text{chance proportion}))}$
should be replaced by

$\sqrt{(\text{hit rate proportion} * (1 - \text{hit rate proportion}) / \text{trials} * \text{chance proportion} * (1 - \text{chance proportion}))}$.

or by

$\sqrt{(\text{hit rate} * (1 - \text{hit rate}) / \text{trials} * \text{chance proportion} * (1 - \text{chance proportion}))}$.

Reply: corrected as suggested

Results

About the "Publication Bias tests" section of "Exploratory analyses" : "at least 20 fold more likely" should be replaced by "at least 4 fold more likely"

Reply: corrected as suggested

As illustrated in a separate *Jupyter Notebook* (R code and output) file (available through this link: [PDF file2](#)), I explain why the authors show re-run this analysis, possibly together with a complementary fail-safe analysis.

This would allow authors to conclude that the findings suggest that although the meta-analysis is not immune to publication bias (given that a moderate level of bias favoring significant results in the published literature may account for the observed effect size), the meta-analysis results appear relatively robust against the potential impact of this bias as indicated by the high fail-safe N value.

Reply: we added the suggested text in the Discussion section. We didn't add the Rosenthal fail-safe test given its limitations (Jin, Z. C., Zhou, X. H., & He, J. (2015). Statistical methods for dealing with publication bias in meta-analysis. *Statistics in Medicine*, 34(2), 343-360.)

Other Rmd problems :

1) Although I could not test RoBMA in R (due to a problem with jags), the results of this analysis are in the GZMADatabase1974_2020.jasp provided by the authors.

2) The ****Mode-based estimate**** part of the Rmd code returned an error :

```
> MBEMeta(beta.in, se.in, alpha=0.05,n_boot=1e4)
```

```
Error in density(beta.in, bw = bw, weights = weights) :
```

```
Argument 'x' must be an object of class "weightfunct".
```

However, when I ran the code that I previously modified (see my previous review) on the new data file, I obtained roughly similar results as the authors (a median of 0.016 where the authors found 0.017, and similar CIs)

Reply: happy you confirmed our results

Discussion

It's unclear to me why the "320 trials" estimate (using G*Power) stayed the same as in version 2 of the manuscript, in spite of the fact that the estimated overall ES changed from .099 (.05-.14) to .08 (.04 -.12).

I also tried G*Power and found that for a two-tailed "Proportion: Sign test (binomial test)" with a ESg of 0.08, alpha = 0.05, and power = 0.80, the required sample size is N = 312.

Could the authors be more specific about the type of analysis (parameters) they used in G*Power ?

Reply: we estimated a one-tailed alpha = 0.05, given that only positive outcomes are related to anomalous perception

Competing Interests: I'm one of the paper authors

Reviewer Report 09 March 2024

<https://doi.org/10.5256/f1000research.162122.r245035>

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Dean Radin 

1

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This is acceptable. I see some typos (e.g., clairvoyance and precognition are misspelled), but otherwise it is okay.

Competing Interests: No competing interests were disclosed.

Reviewer Expertise: Experimental psychology, parapsychology, psychophysiology, physics, and statistics.

I confirm that I have read this submission and believe that I have an appropriate level of expertise to confirm that it is of an acceptable scientific standard.

Reviewer Report 21 February 2024

<https://doi.org/10.5256/f1000research.162122.r245036>

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Pavo Orepic 

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The authors did not address my concerns. I would suggest not approving this manuscript. My status is "Not Approved".

Competing Interests: No competing interests were disclosed.

Reviewer Expertise: neuroscience, hallucination engineering, speech, inner speech, self-consciousness, self-voice perception, EEG, intracranial EEG

I confirm that I have read this submission and believe that I have an appropriate level of expertise to state that I do not consider it to be of an acceptable scientific standard, for reasons outlined above.

Version 2

Reviewer Report 25 January 2024

<https://doi.org/10.5256/f1000research.143962.r224487>

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Pavo Orepic 

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This is a meta-analysis of the studies investigating the Ganzfeld effect. It builds up on the previous meta-analyses by including the most recent studies and applying different, more detailed statistical approaches. I have several major concerns related to this work.

My biggest concern is that I found the title (as well as the story) misleading. The paper is actually about telepathic communication through the Ganzfeld effect, and not the Ganzfeld effect itself. This research question is not clearly stated. The ganzfeld effect is a much wider phenomenon that is based on Gestalt psychology, which was not even introduced. Moreover, other Ganzfeld effect studies that investigated the phenomenon itself and its neural correlates (e.g. works of Jiri Wackermann or Timo Schmidt) besides its alleged use for telepathic communication were not mentioned. Generally, the manuscript is quite difficult to follow, needs more thorough proofreading (as detailed by the other reviewer), and important information about the reviewed studies (specified below) is missing. I would suggest rewriting the manuscript such that its real purpose becomes clearer.

Another major issue that makes the results unconvincing is the inclusion of studies that are not properly peer-reviewed. Using "peer review level" as a correlate or a covariate in analyses and not observing a difference between "proper" and "improper" actually raises more doubts about the quality of the "proper" studies.

The Ganzfeld procedure itself (especially limited to this context of communication) is not clearly introduced. What is the "target", the "decoys", the "judging process", etc? What are the instructions for the participants? How is the hit rate estimated and computed? The difference between the three different conditions is also not clear. How can a target be chosen after the judgment phase? A schematic would be useful. Some of the covered studies should be introduced in detail to give the reader the impression of what is actually investigated here. I only understood what the paper was about after I read another paper on the topic.

A table summarizing the reviewed studies and their main approaches and findings is missing. Since the goal of the article also seems to be to contrast this meta-analysis with the previous ones, this table should also indicate which studies were indicated in which other meta-analyses.

I find it also strange to use bulletpoints in such a way throughout the manuscript, especially in the Introduction. It breaks the flow of the narrative, which is largely missing in the first place. The

paragraphs are often not connected – the manuscript reads more as a list than as a story.

What is “autoganzfeld”? The difference between the introduced meta-analyses is not very clear. Did they cover different studies? What were their inclusion criteria and metrics? How is this meta-analysis advancing the previous ones – is it simply margining the data?

I am also not convinced that the problem of publication bias is solved. I would like to see the Test for Excess Success (TES) which examines whether the reported success rate of a set of experiments agrees with the estimated magnitude of the effect. For a relevant discussion, see Chapter 10 from the book of Herzog et al., *Understanding Statistics and Experimental Design, Learning Materials in Biosciences*, https://doi.org/10.1007/978-3-030-03499-3_10). They describe an interesting scenario in which out of 10 papers investigating precognition, 9 found a significant result (90%), whereas TES indicated that there is a 6% chance of having the same degree of success as the original report.

Even if the evidence for the telepathic phenomena proves to be true – there should be more discussion (as well as more introduction) on the supposed/proposed mechanisms behind it. What do different authors propose to explain the effect? How is this addressed in different studies? How in different meta-analyses? Are differences in supposed mechanisms taken into account? For a person completely outside of the field, such a “gentle introduction” is necessary to at least try to consider the possibility of such effects. I would suggest moving (and extending) the one paragraph talking about this from the Discussion to the Introduction, and in the Discussion focus on how the differences are reflected in different studies and meta-analyses.

Generally, even though I find the topic very interesting, I do not find the work convincing. I am not familiar with the literature nor the methods in parapsychology, but for the field to advance and be more accepted by the scientific community, it would be useful if the research practices and writing style were done such that it is understandable to a person outside the field.

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Is the work clearly and accurately presented and does it cite the current literature?

No

Is the study design appropriate and is the work technically sound?

No

Are sufficient details of methods and analysis provided to allow replication by others?

Partly

If applicable, is the statistical analysis and its interpretation appropriate?

Partly

Are all the source data underlying the results available to ensure full reproducibility?

Partly

Are the conclusions drawn adequately supported by the results?

No

Competing Interests: No competing interests were disclosed.

Reviewer Expertise: neuroscience, hallucination engineering, speech, inner speech, self-consciousness, self-voice perception, EEG, intracranial EEG

I confirm that I have read this submission and believe that I have an appropriate level of expertise to state that I do not consider it to be of an acceptable scientific standard, for reasons outlined above.

Author Response 31 Jan 2024

Patrizio Tressoldi

My biggest concern is that I found the title (as well as the story) misleading. The paper is actually about telepathic communication through the Ganzfeld effect, and not the Ganzfeld effect itself. This research question is not clearly stated.

Reply: The title indicates anomalous perception as tested using ganzfeld principles. In the revised introduction the reader will see that "telepathic communication" is only one parapsychological modality tested (i.e., Type 3), as precognition (Type 1) and clairvoyance (Type 2) are not specifically identified and listed. These are nominal terms, so neutral terms (i.e., 'type') is preferred throughout the paper.

The ganzfeld effect is a much wider phenomenon that is based on Gestalt psychology, which was not even introduced. Moreover, other Ganzfeld effect studies that investigated the phenomenon itself and its neural correlates (e.g. works of Jiri Wackermann or Timo Schmidt) besides its alleged use for telepathic communication were not mentioned. Generally, the manuscript is quite difficult to follow, needs more thorough proofreading (as detailed by the other reviewer), and important information about the reviewed studies (specified below) is missing. I would suggest rewriting the manuscript such that its real purpose becomes clearer.

Reply: acknowledged, but we believe we have been very clear about the specific use of ganzfeld in parapsychology. We feel the review, coupled with the information provided in the Introduction, makes it quite clear what the history of Ganzfeld psi testing was all about. Other than that, it is not clear to us what additional information Reviewer #3 would require.

Another major issue that makes the results unconvincing is the inclusion of studies that are not properly peer-reviewed. Using "peer review level" as a correlate or a covariate in analyses and not observing a difference between "proper" and "improper" actually raises more doubts about the quality of the "proper" studies.

Reply: It is not a case of "Improper" studies being assessed. Clearly, studies that might be regarded as having been reviewed to an 'adequate' or 'sufficient' standard (level 1)

only, could easily have passed full review (level 2) since we showed that the level of peer-review “did not yield differences in the effect sizes”. That is, we should expect that a sharper peer-review would translate as significantly lower effect sizes under the assumption that psi effects are the result of design flaws.

The Ganzfeld procedure itself (especially limited to this context of communication) is not clearly introduced. What is the “target”, the “decoys”, the “judging process”, etc? What are the instructions for the participants? How is the hit rate estimated and computed? The difference between the three different conditions is also not clear. How can a target be chosen after the judgment phase? A schematic would be useful. Some of the covered studies should be introduced in detail to give the reader the impression of what is actually investigated here. I only understood what the paper was about after I read another paper on the topic.

A table summarizing the reviewed studies and their main approaches and findings is missing. Since the goal of the article also seems to be to contrast this meta-analysis with the previous ones, this table should also indicate which studies were indicated in which other meta-analyses.

I find it also strange to use bulletpoints in such a way throughout the manuscript, especially in the Introduction. It breaks the flow of the narrative, which is largely missing in the first place. The paragraphs are often not connected – the manuscript reads more as a list than as a story.

Reply: Some of these problems are now fixed, and we find the use of bulletpoints is expedient and convenient for the readers (many meta-analyses employ this formatting).

What is “autoganzfeld”? The difference between the introduced meta-analyses is not very clear. Did they cover different studies? What were their inclusion criteria and metrics? How is this meta-analysis advancing the previous ones – is it simply margining the data?

Reply: “autoganzfeld” was defined in the Introduction under the heading, Review of the Ganzfeld Meta-Analysis. In that review, the sequencing of the studies across the years indicates gradual improvements in study designs.

I am also not convinced that the problem of publication bias is solved. I would like to see the Test for Excess Success (TES) which examines whether the reported success rate of a set of experiments agrees with the estimated magnitude of the effect. For a relevant discussion, see Chapter 10 from the book of Herzog et al., Understanding Statistics and Experimental Design, Learning Materials in Biosciences, https://doi.org/10.1007/978-3-030-03499-3_10. They describe an interesting scenario in which out of 10 papers investigating precognition, 9 found a significant result (90%), whereas TES indicated that there is a 6% chance of having the same degree of success as the original report.

Reply: We could have conducted any number of tests on publication bias, and still not satisfied many readers/critics; we have conducted various such tests in our past meta-

analytic papers and found similar results (e.g., see Storm, Tressoldi, & Di Risio, 2010). Furthermore, if you check the z values of all experiments included in the database you will notice that only 30 (26.5%) are statistically significant, confirming that there is not an excess of success.

Even if the evidence for the telepathic phenomena proves to be true – there should be more discussion (as well as more introduction) on the supposed/proposed mechanisms behind it. What do different authors propose to explain the effect? How is this addressed in different studies? How in different meta-analyses? Are differences in supposed mechanisms taken into account? For a person completely outside of the field, such a “gentle introduction” is necessary to at least try to consider the possibility of such effects. I would suggest moving (and extending) the one paragraph talking about this from the Discussion to the Introduction, and in the Discussion focus on how the differences are reflected in different studies and meta-analyses.

Reply: we agree possible mechanisms of the psi process could be discussed but it is likely there is more than one process besides telepathy to be discussed (i.e., precognition and clairvoyance as well), and to introduce various theories would go beyond the parameters of our paper which is largely statistical.

Generally, even though I find the topic very interesting, I do not find the work convincing. I am not familiar with the literature nor the methods in parapsychology, but for the field to advance and be more accepted by the scientific community, it would be useful if the research practices and writing style were done such that it is understandable to a person outside the field.

Reply: we are very sorry that this paper is not suitable for researchers not familiar with the literature, and the methods in parapsychology, and we add the characteristics of a Stage 2 meta-analysis Registered Report. Unfortunately, we cannot satisfy your request, unless we change completely the type of paper (see previous comment).

Competing Interests: No competing interests were disclosed.

Reviewer Report 24 January 2024

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My report is provided in a separate file available through this link: [PDF file](#).

Is the work clearly and accurately presented and does it cite the current literature?

Yes

Is the study design appropriate and is the work technically sound?

Partly

Are sufficient details of methods and analysis provided to allow replication by others?

Partly

If applicable, is the statistical analysis and its interpretation appropriate?

Partly

Are all the source data underlying the results available to ensure full reproducibility?

Partly

Are the conclusions drawn adequately supported by the results?

Partly

Competing Interests: No competing interests were disclosed.

Reviewer Expertise: psychophysics, cognitive psychology, cognitive and behavioral neuroscience, psi phenomena

I confirm that I have read this submission and believe that I have an appropriate level of expertise to confirm that it is of an acceptable scientific standard, however I have significant reservations, as outlined above.

Author Response 31 Jan 2024

Patrizio Tressoldi

Thank you for your accurate review in particular the reproducibility of our results using our database and code syntax.

Here follow our replies to all your comments.

Main comments:

1-At the beginning of the "Effect size measures" section, the authors write "As a standardized measure of effect size, we used [...] Binomial Z score/ $\sqrt{\text{number of trials}}$ using the number of trials, the hits score and the chance probability as raw scores." However, in the xlsx data file provided by the authors, the values in the ES column correspond to Z/\sqrt{N} , with N as the number of participants rather than the

number of trials in the study.

Reply: even if using trials or participants as a unit of analysis changes only some decimals of all results for this database, in this revision we used trials as this is the standard choice in all meta-analyses related to the topic investigated.

2-I also share the concerns expressed earlier by S Schmidt and J Utts about using webpages to compute exact binomial Z score values, either directly (with <http://vassarstats.net/binomialX.html>) or indirectly (with <http://www.fourmilab.ch/rpkp/experiments/analysis/zCalc.html> from the p value provided by [binomialX.html](http://vassarstats.net/binomialX.html)).

Reply: what is important is the correctness of the formulas and not where they are implemented. In our case, the choice of webpages could allow independent controls without using other software. In our case, the formulas are available on the websites. Thank you for pointing us the error related to the Z score of the third experiment.

3- Publication Bias tests

....On the one hand, I find this statement incomplete and misleading because it ignores the confidence

interval around the meta-analytic pooled point estimate.

Reply: we added "These results suggest that for publication bias to attenuate (to "explain away") the observed point effect lower CI interval to the null, affirmative results would need to be at least 10 fold more likely to be published than nonaffirmative results", in the revised version as suggested.

4- Could the authors give a reference or spell out the practical (or theoretical) reasons for this

statement: "the .01 level, considered the smallest effect size of interest"?

Reply: This is an arbitrary cutoff close to the null (zero) level for this line of investigation. This level can change in other research fields.

Further comments

1-Table 1 reports several descriptive values. The mean (SD) values in the Hit rate column are for 4 free-choice designs. However, the corresponding note at the bottom of Table "this value is purely

descriptive because not all studies are 4 free-choice designs" is ambiguous;

Reply: we changed the proportion of hits with the proportion of hits above the chance level a measure that takes into account the differences in the chance level in the different experiments.

2-The manuscript mentions several supplementary figures that are not currently available at the Figshare link the authors provided

Reply: Sorry for that. Now we have uploaded the Forest, the cumulative and the Sequential Bayes Factor in the Stage 2 Supplementary Figures.doc file in the figshare repository.

3- Unfortunately, the authors do not provide any funnel plot figure. I recommend the authors to use the

metaviz R library to generate a funnel plot with power regions

Reply: in the text of the revised version we invited the readers to read your review and in particular your power funnel plot.

4- Rationale of the statistical analysis and Discussion of the results
Surprisingly, they never clearly wrote down (or maybe I missed something?) that the theoretical GZ effect size would be " $Z / \sqrt{N} = 0.1$ ".

Reply: the use of this prior in the Bayesian meta-analysis is an arbitrary choice based on the overall frequentist meta-analysis, and the previous meta-analyses e.g. Storm & Tressoldi, 2010.

Competing Interests: No competing interests were disclosed.

Reviewer Report 02 November 2023

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This is a follow-up to a previous meta-analysis of the ganzfeld condition experimental database. It uses the latest meta-analytic techniques and adds further evidence to the original conclusion.

My primary comment about this document is that it would be difficult for those who are not already familiar with the relevant literature to fully understand what this experiment is about. For example, in the Introduction, participants are referred to as "they," but given that "they" is sometimes used a neutral gender term, we cannot tell if this entails one person or two, who is describing their impressions aloud, how that information is recorded, who judges that information against the target, how many targets and decoys are involved (we read four at one point, and binary at another), and so on.

Because of such confusions, I think it would be much clearer if the various types of ganzfeld condition experiments that are being considered were defined upfront, and then state how many

participants are involved in each kind of study, and exactly what each of their roles are. As currently written, some of this information is provided later in the Introduction, but by the time the reader gets to that description they will already be confused.

Other phases that might be clarified:

> Once participants are sensorially isolated from external visual and auditory stimulation, they are in a favorable condition

I'd prefer a more cautious "*thought to be* a favorable condition"

> in the mainstream journal *Psychological Bulletin*.

The word *mainstream* is unnecessary.

> Moreover the autoganzfeld procedure avoids

Maintain consistency in tenses. Thus, "avoided" and not avoids.

> They overall reported hit rate

You mean "*The* overall reported hit rate"

> average standardized effect size

Meaning? There are many definitions for effect size.

> with the more advanced statistical procedures that should overcome the limitations of the previous meta-analyses

Such as? List these limitations.

> have been approved in the Stage 1

The Stage 1 what? That term only makes sense if one is already familiar with how this journal works.

> the editorial policy of accepting paper

You mean "papers". Note: There are many other examples like this that a proofreader would catch. I suggest that the authors do a very careful review of the text for these kinds of grammatical mistakes. I will mention some (not all) of them below.

> Studies must use human participants only (not animals);

Are there ganzfeld studies with animals?

> Number of participants must be in excess of two to avoid the inherent problems that are typical

in case studies;

This is confusing because we don't know if you mean the number of participants per session, or the number of sessions.

> Target selection must be randomized by using a Random Number Generator (RNG) in a computer or similar electronic device

An RNG "in" a computer can mean a pseudorandom algorithm or a true hardware-based RNG. Clarify.

> Randomization procedures must not be manipulated by the experimenter or participant;

What does "manipulated" mean in this context? If I select from a table of random numbers, that is a manual process, and thus could be interpreted as manipulated.

> *Researches In Parapsychology*

You mean "conference proceedings such as those published in a book series called **Research In Parapsychology**"

> As standardized measure of effect size, we used that one

we used **the** one

> Binomial Z score/ $\sqrt{\text{number of trials}}$ using the number of trials, the hits score and the chance probability as raw scores

A comma is necessary to clarify this sentence: Binomial Z score/ $\sqrt{\text{number of trials}}$, using the number of trials, the hits score and the chance probability as raw scores

> were transformed in the Hedge's *g* effect sizes,

were transformed into **the** Hedge's *g* effect sizes,

> a Bayesian random-effect model for testing its robustness

What does "it" refer to?

> See syntax details provided as extended data

What does **extended data** mean?

In Table 2 we find a cell with the number 1909. It isn't clear what that refers to. Is that a Bayes Factor?

> We compared the influence of the following tree moderators:

What is a tree moderator? Do you mean **three** moderators?

> the “file drawer” is pretty empty,

Can you be more objective? Pretty can mean all sorts of things.

>The results of the cumulative meta-analysis is represented

Results **are**, not is.

> Thus, it appears to be stable

Should probably emphasize *cumulatively* stable, or stable on average.

Is the work clearly and accurately presented and does it cite the current literature?

Partly

Is the study design appropriate and is the work technically sound?

Yes

Are sufficient details of methods and analysis provided to allow replication by others?

Partly

If applicable, is the statistical analysis and its interpretation appropriate?

Yes

Are all the source data underlying the results available to ensure full reproducibility?

Yes

Are the conclusions drawn adequately supported by the results?

Yes

Competing Interests: No competing interests were disclosed.

Reviewer Expertise: Experimental psychology, parapsychology, psychophysiology, physics, and statistics.

I confirm that I have read this submission and believe that I have an appropriate level of expertise to confirm that it is of an acceptable scientific standard, however I have significant reservations, as outlined above.

Author Response 31 Jan 2024

Patrizio Tressoldi

Sorry for the late response to your review, but we preferred to wait for further reviews' before revising the paper.

Here are the replies to your comments.

>My primary comment about this document is that it would be difficult for those who are not already familiar with the relevant literature to fully understand what this experiment is about. For example, in the Introduction, participants are referred to as "they," but given that "they" is sometimes used a neutral gender term, we cannot tell if this entails one person or two, who is describing their impressions aloud, how that information is recorded, who judges that information against the target, how many targets and decoys are involved (we read four at one point, and binary at another), and so on.

Because of such confusion, I think it would be much clearer if the various types of ganzfeld condition experiments that are being considered were defined upfront, and then state how many participants are involved in each kind of study, and exactly what each of their roles are. As currently written, some of this information is provided later in the Introduction, but by the time the reader gets to that description they will already be confused.

Reply: See restructured and corrected text in the Introduction, margin comments, and additional text.

Other phrases that might be clarified:

Reply: We revised the introduction following these suggestions

What does extended data mean?

Reply: This is an editorial requirement replacing the Supplementary Material.

In Table 2 we find a cell with the number 1909. It isn't clear what that refers to. Is that a Bayes Factor?

Reply: Correct, even If after a re-analysis this value is now 85 in the range of "very strong evidence" .

> the "file drawer" is pretty empty,

Can you be more objective? Pretty can mean all sorts of things.

Reply: we deleted the term "pretty".

Competing Interests: No competing interests were disclosed.

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