

The Impact of Financial Resources on Cognitive Performance: A Systematic Review and Meta-Analysis

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Abstract

Previous research has proposed that financial resources influence cognitive performance, though subsequent studies have questioned the magnitude and even the existence of this effect. To clarify these discrepancies, we conducted a systematic review and Bayesian meta-analysis aimed at identifying when and to what extent this effect appears. After screening 38,366 results, we identified 14 relevant studies with 26 effect sizes. We found that evidence for the effect's existence remains inconclusive ($BF_{10} = 1.35$) but suggests that, if present, it is relatively small ($g = 0.06$ [0.00, 0.18], $\tau = 0.14$ [0.09, 0.22]). These findings are consistent across alternative analytical specifications. We detected moderate evidence that the effect is likely to arise in studies using unconditional cash transfers ($BF_{10} = 8.08$) and unlikely in payday variation designs ($BF_{10} = 0.18$). However, we found no evidence to support or oppose the hypothesis that the integration of financial windfalls into participants' financial routines influences the effect. Additionally, our results provide moderate evidence that financial resources do not impact memory performance ($BF_{10} = 0.24$). These results vary across alternative analytical specifications. In summary, while current evidence is insufficient for definitive conclusions, our findings highlight key patterns and limitations in the literature.

Keywords. poverty; cognitive performance; financial scarcity, unconditional cash transfer, Bayesian meta-analysis; financial resources

Evidence suggests that living in financial scarcity creates psychological traps for the poor by triggering counterproductive behavioural patterns that reinforce their poverty (Barr 2012; Blank and Barr 2009; Kim, Sorhaindo, and Garman 2006; Neal et al. 2001; Shah, Mullainathan, and Shafir 2012). Cognitive functioning is discussed as one major pathway through which such poverty traps might be expressed (Mullainathan and Shafir 2013). According to this view, living in poverty is associated with several factors (e.g., high anxiety and stress (REF), malnutrition (REF), chronic disease limitations on activity (Braverman2010), sleep environments) that negatively impact people's cognitive functioning (de Bruijn and Antonides 2022; Dean, Schilbach, and Schofield 2017). Then, decreased cognitive performance can undermine the chances to escape poverty through various channels such as schooling (Cawley, Heckman, and Vytlačil 2001; Duquenois 2022) or job performance (Bishop 1992; Kaur et al. 2021). The present systematic review and meta-analysis examines how financial resources influence cognitive performance.

The literature supports both short- and long-term correlational relationships between financial resources and cognitive functioning. On average, low-income children and adults show impaired cognitive performance compared to their high-income counterparts (e.g., Feinstein 2003; Hurley 1969; Oasis and Remy 2014; Smith, Brooks-Gunn, and Klebanov 1997). Additionally, studies indicate that exposure to poverty during childhood or adulthood is associated with worse cognitive abilities later in life (e.g., Al Hazzouri et al. 2017; Barnett 1998; Kaplan et al. 2001; Karlamangla et al. 2009; Lynch, Kaplan, and Shema 1997; Schoon et al. 2012). Almeida et al. (REF) conducted a meta-analysis summarising the correlational and some experimental and quasi-experimental findings on the relationship between scarcity and cognitive functioning, incorporating 256 effects from 29 datasets. Their results suggested that the effect of scarcity on cognitive performance falls between $-0.58\ g$ and $-0.29\ g$ with 95% confidence, with education accounting for about 60% of this effect. However, correlational studies do not provide causal evidence of the relationship or the direction of the effect. For example, while limited access to quality education can explain lower cognitive scores among the poor, individuals with lower cognitive abilities may also engage in less or lower-quality education, further deepening their poverty.

To explore the causal relationship between financial resources and cognitive performance, researchers have turned to experimental and quasi-experimental designs. Mani and colleagues (2013) provided evidence for this link in a study of Indonesian sugarcane farmers. They observed

that farmers scored lower on cognitive control and Raven's Matrices tests before the harvest, when money was scarce, compared to post-harvest, when resources were more abundant. The effect sizes were surprisingly large, comparable to losing one night of sleep (see Wicherts and Scholten (REF) for a critique of their methods). Similar studies in Zambia (Fehr) and Uganda (Pande) replicated this effect, with effect sizes varying from small to huge, while Lichand and Mani (2020) failed to replicate the finding. In the U.S., Carvalho et al. (2016) examined whether receiving monthly wages improves cognitive performance by measuring it before and after payday but found no evidence supporting the effect (see Mani et al. (2020) for the critique of their studies). In contrast, Kaur et al. (REF) conducted a field study in India and observed that workers who received nearly a month's worth of one-time wages in advance made fewer attentional errors than those still awaiting payment. Unconditional cash transfer studies failed to consistently reproduce the effect in Liberia (Szaszi), in the US (Jaroszewicz), and in Canada (Dwyer). When the effects were found, they were small (X). Other research designs were used successfully to demonstrate the effect: Ayyagari and Frisvold (REF) exploited a systematic error in the US social security payments, while Ong and colleagues (REF) examined the effect of paying off debts for people in poverty.

The scientific evidence is also limited on which cognitive functions are affected by the lack of financial resources, and when. Correlates of different degrees of absolute scarcity (i.e. "having less than an objectively defined, absolute minimum" (REF)), like malnutrition (REF), sleep deprivation (REF), and pain (REF) are known for affecting ... However, the picture is less clear when looking at cognitive functions that are affected through the effects of subjective scarcity (i.e. the feeling of having less than enough to get by, ref). According to *scarcity theory*, a broad framework describing the mental consequences of subjective scarcity (Mani, Shafir book, (Zhao & Tamm, 2018),(Hamilton et al., 2019)), two major phenomena affect cognitive performance (de Bruijn and Antonides 2022). First, it is hypothesised that the cognitive bandwidth of individuals experiencing scarcity is reduced, thereby having less capacity for mentally draining tasks (REF). Second, the attentional tunneling effect is thought to enhance a person's focus on cues related to scarce resources, but it limits their ability to detect other types of cues (REF). The effect has been tested with tools measuring various cognitive functions (e.g., fluid intelligence, working memory, cognitive control), but yielded contradictory results (Haushofer and Salicath 2023). Szaszi and colleagues (REF) explored whether different facets of cognitive functioning are impacted

distinctly in a study on unconditional cash transfers, and found evidence suggesting that financial resources affect specific subcomponents of executive functions in varying ways.

In this paper, we investigate four explanations that have been proposed to account for the contradictory findings regarding the impact of financial resources on cognitive function. First, as detailed above, the effect might not appear on all cognitive functions (Szasz REF). Second, certain manipulations used in the literature (e.g. REF) might not produce large enough changes in financial resources to stand out from everyday financial fluctuations (REF). Third, different empirical strategies might also lead to different results due to distinct potential confounds. For instance, the observed positive effects of harvests, might only be the product of confounding factors such as stress and sleep deprivation induced by the workload of the harvest preparations, not the amount of available financial resources (REF). Fourth, the impact of changes in financial resources might be negligible when the received money is part of a predictable financial routine, such as a monthly pay cheque or seasonal harvest (REF). Individuals may mentally account for the money in advance, leaving their perceptions of scarcity unchanged upon receiving the money.

To resolve the discrepancies in the literature, in the present systematic review and meta-analysis our goal was to collect and synthesise the available causal evidence on how financial resources affect cognitive performance. We improve upon prior research in three key areas. First, we focus exclusively on experimental and quasi-experimental studies, enabling causal claims, including randomised controlled trials, which allow us to overcome the potential confounds of pre-post designs, such as stress unrelated to financial scarcity and learning effects (REF). Second, we investigate four potential moderators of the effect—the research design employed, the type of cognitive function measured, the magnitude of the financial sum used in the manipulation, and whether the manipulation aligned with participants’ financial routines —deepening our understanding of the generalisability of the effect. Third, we utilise Bayesian meta-analytic methods, which enable us to quantify evidence against the existence of the effect.

Methods

In our reporting, we largely adhered to the reporting guidelines of the Preferred Reporting Items for Systematic Review and Meta-Analysis Protocols (PRISMA-P, Moher et al., 2015). Deviations from the guidelines are detailed in Table SX and Table SX.

Information Sources

We collected studies from the following sources. First, based on Google Scholar results, we applied a pilot screening on the studies citing Mani et al. (2013), in September 2019. This pilot screening was used as an input to refine the search queries used in the systematic search phase. Second, we systematically searched seven scientific databases in two rounds, once in 2020 and once in 2023. We searched one domain-general scientific database, Web of Science, which includes papers from a wide range of fields including life sciences, social sciences, physical sciences and health sciences, and two domain-specific databases, Econlit and PsycINFO for economics and psychology, respectively. Note that we lost access to Econlit between the two screening rounds, so it was not used during the second round. Additionally, we screened the grey literature to identify (yet) non-published results by searching in PsyArXiv, OSF, Socialscienceregistry.org, and NBER. Due to technical errors, we only managed to screen 1000 papers from NBER, and the second round of screening did not include NBER as an information source, see the Table S1 for details. The keywords we used to identify the papers of interest are shared in Table 1. We shared the exact date and syntax of the searches for each of the databases in Table S1. Third, we reached out to experts for recommendations. In October 2019, we contacted 168 experts to suggest relevant papers. We contacted experts who published peer-reviewed academic papers as first authors on poverty-related topics, and researchers from the Abdul Latif Jameel Poverty Action Lab (J-PAL) network. We also sent a letter to the mailing list run by the Society for Judgment and Decision Making with the same inquiry. The templates of the contact emails are available on the OSF page of the project at [LINK].

Table 1

Literature Search Terms

Financial nature	Cognitive functions
poor NOT "poor performance"	cognitive abilit*

poverty	cognitive function*
money scarcity	cognitive performance
	executive function*
	decision-making

Note. To appear in the search results, studies need to mention at least one term from each column (i.e., poverty AND cognitive ability) in their title, abstract or set of keywords.

Eligibility Criteria

We selected studies during the screening procedure based on the following eligibility criteria.

- 1) The methods and results are available in English.
- 2) Primary data is used in the investigation.
- 3) The design is experimental or quasi-experimental.
- 4) The amount of financial resources available to the participant was manipulated before the measurement of the dependent variable.
- 5) The manipulation included real money. We excluded studies where hypothetical resources or gifts were distributed.
- 6) The manipulation of financial resources did not involve providing loans.
- 7) The dependent variable is based on objective performance criteria.
- 8) The dependent variable was measured with an assessment tool aiming to capture at least one of the following broad categories: memory, attention, executive functions (shifting / inhibition, inhibitory control, cognitive control/monitoring), and higher-order cognition.
- 9) The study investigates a healthy adult population.
- 10) Individuals (and not groups of individuals) or trials are the unit of analysis.
- 11) The study was not published as a book, book chapter, review article, retracted publication or editorial content.

Note that the inclusion criteria described above were aimed to minimise the probability of missing relevant studies.

Study Selection

We applied a two-step screening procedure on the identified studies. First, the title and the abstract of each candidate paper were screened by one team member. In the second round of data collection, each abstract was screened by two team members. Second, selected works were examined based on the full text by two team members. In case of a disagreement, a third author was consulted.

Data Extraction

One team member extracted statistical results from the selected studies. If the analysis of interest was not conducted, we extracted summary statistics or accessed the shared data. In case neither was sufficient, we contacted the authors for additional data or results. We shared the summary of this communication in Figure S1.

When multiple effects were tested in a paper, we applied the following principles to select which to include.

- 1) If a paper included multiple studies meeting the inclusion criteria, we added each to the review.
- 2) If a study had multiple post-manipulation measurement sessions, we extracted the effect sizes from the session conducted closest to the manipulation. This approach was chosen because theoretically it is expected that the effect would be strongest in the short term, and our goal is to determine whether evidence confirms the existence of the effect at any time point.
- 3) If the authors reported the results in the forms of both individual cognitive ability measures and a summarised cognitive ability index, we extracted the data for each of the individual measures. This was necessary to differentiate the effects of poverty on different cognitive functions.
- 4) If effect sizes for both accuracy and response times were reported for the same cognitive task, we extracted both metrics. Note, that in the primary analyses we only included accuracy results. See the *Multiverse Analysis* subsection of the *Analytic Strategy* section

for details.

Analytic Strategy

Calculation of the Effect Sizes

We calculated Hedges's g values to quantify the difference in cognitive performance between conditions where financial resources were manipulated and conditions where they were not. A positive Hedges's g indicates better performance in the resource-rich condition. To transform the effect sizes into Hedges's g , we used the R package *esc* (REF). For studies that did not report sufficient information to directly calculate Hedges's g values, we used summary statistics or shared data to derive the effect sizes. In cases where effect sizes were originally reported as regression coefficients, we transformed and recalculated them to obtain effect sizes both with and without control variables.

Calculation of the Meta-analytic Estimates

To obtain the meta-analytic estimates, we applied Bayesian model-averaging using the R package *RoBMA* (REF). The package fits multiple meta-analytic models applying different assumptions in each about the presence of the effect, the heterogeneity, publication bias, and hierarchical structure. Then, the meta-analytic estimates are calculated from the posterior model probabilities. We estimated two-level random-effects models treating effect sizes as a random effect nested within papers to calculate our meta-analytic estimations, because we assumed that beyond the variability coming from the sampling procedure, the treatment effect varied also due to differences in the designs and context of included studies. To test the presence of small study effects, we utilised models adjusting for small study bias with the PET and PEESE methods (REF).

Based on the recommendations of Haaf and Rouder (2021), we selected Normal(0, 0.3) as the prior distribution for the presence of effect size and Inverse-Gamma(1, 0.3) as the prior distribution for the presence of between-study heterogeneity. Furthermore, we specified the prior distribution for models that do not assume a hierarchical structure by using a point-mass (spike) prior centred at 0. We used the default priors of *RoBMA* for the models assuming the absence of the effect, the absence of heterogeneity, the presence of the hierarchical structure, and the small-study effect adjustment of the PET and PEESE methods. To interpret Bayes factors, we followed Jeffreys' (1961) framework with a slight modification (see Figure 1). Specifically, we treated

Bayes factors between $\frac{1}{3}$ and 3 as inconclusive rather than classifying them as anecdotal evidence, in line with the approach of Keyzers et al. (2020).

Figure 1

Interpretation Scale for Bayes Factors

BF ₁₀	Very strong evidence
30	Strong evidence
10	Moderate evidence
3	Inconclusive evidence
1	Inconclusive evidence
$\frac{1}{3}$	Moderate evidence
$\frac{1}{10}$	Strong evidence
$\frac{1}{30}$	Very strong evidence

Subgroup Analysis

We coded various properties of the included effect sizes to examine their influence on the observed effects. We conducted separate analyses for the subgroups within each moderator to explore potential differences.

Cognitive Domain. To investigate the impact of scarcity on different cognitive domains, we conducted separate analyses for tasks targeting high-level cognition, memory or working-memory, or executive functioning (excluding working memory).

Empirical Approach. To examine the influence of research design on the effect, we categorised the studies based on their empirical strategies. The three identified groups were pre-post harvest, around payday, and unconditional cash-transfer designs. Two studies (Ong and Ayyagari) were excluded from this analysis due to their unique designs that did not fit into any category.

Financial Routine. To assess how the effect varies depending on whether the financial change occurs within or outside of one's financial routine, we categorised the manipulations accordingly.

Manipulation Size. To evaluate whether the magnitude of financial resources in the manipulation affected the effect size, we categorised the studies based on whether the total sum was smaller vs. equivalent to or greater than one month's income.

Multiverse Analysis

To explore the sensitivity of our results to different analytical specifications, we conducted a multiverse analysis (Steege et al., 2016). The multiverse analysis involves “performing all analyses across the whole set ... of reasonable scenarios” (Steege et al., 2016, p. 702). We systematically varied five analytical specifications in our study. While the theoretical maximum number of combinations was 48, not all combinations resulted in unique datasets. The main analysis produced 33 distinct analyses. The number of conducted analyses varied across the different subgroup analyses.

Outcome Type. Many of the included cognitive tests assessed both response times and accuracy scores. We selected accuracy scores as the primary measure, because accuracy was analysed in every included cognitive task. To test the robustness of this decision, we repeated the analyses using only response times.

Effect Size Calculation. To evaluate the impact of recalculating Hedges's g values rather than transforming the original regression coefficients, we repeated the analysis using effect sizes derived from transforming the reported coefficients. Note that while we had a Hedge's g value for each statistical result, in some cases the original analysis was not regression. In the alternative analyses, we included those Hedge's g values which did not have a coefficient counterpart. This approach aligns with the spirit of conducting a multiverse analysis by incorporating alternative analyses that are meaningful: even if we had used effect sizes transformed from coefficients in our primary analysis, we would still have included Hedge's g values to account for all identified effects.

Accounting for Structured Data. For two studies with hierarchical data, we calculated Hedges's g at both the smallest unit level and at an aggregated level. We repeated the analyses with both set of effect sizes.

Prior Specifications. Following Haaf and Rouder (2021), we tested the sensitivity of our Bayesian analyses by fitting models with priors that had half the width (narrow priors) and double the width (wide priors) of the primary effect size and standard deviation priors.

Unrealistically Large Effect Size. Pande (2023) reported unrealistically large effect sizes (largest $g = 3.76$), so we excluded the paper from the primary analysis. We included it in a robustness check to assess its impact on the overall results.

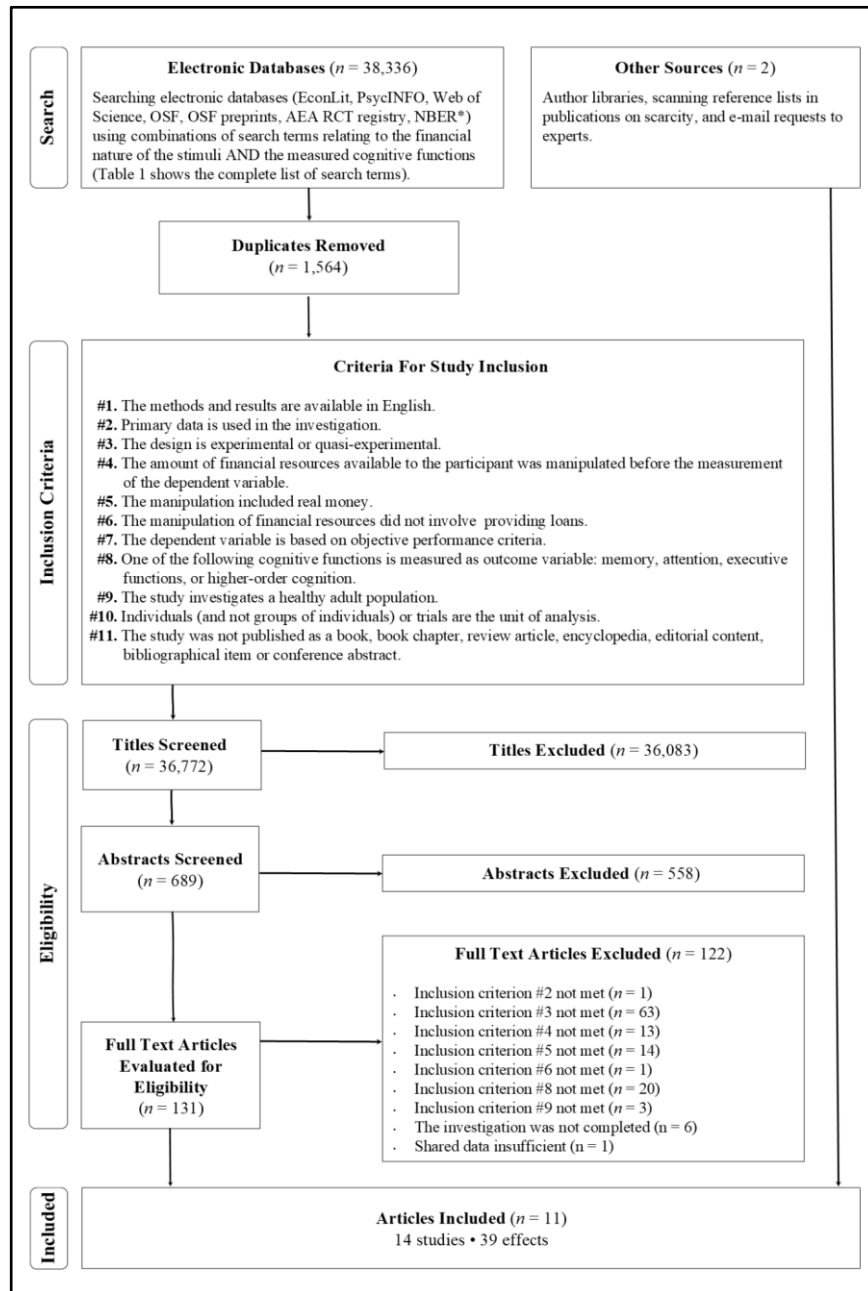
Results

Study selection

A total of 38,336 entries were identified across three academic databases and four grey literature databases. After an initial screening of titles and abstracts, 36,641 articles were excluded, and 1,564 duplicates were removed. This left 131 articles for further screening, of which 121 were excluded based on the predefined criteria. One additional study was omitted because it lacked the necessary results for inclusion, and the authors did not provide additional data upon request. Ultimately, 9 studies met the inclusion criteria, with 2 additional articles sourced elsewhere, resulting in a final dataset of 11 empirical studies (pooled sample size = 30,341) with 39 effects. Figure 2 illustrates this process using a PRISMA flow diagram. Table S2 summarises the main characteristics of the identified studies, while Table S3 lists the properties of analysed effect sizes. Note, that Table S3 includes more than 39 effect sizes due to the different recalculations of the same effects.

Figure 2

PRISMA Flow Diagram



Note. The violated exclusion criterion indicates only the first criterion the coders indicated was violated, an article may fit into multiple categories. On OSF and OSF preprints, we could not save the bibliographic data of the initial search results as such feature was not available on the website. Therefore, we scanned the titles directly on the website and could only check for duplicates after extracting the eligible titles.

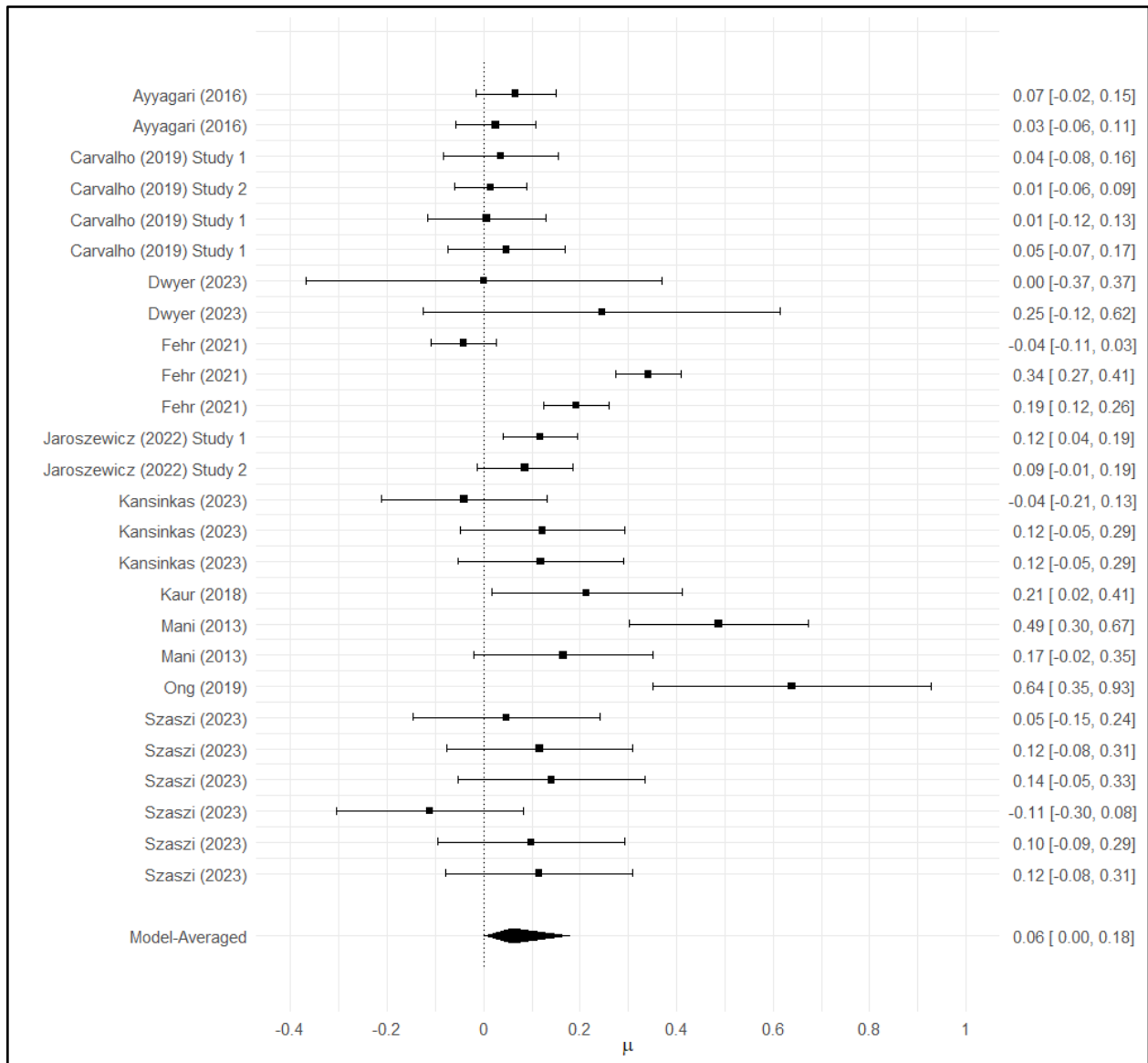
Meta-analytic Effect Size Estimate

The meta-analytic model assessing the impact of financial resources on cognitive performance included 26 effect sizes and showed a small positive effect size. However, the

corresponding Bayes factor showed that the evidence is inconclusive regarding the existence of the effect ($g = 0.06$ [0.00, 0.18], $BF_{10} = 1.35$). Between-study heterogeneity was small ($\tau = 0.14$ [0.09, 0.22], $BF_{10} = 2.16e+13$). These findings are visualised in a forest plot in Figure 3. The analysis indicated a small evidence in favour of the presence of small study effects ($BF_{10} = 3.56$).

Figure 3

Forest Plot of the Primary Effect Size Estimate



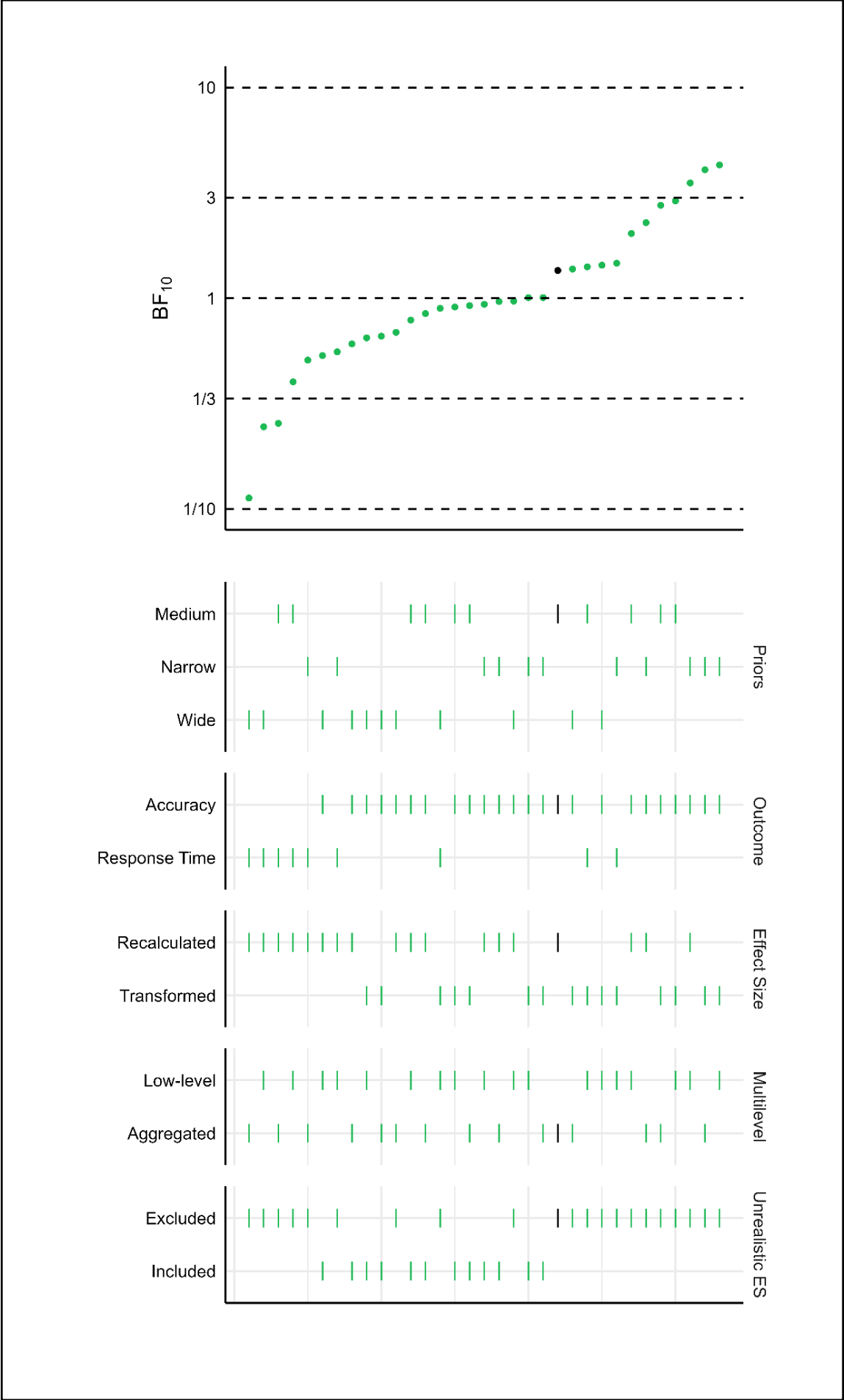
Note. The x-axis represents Hedge's g . The right side of the plot displays the mean estimates along with the corresponding 95% confidence intervals.

Multiverse Analysis. The multiverse analysis resulted in 33 distinct analyses, which confirmed the inconclusiveness of the evidence, as only 6 of the possible analyses resulted in

evidence for or against the effect. The size and evidence for heterogeneity was influenced by alternative analytic specifications, with τ values ranging from 0.09 to 0.89 and BF_{10} values ranging from 0.06 to $2.38e+300$. This variability can be attributed to our method of effect size inclusion, which, in certain specifications, combines effect sizes calculated using different methods (e.g. Hedge's g values derived from recalculation and transformation) or includes implausibly large effect sizes. The results of each analysis are shared in Table S4 and visualised in Figure 4.

Figure 4

Distribution of Bayes Factors Across Analytical Specifications of the Primary Analysis



Note. The dots in the top panel represent Bayes factor estimates. The lines in the bottom panel correspond to the analytical choices made in the individual analyses, aligned vertically with the respective estimates. Black indicates the Bayes factor estimate and analytical choices of the primary analysis. The Priors, Outcome, Effect Size, Multilevel, and Unrealistic ES groups represent the choices for *Prior Specifications*, *Outcome Type*, *Effect Size Calculation*, *Accounting for Structured Data*, and *Unrealistically Large Effect Size*, respectively, as described in the Methods section.

Subgroup Analysis

The detailed results of the moderator analyses are presented in Table 2 and visualised in Figure 5. Overall, three of the four investigations helped to understand the heterogeneity of the main analysis.

Table 2

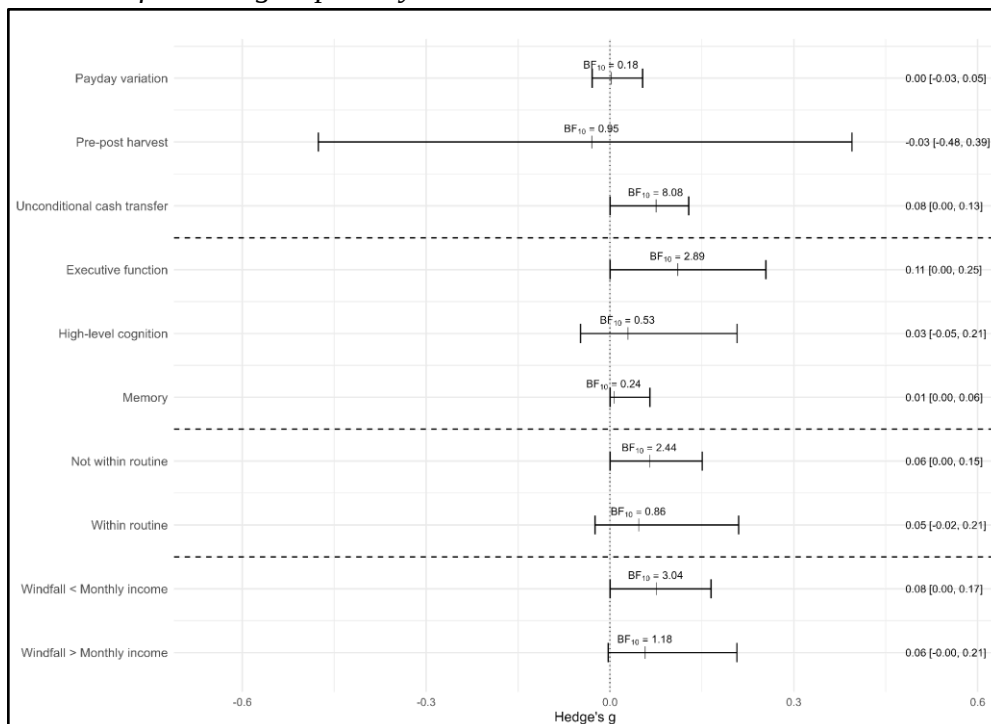
Subgroups Analysis Results

[main_moderator_summary.docx](#)

Note. All rows indicate separate model averages. N indicates the number of effect sizes indicated in each analysis. CI_{lower} and CI_{upper} indicate the lower and upper ends of the 95% confidence interval of the Hedge's g estimate. τ_{lower} and τ_{upper} indicate the lower and upper ends of the 95% confidence interval of the heterogeneity estimate.

Figure 5

Estimates from Subgroup Analyses



Note. The x-axis represents Hedge's g . Dashed lines on the y-axis separate the groups of subgroup analyses, ordered from top to bottom as follows: empirical approach, cognitive domain, financial routine, and manipulation size. The right side of the plot displays the mean estimates with their corresponding 95% confidence intervals, while Bayes factors are shown above the plotted estimates.

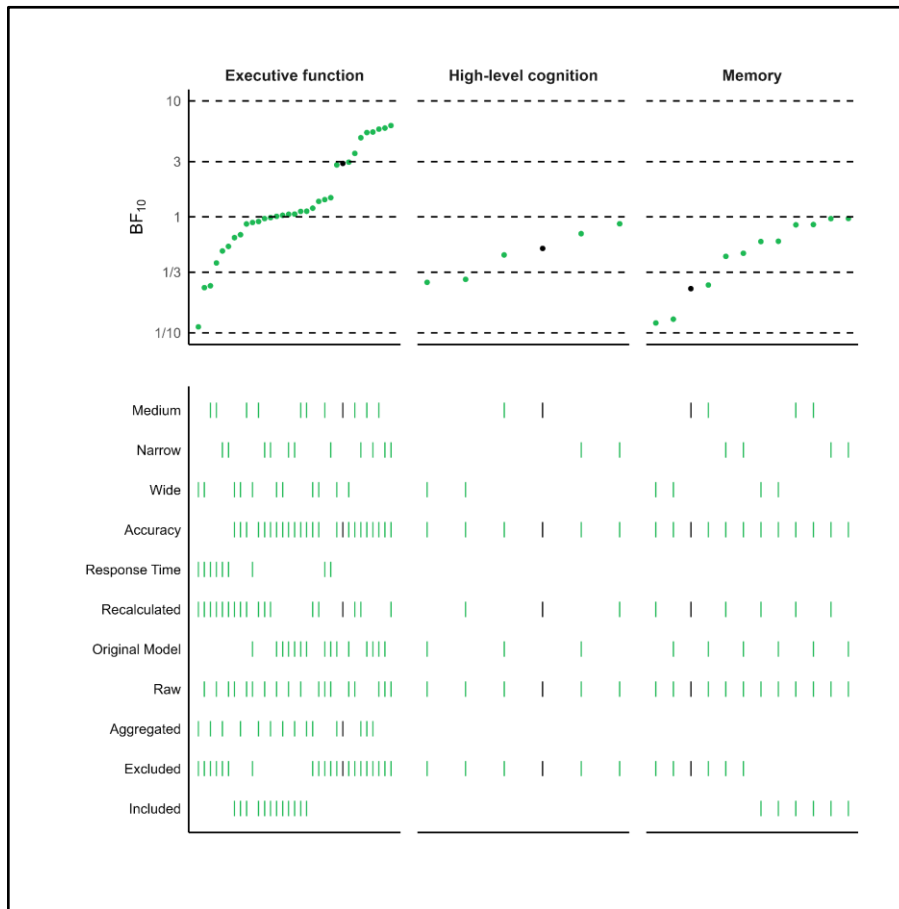
Cognitive Domain

We found moderate evidence against an effect in studies measuring memory performance ($g = 0.01$ [0, 0.06], $BF_{10} = 0.24$). The analyses for executive function ($g = 0.11$ [0, 0.25], $BF_{10} = 2.89$) and high-level cognition performances ($g = 0.03$ [-0.05, 0.21], $BF_{10} = 0.53$) were inconclusive. Regarding heterogeneity, the data supported its absence in the memory subcategory ($\tau = 0.00$ [0, 0.07], $BF_{10} = 0.04$), while very strong evidence for heterogeneity was observed in the executive function ($\tau = 0.15$ [0.08, 0.28], $BF_{10} = 5,708,554.01$) and high-level cognition categories ($\tau = 0.19$ [0.08, 0.37], $BF_{10} = 126.37$).

Multiverse Analysis. The multiverse analysis supported the conclusions of the primary analysis, with all Bayes factors remaining below one. However, only four out of twelve models provided evidence against the effect. The inclusion of effect sizes from the study by Pande (2023) increased Bayes factors beyond the $\frac{1}{3}$ threshold. The multiverse analysis yielded inconclusive results for executive function and high-level cognition performance, in line with the primary analysis. The conclusions regarding heterogeneity for executive function and high-level cognition were robust, with 82% and 100% of analyses reaching the same conclusions, respectively. For studies investigating memory, however, only half of the alternative analyses aligned with the original findings. This discrepancy arose due to the inclusion of implausibly high effect sizes, which significantly increased heterogeneity. Results for all included models are presented in Table S5 and visualised in Figure 6.

Figure 6

Distribution of Bayes Factors Across Analytical Specifications of the Cognitive Domain Moderator Analysis



Note. The dots in the top panel represent Bayes factor estimates. The lines in the bottom panel correspond to the analytical choices made in the individual analyses, aligned vertically with the respective estimates. Black indicates the Bayes factor estimate and analytical choices of the primary analysis. The Priors, Outcome, Effect Size, Multilevel, and Unrealistic ES groups represent the choices for *Prior Specifications*, *Outcome Type*, *Effect Size Calculation*, *Accounting for Structured Data*, and *Unrealistically Large Effect Size*, respectively, as described in the Methods section.

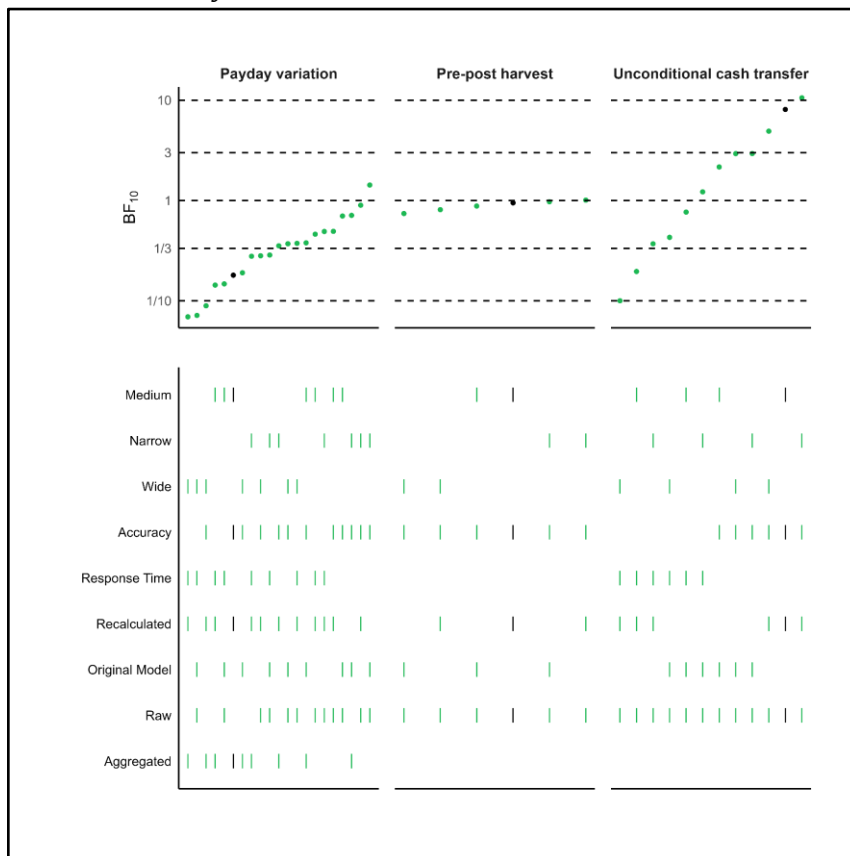
Empirical Approach

We found moderate evidence for an effect in unconditional cash transfer studies ($g = 0.08$ [0.00, 0.13], $BF_{10} = 8.08$) and moderate evidence against the effect in payday variation studies ($g = 0$ [-0.03, 0.05], $BF_{10} = 0.18$). For pre-post harvest studies, the evidence remains inconclusive ($g = -0.03$ [-0.48, 0.39], $BF_{10} = 0.95$), with very strong evidence for small study effects ($BF_{10} = 31.83$). Additionally, we found very strong evidence for the presence of heterogeneity in pre-post harvest studies ($\tau = 0.92$ [0.47, 2.08], $BF_{10} = 9.02e+68$), and strong evidence against its presence in payday ($\tau = 0.00$ [0, 0.08], $BF_{10} = 0.04$) and unconditional cash transfer studies ($\tau = 0.00$ [0, 0.07], $BF_{10} = 0.04$).

Multiverse Analysis. Only 45% of analyses provide moderate or strong evidence against the effect in payday variation studies, while the remaining analyses yield inconclusive evidence. Similarly, only 3 out of 12 analyses on UCTs indicate moderate or strong evidence for the effect, 2 suggest moderate evidence against it, and the rest are inconclusive. For pre-post harvest studies, the multiverse analysis revealed a lack of evidence for or against the effect, while supporting the presence of both small study effects and heterogeneity. In the case of payday studies, only 12 out of 21 models aligned with the original conclusion regarding the absence of heterogeneity. For UCT studies, the multiverse analysis supported the conclusion about the absence of heterogeneity in 10 out of 12 analyses. Results of all included models are reported in Table S6 and visualised in Figure 7.

Figure 7

Distribution of Bayes Factors Across Analytical Specifications of the Empirical Approach Moderator Analysis



Note. The dots in the top panel represent Bayes factor estimates. The lines in the bottom panel correspond to the analytical choices made in the individual analyses, aligned vertically with the respective estimates. Black indicates the Bayes factor estimate and analytical choices of the primary analysis. The Priors, Outcome, Effect Size, and Multilevel groups represent the choices for *Prior Specifications*, *Outcome Type*, *Effect Size Calculation*, and *Accounting for Structured Data*, respectively, as described in the Methods section.

This group of analyses excluded the *Unrealistically Large Effect Size* alternative decision. However, we included these effect sizes in all *Pre-post harvest* analyses, as excluding them would have left an insufficient number of studies to conduct most of the alternative analyses. Importantly, this adjustment did not impact the other two groups of analyses, as all studies in the Unrealistically Large Effect Size category were pre-post harvest studies.

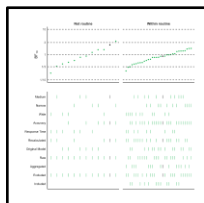
Financial Routine

The results were inconclusive regardless whether the manipulation occurred within participants' financial routines ($g = 0.05 [-0.02, 0.21]$, $BF_{10} = 0.86$) or not ($g = 0.06 [0, 0.15]$, $BF_{10} = 2.44$). Additionally, we found very strong evidence for the presence of heterogeneity in the within-routine studies ($\tau = 0.17 [0.09, 0.29]$, $BF_{10} = 1.99e+14$) and moderate evidence for a small study effect ($BF_{10} = 3.07$). In contrast, we found moderate evidence against the presence of heterogeneity in studies where manipulation was not part of the financial routine ($\tau = 0.03 [0, 0.21]$, $BF_{10} = 0.24$).

Multiverse Analysis. The multiverse analysis aligned with our primary conclusions. In studies where manipulation was within participants' financial routines, 30 out of 33 analyses evaluated the evidence as inconclusive. Similarly, in studies where the manipulation was not part of financial routines, 10 out of 12 analyses found the evidence to be inconclusive. Furthermore, 30 analyses provided evidence for the presence of heterogeneity in the within-routine studies. For studies where manipulation was not within financial routines, 8 out of 12 analyses supported the presence of heterogeneity. However, the robustness of the conclusion regarding the presence of a small study effect in the within-routine studies is less certain, as only 12 out of 33 analyses supported it. Results for all included models are reported in Table S7 and visualised in Figure 8.

Figure 8

Distribution of Bayes Factors Across Analytical Specifications of the Financial Routine Moderator Analysis



Note. The dots in the top panel represent Bayes factor estimates. The lines in the bottom panel correspond to the analytical choices made in the individual analyses, aligned vertically with the respective estimates. Black indicates the Bayes factor estimate and analytical choices of the primary analysis. The Priors, Outcome, Effect Size, Multilevel, and Unrealistic ES groups represent the choices for *Prior Specifications*, *Outcome Type*, *Effect Size Calculation*, *Accounting for Structured Data*, and *Unrealistically Large Effect Size*, respectively, as described in the Methods section.

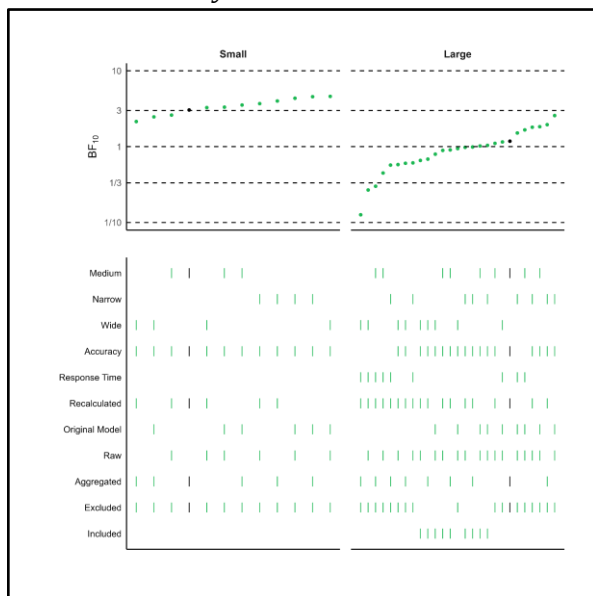
Manipulation Size

Studies using financial sums equivalent to at least a month's income provided inconclusive evidence ($g = 0.06$ [-0.00, 0.21], $BF_{10} = 1.18$), while studies involving smaller amounts showed moderate evidence for an effect ($g = 0.08$ [0, 0.17], $BF_{10} = 3.04$). Additionally, there was very strong evidence for heterogeneity in studies using larger sums ($\tau = 0.16$ [0.09, 0.28], $BF_{10} = 2.16e+14$) and moderate evidence for the presence of a small study effect ($BF_{10} = 3.29$). In contrast, there was moderate evidence against the presence of heterogeneity in studies using smaller sums ($\tau = 0.02$ [0, 0.21], $BF_{10} = 0.20$).

Multiverse Analysis. The multiverse analysis supported our conclusions regarding the role of the magnitude of financial manipulation. For studies using smaller sums, 9 out of 12 analyses supported the existence of the effect, while the remaining three were inconclusive. 24 of 27 analyses, that investigated studies where manipulations are larger than one month worth of income, were inconclusive, and the remaining 3 yielded evidence against the effect. Furthermore, 23 of the 27 analyses supported our conclusion that the group of studies applying larger manipulation sums are heterogeneous, and 20 that a small study effect is present. However, the robustness of the conclusion regarding the absence of heterogeneity in studies using smaller sums is unclear, as only 6 of the 12 analyses aligned with our original findings. Results of all included models are reported in Table S8 and visualised in Figure 9.

Figure 9

Distribution of Bayes Factors Across Analytical Specifications of the Manipulation Size Moderator Analysis



Note. The dots in the top panel represent Bayes factor estimates. The lines in the bottom panel correspond to the analytical choices made in the individual analyses, aligned vertically with the respective estimates. Black indicates

the Bayes factor estimate and analytical choices of the primary analysis. The Priors, Outcome, Effect Size, Multilevel, and Unrealistic ES groups represent the choices for *Prior Specifications*, *Outcome Type*, *Effect Size Calculation*, *Accounting for Structured Data*, and *Unrealistically Large Effect Size*, respectively, as described in the Methods section.

Discussion

Our systematic review and meta-analysis found inconclusive evidence regarding the positive impact of financial resources on cognitive performance. This conclusion was largely robust against alternative specifications of the analysis, including the choice of priors. If a positive effect exists, it is small ($g = 0.06$ [0.00, 0.18]), and considerably smaller than initially suggested by Mani and colleagues (2013). Furthermore, we found moderate evidence for publication bias, though this finding was sensitive to analytical variations.

The between-study heterogeneity was small by Cohen's benchmarks (REF) ($\tau = 0.14$ [0.09, 0.22]) but significant enough relative to the estimated effect size to obscure potential effects. We explored several sources of this heterogeneity. Although the alignment of the manipulation with participants' routines did not account for the variability in effect sizes, the type of cognitive functions measured, the empirical strategies employed, and the magnitude of financial changes offered insights into the contexts in which the investigated effect is more likely to prevail.

Our results indicate that not all cognitive domains are equally susceptible to the effect. Moderate evidence suggests that memory and working memory performance is the least likely to be affected, while based on the relative distribution of Bayes factors (Fig X), executive functions (excluding working memory) are the most likely to be impacted. This contradicts the findings of Szaszi and colleagues (REF), who provided evidence suggesting that working memory is more strongly impacted than inhibitory control, a key component of executive functioning. However, our findings were not consistent across alternative analyses, indicating that further research is required in all cognitive domains to draw definite conclusions.

Moderate evidence suggests that a financial sum smaller than one month's income influences cognitive performance, while the evidence is inconclusive about the effect of larger amounts. To the best of our knowledge, this finding does not align with existing theoretical frameworks of financial scarcity. A possible explanation is that a confounding factor, the timing of the cognitive measurement, introduced this difference. Included studies involving smaller sums seem to measure cognitive performance after the manipulation was administered following a shorter time interval (Table S2), although this cannot be quantified and directly tested due to the fact that these intervals were reported in broad intervals.

Our results also suggest that utilising unconditional cash transfers is the most effective way to induce the effect, while payday variation is the least effective. Notably, while this conclusion was sensitive to alternative analytical specifications, the relative distribution of the two sets of Bayes factors supports this statement. Factors such as the difference in the size of the employed financial manipulation, or their integration into participants' financial routines, could serve as explanations of this finding, but separate analyses did not support these hypotheses.

In summary, the current evidence in the literature is limited, making it difficult to draw definitive conclusions. We highlight several methodological considerations that could help advance research on this effect.

Test Uncertainty with Repeated Transfer Designs

We could not test the moderating role of financial uncertainty (i.e. the feeling of the risk of potentially not having enough on any given day (Lichand, Mani)), a key factor highlighted in prior research (REFs), as uncertainty levels were not manipulated differently across studies. The adverse effects that scarcity theory associates with subjective scarcity may, in fact, stem from financial uncertainty—a component of subjective scarcity—as it generates worries that could preoccupy individuals living in poverty and deplete their mental resources (Mullainathan, Shafir, 2013). The impact of certain aspects of uncertainty can be explored through pre-post harvest designs, as the timing and magnitude of the financial shock are relatively unpredictable (Lichand, Mani). However, because farmers are aware that a shock is likely to occur, complete uncertainty is absent. We propose that investigating the effects of repeated monetary transfers could offer valuable insights into the effects of financial uncertainty. Lump-sum payments, that are predominantly used, may offer temporary relief, but fail to address long-term financial instability. In contrast, smaller, recurring transfers could offer families a reliable financial help, aiding the management of daily financial challenges and sustain a longer-lasting reduction in worries stemming from uncertainty. Among the reviewed studies, however, only one examined the effects of repeated financial aid. Additionally, comparing the results of repeated and lump-sum interventions could help disentangle the short-term (e.g. attentional tunneling, increased cognitive load) and the medium-term effects of scarcity (e.g. sleep deprivation and starvation caused cognitive deficit).

Consider Different Aspects of Financial Scarcity

Changes in financial resources might affect cognitive performance through changing absolute (e.g., nutrition, stress from managing expenses), subjective (e.g., financial worries), and relative (e.g., social status) dimensions of scarcity (for details on these dimensions see REFs). Despite the multiple possible channels, prior studies failed to investigate how subjective and relative dimensions of scarcity change after the manipulation is applied, leaving the relationship between these dimensions and cognitive performance unexplored. For instance, in the study of Kaur and colleagues (REF), certain participants received the payment for their work earlier than others, while they still worked in the same environment. Their findings showed that receiving payment earlier had a beneficial effect on attentiveness in work tasks. However, we cannot know if the effect was induced by the higher amount of available financial resources, or by having more financial resources relative to their peers, or both. Conducting similar studies with full-factorial designs would help to clarify the role of relative financial status.

Subjective scarcity often remains unmeasured as well, despite its central role in scarcity theory (REF). The theory provides a broad framework describing how the feeling of not having enough to get by may affect a broad range of behaviours (e.g., Fehr et al., 2019; Shah et al., 2015), including cognitive performance. While researchers often use scarcity theory to explain the impact of changes in financial resources (REFs), only testing the relationship between subjective scarcity and cognitive function could yield direct evidence for or against the theory.

Focus more on the Exploration of the Generalisability of the Effect

In search of the effect, X out of X included papers did not account for differences among affected populations beyond participants' absolute financial status, assuming all participants would be similarly impacted—an assumption that seems implausible. Future research should focus on exploring the generalisability of the effect. First, we encourage researchers to measure a broad range of relevant mediators, including anxiety, nutrition, pain, sleep deprivation, access to education, environment, cognitive load, and financial control, even if these fall outside the primary goals of the study. Second, we recommend employing advanced analytical methods, such as random forest (REF) or LASSO (REF), to identify influential variables from a broader dataset. For example, Farbmacher and colleagues (REF) reanalysed Carvalho and colleagues' data (REF) using random forest analysis, leveraging one study to generate hypotheses and another to test them. This

approach successfully identified the effect on US individuals under 30 or over 70 with incomes below \$750, demonstrating the value of targeted exploration.

Limitations

Our investigation had a number of limitations. First, certain studies that were identified during the literature review were not included in the analysis due to not sharing their data. Second, our claims are not generalisable to the long-term effects of financial resources on cognitive performance, as only one study investigated such effects (Ayyagari). Third, some included studies used multilevel analyses, making it difficult to transform effect sizes. In certain cases, transformed effect sizes had confidence intervals that included zero, unlike the originals, which may have reduced the overall effect size estimate. Fourth, several subgroup analyses relied on as few as five or six effect sizes (e.g., payday variation, memory, small-sum manipulations), decreasing the confidence in our conclusions, limiting our ability to reliably assess the potential influence of the examined moderators. Finally, the studies in our meta-analysis varied across dimensions like the target population and the time interval between manipulation and cognitive performance measurement. This heterogeneity, combined with the small number of studies, may have introduced confounds, potentially weakening the internal validity of the meta-analysis, questioning that observed effects can be confidently attributed to manipulation of the financial resources.

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Old

Of the 46 effects from 11 papers, which were derived from studies where scarcity was quasi-experimentally or experimentally manipulated, 14 effects from 5 papers came from studies where scarcity priming and not actual financial resources formed the basis of the comparison between the experimental and control groups (e.g., REFs). As a result, we haven’t included them in the present manuscript (but see Szecsi et al...) .

As de Bruijn and Antonides (REF) note, fluid intelligence was often measured with a non-validated, shortened version of Raven’s progressive matrices (REF).

Alternative terms, such as "psychological responses to scarcity" (Zhao & Tamm, 2018) and "resource scarcity" (Hamilton et al., 2019), have also been used in earlier studies to refer to the same overarching concept.

Although prior theories suggest that both cognitive bandwidth and attentional tunneling as mechanisms are affected by financial resources, our findings suggest that the latter plays a more pronounced role. Executive functions that were measured in the included studies were less complex (e.g., inhibition, cognitive control), and demand less cognitive bandwidth (REF), making shifts in attention more pronounced compared to memory performance.

Another plausible explanation could be the difference in the targeted populations, however, payday (US adults with an annual household income of \$40,000 or less (Carvalho); low-income male workers India (Kaur)) and cash transfer (homeless individuals in Vancouver (Dwyer); US individuals in poverty (Jaroszewicz); low-income household members in Kenya (Kansinkas); low-income, high-risk individuals in Liberia (Szaszi)) studies investigate their claims on populations with similar poverty levels.

Accordingly, we applied little to no restriction on the context or the populations of the study, nor was the literature search restrained to specific causal designs. Neither was the size of the financial sum used in the manipulation nor the time passed between the manipulation and the measurement of cognitive functions prespecified. Furthermore, we applied broadly defined subcategories of cognitive functioning in our literature search. While we acknowledge that these broad labels are non-exhaustive, we used these categories as they are widely used (e.g., REF) as there is no generally accepted unifying theory of cognitive functions.

We excluded children from our investigation on scarcity's impact on cognitive performance for two reasons: First, studies on long-term effects of scarcity in children often necessarily compare differences in cognitive development, unlike those on adults. Second, children's responsibilities in life differ, suggesting scarcity affects their cognition through different pathways.

PÉLDA LEHET ÍGY KELLENE ÁTÍRNI A VÉGÉT

In order to support the evidence accumulation and increase the generalizability of the findings, we emphasize the importance of two research practices. First, researchers need to be explicit on what their theory predicts and what their study tests regarding the underlying mechanisms of the effect: Does the study investigate the short-term immediate impact or the cumulative long-term impact of financial scarcity on cognition? How much time needs to pass for money scarcity / money affluence to be present to have an effect? Which timing (lump-sum vs. monthly instalments) or

magnitude in the change in financial scarcity have the strongest effect on 1 dollar investment? What is the impaired cognitive functioning of the poor reflect: an acute capability deficit caused by cognitive load, short term sleep deprivation, malnutrition or stress; a chronically diminished capability due to long term sleep deprivation, malnutrition, stress; or a reduced ability (not capability) due to the lack of education care parental care. Is there a specific part of cognition which is affected and why? Are all subsets of the population affected to the same extent by poverty?

“The studies are weighted using inverse-variance weights, which has been shown to perform better than weighting by sample size in random-effects analysis (Marín-Martínez & Sánchez-Meca, 2010).”

- Mention the practice that authors look separately on rt and accuracy and how it is not optimal as it could be misleading: it might be the preferred strategies that differ in conditions
- What were these manipulations enough for, did it meaningfully change the financial situation of participants? Did participants evaluate the change as meaningful? what could people really do the money between getting it and completing the cognitive task

Additionally, the moderator analysis needs to be treated with caution. While our investigation tested four different moderators, the number of included studies was relatively low (X).

- Had the participants have time to spend the money?
 - Situational effects: they all focus on 1. Attentional (tax) and tunnelling: differential predictions - for task using intuitive processes no deteriorated performance but those tasks which need deliberation more. How to measure attention? We have not seen that coming.

Consequently, following Almeida and colleagues (REF), we could not test if the differences in financial resources exert their influence through education.

-

- Lichand and Mani (REF) highlight uncertainty as the source of the adverse effects of financial scarcity.
- It would be interesting to test effects of reliable, repeating money infusion (such as the ubi study)
 - *Lump-sum vs. monthly transfers*
 - Disentangle situational effects vs. long term effects: situational effect can be changed quickly short term effect. state level effect harder to change but have long term effects.
 - Situational effects: they all focus on 1. Attentional (tax) and tunnelling: differential predictions - for task using intuitive processes no deteriorated performance but those tasks which need deliberation more. How to measure attention? We have not seen that coming.
 - Scarcity theory: “having less than you feel you need”. Is it continuous? Is there a difference between having much less and slightly less than you feel you need? **We can't expect any single experiment to remove individuals completely from subjective scarcity to test the financial aspect of scarcity theory on the field, as it would require immense financial resources.**
 - In the study of Kaur and colleagues (REF), certain workers not only receive the payment for their work earlier, but both early and late payment groups are informed about the discrepancy in the timing. This leads to a confound: we cannot be certain whether the differences in productivity are the consequence of more cash on hand, or the feeling of relative abundance or scarcity.
 - Sense of agency over budget
 - In the study conducted by Ong and colleagues (REF), although the financial status of participants improved by having their debts paid off, they did not have agency over the money. As this is the only study where this situation occurred, we cannot be sure how this factor influenced the effect.

Positive or negative framing

- Or change at all, testing in contexts where no change happened would be vital

One limitation of the present study is that we cannot be certain that all relevant empirical papers were identified during our search process. However, given the large number of topic-level experts involved, we believe that our study provides a strong summary of the available causal evidence. One weakness of the presented evidence comes from the fact that the field is dominated by pre-post design studies, and no randomized controlled trials were published yet. Notably, during the search of the grey literature, we identified several ongoing randomized controlled trials (Jarosevitz et al., 2020; Mani et al., 2020; Szaszi et al., 2020), which will hopefully provide further causal evidence on how financial scarcity affects cognitive performance.

No preferences / trade-offs, no priming studies...

In sum, our meta-analysis suggests that money scarcity has moderate size negative effects on cognitive performance, but our knowledge on when and why this effect unfolds is very limited. Although the topic has attracted a wide policy and academic interest, there is still only a small number of quasi(experimental) studies completed which leaves many questions open for future research endeavours.

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Throw out from here

- How is poverty defined?
 - Does the theory tell something about the dynamics of poverty? Does the change in the level poverty matters or the level of poverty? If the theory focuses on change, what size of change shall be considered?
 - Is the theory focused on the cumulative long-term impact or the immediate impact of poverty on cognition?
 - For how long the effect of poverty is expected to last?
 - How much time need to pass in poverty before the effect of poverty unfolds
 - Would we get the same effect if poverty would defined relatively ('the gap between one's needs and the resources available to fulfill them') or objectively (having <X USD) ?
- in poverty the same effect on cognitive functions?

Temporal dynamics:

Prior theories providing explanation on how poverty effects cognition can be classified into three categories:

How can be the cognitive capacity be deteriorated?

-
attention capturing + capacity is lowered due to a state of fatigue, sleeplessness etc. + less efficient cognitive strategies as a result of lack of education
attention capturing + capacity is lower due to long term states, malnutrition, sleep + less efficient for lack of education

We posit that this framework has the potential to increase the conceptual clarity for understanding when, how, and why' this effect unfolds, and also helps to fine-tune their research design, methodological choices and reporting: the *eight questions framework* which illustrates the important theoretical and empirical decisions

Measure the potential moderators and mediators which can be used in further meta-analysis...
To understand when and why poverty deteriorates cognitive functioning (to explain the huge variance in effect sizes (.X-.Y), the focus of future research shall be on the exploration and measurement of moderators/mediators.

Additional note: these search words (at least in Web of science) are also looked up in the abstracts of the articles as well.

Pre-registration template for meta-analysis: <https://osf.io/zbr5u>

Since then, this pivotal paper has become one of the most cited social science paper*, argument for the introduction of universal basic income ([TED, 2017](#)) and a reference point for future research on the topic.

In September 2019, based on Google Scholar, we screened all the studies which cited Mani et al. (2013). One team member reviewed the titles and abstracts of 1402 citations while full-text review was carried out by at least two team members. Three papers were identified as the result of the screening process. Furthermore, this pilot screening was used as an input to refine the search queries used in the systematic search phase.

Expert recommendations:

We got 21 replies that contained 16 suggested articles, 2 databases, a handbook. 2 articles turned out to be relevant for our analysis.

- a) Judgment and Decision Making mailing list (7 replies)
- b) Contacting the first authors published studies including understandingscarcity.com (25 contacts, 10 replies)
- c) Poverty Action Lab (J-PAL) (143 contacts, 4 replies)

Inclusion criteria:

1. English language papers
2. Original empirical studies
3. The authors captured the causal effect of poverty on cognitive functions. That is, they capture/manipulate the level of poverty/money scarcity (the amount of available money of the participants). They might do that through
 - i. Randomized Control Trials
 - ii. Basic non-experimental designs
 1. The variation of (time) payday (pre-post)
 2. Instrumental variable analysis
 - iii. Other non-predefined quasi-experimental designs
4. The outcome is assessed with one or more (broadly defined) cognitive function measure. These cognitive functions can be
 - i. Memory
 - ii. Attention and focus
 - iii. Executive functions (shifting / inhibition, inhibitory control, cognitive control / monitoring)
 - iv. Higher-order intelligence
5. Individuals were the unit of analysis
6. Manipulated or captured the variance of real money

Exclusion criteria:

- (1) The results are based on correlational data
- (2) The language of the paper is not English
- (3) The publication is a(n)
 - book, book chapter
 - review article
 - retracted publication
 - encyclopedia
 - editorial content