UNIVERSITY of CALIFORNIA PRESS | Collabra: Psychology

Szecsi, P., Kolumban, P., Taka, A., & Szaszi, B. (2025). The Impact of Financial Resources on Cognitive Performance: A Systematic Review and Meta-Analysis. *Collabra: Psychology*, *11*(1). https://doi.org/10.1525/collabra.147236

Social Psychology

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

Peter Szecsi[1,2] [a], Pal Kolumban[1,2], Aikaterini Taka[3], Barnabas Szaszi[2,4] [b]

[1] Doctoral School of Psychology, ELTE Eötvös Loránd University, Budapest, Hungary, [2] Institute of Psychology, ELTE Eötvös Loránd University, Budapest, Hungary, [3] Department of Geography, University of the Aegean, Mytilene, Greece, [4] Corvinus Institute for Advanced Studies (CIAS), Corvinus University of Budapest, Hungary

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

https://doi.org/10.1525/collabra.147236

Collabra: Psychology

Vol. 11, Issue 1, 2025

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 26 effect sizes from 14 relevant studies (N_{pooled} = 30,341). We found that the aggregated findings remain inconclusive regarding the effect's existence (BF_{10} = 1.35) but suggests that, if present, it is relatively small (g = 0.06 [0.00, 0.18], τ = 0.14 [0.09, 0.22]). These findings are consistent across alternative analytical specifications. We detected moderate support for the effect in studies using unconditional cash transfers (BF_{10} = 8.08) and moderate support against it in payday variation designs (BF_{10} = 0.18). However, we found no support for or against the hypothesis that the integration of financial windfalls into participants' financial routines influences the effect. Additionally, our synthesis provides moderate support for the absence of an effect of financial resources on 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.

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 & Barr, 2009; Kim et al., 2006; Neal et al., 2001; Shah et al., 2012). Cognitive functioning is discussed as one major pathway through which such poverty traps might be expressed (de Bruijn & Antonides, 2022; Haushofer & Salicath, 2023; Mullainathan & Shafir, 2013). According to this view, living in poverty is associated with several factors (e.g., high anxiety and stress (Ridley et al., 2020), malnutrition (Siddiqui et al., 2020), pain (Chou et al., 2016), chronic disease limitations on activity (Braveman et al., 2010), and sleep environments) that negatively impact people's cognitive functioning (de Bruijn & Antonides, 2022; Dean et al., 2017). Then, decreased cognitive performance can undermine the chances to escape poverty through various channels such as schooling (Cawley et al., 2001; Duquennois, 2022) or job performance (Bishop, 1992; Kaur et al., 2025). 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 & Remy, 2014; Smith et al., 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 et al., 1997; Schoon et al., 2012). De Almeida et al. (2024) 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

a Please address correspondence to pter.szecsi@gmail.com.

b Barnabas Szaszi's primary affiliation is Corvinus Institute for Advanced Studies (CIAS), Corvinus University of Budapest.

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 Indian sugarcane farmers. They observed that farmers scored lower on cognitive control and fluid intelligence 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 (2013) for a critique of their methods). Similar studies in Zambia (Fehr et al., 2022) and Uganda (Pande, 2023) replicated this effect, with effect sizes varying from small to huge, while Lichand and Mani (2020) failed to replicate the finding in Brazil. 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. (2025) 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 et al., 2022), in the US (Jaroszewicz et al., 2024), and in Canada (Dwyer et al., 2023). When the effect was found, it was small (Hedge's g = 0.12). Other research designs were used successfully to demonstrate the effect: Ayyagari and Frisvold (2016) exploited a systematic error in the US social security payments, while Ong and colleagues (2019) examined the effect of paying off debts for people living 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" (Hagenaars & de Vos, 1988, p. 212)), like sleep deprivation (Lim & Dinges, 2010), and pain (Rathbone et al., 2016) are known for affecting various cognitive functions. 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 (Mullainathan & Shafir, 2013)). According to *scarcity theory*, a broad framework describing the mental consequences of subjective scarcity (Hamilton et al., 2019; Mullainathan & Shafir, 2013; Zhao & Tomm, 2018), two major phenomena affect cognitive performance (de Bruijn & Antonides, 2022; Haushofer & Salicath, 2023). First, it is hypothesised that the cognitive bandwidth of individuals experiencing scarcity is reduced, thereby having less capacity for mentally draining tasks. 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. The effect has been tested with tools measuring various cognitive functions (e.g., fluid intelligence, working memory, cognitive control), but yielded contradictory results (Haushofer & Salicath, 2023). Szaszi and colleagues (2022) explored whether different facets of cognitive functioning are impacted distinctly in a study utilising 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 (Szaszi et al., 2022). Second, certain manipulations used in the literature (Cassidy, 2018; e.g., Kansikas et al., 2023) might not produce large enough changes in financial resources to stand out from everyday financial fluctuations (de Bruijn & Antonides, 2022; Mani et al., 2020). 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 (de Bruijn & Antonides, 2022). 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 (Haushofer & Salicath, 2023; Lichand & Mani, 2020). 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 (de Bruijn & Antonides, 2022; Haushofer & Salicath, 2023). 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 (Bartoš et al., 2022; Maier et al., 2023), 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

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.

al., 2015). Deviations from the guidelines are detailed in Supplementary Table S1 and Table S2.

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 Supplementary Table S3 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 Supplementary Table S3. 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 https://osf.io/qdtg4.

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 Supplementary Fig. 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* (Lüdecke, 2016). 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* (Bartoš & Maier, 2020). 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 (Bartoš et al., 2022).

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' framework (Jeffreys, 1961) with a slight modification (see Fig. 1). Specifically, we treated Bayes factors between ⅓ and 3 as inconclusive rather than classifying them as anecdotal evidence, in line with the approach of Keysers and colleagues (2020).

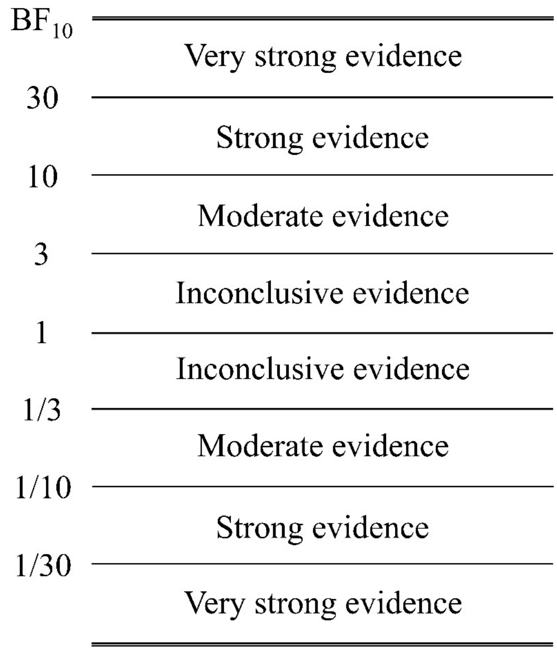

Figure 1. Interpretation Scale for Bayes Factors

Note. A scale illustrating the interpretation of Bayes factors, with regions indicating evidence strength from very strong support for the null to very strong support for the alternative hypothesis.

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 including working-memory, and 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 (Ayyagari &

Frisvold, 2016; Ong et al., 2019) 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 (Steegen et al., 2016). The multiverse analysis involves "performing all analyses across the whole set … of reasonable scenarios" (Steegen 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 article was omitted because it lacked the necessary results for inclusion, and the authors did not provide additional data upon request. Ultimately, 9 papers met the inclusion criteria, with 2 additional articles sourced elsewhere, resulting in a final dataset of 11 empirical papers (pooled sample size = 30,341) with 14 studies and 39 effects. Fig. 2 illustrates this process using a PRISMA flow diagram. Supplementary Table S4 summarises the main characteristics of the identified studies, while Table S5 lists the properties of analysed effect sizes. Note, that Supplementary Table S5 includes more than 39 effect sizes due to the different recalculations of the same effects. Supplementary Table S6 indicates which results were used in the primary analyses.

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 (τ = 0.14 [0.09, 0.22], BF_{10} = 2.16e+13). These findings are visualised in a forest plot in Fig. 3. The analysis indicated a small evidence in favour of the presence of small study effects (BF_{10} = 3.56).

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 Supplementary Table S7 and visualised in Fig. 4.

Subgroup Analysis

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

Table 2. Subgroup Analysis Results

Subgroup	*N*	Hedge's g	CI_{lower}	CI_{upper}	τ	τ_{lower}	τ_{upper}	Effect BF_{10}	Heterogeneity BF_{10}	Bias BF_{10}	Hierarchical BF_{10}
Pre-post harvest	9	-0.03	-0.476	0.395	0.918	0.47	2.077	0.947	9.025e+68	31.833	1.504
Payday variation	5	0.002	-0.029	0.053	0.004	0	0.081	0.18	0.036	2.475	0.072
Unconditional cash transfer	13	0.075	0	0.129	0.003	0	0.071	8.083	0.035	0.579	0.064
Memory	6	0.007	0	0.065	0.004	0	0.075	0.241	0.036	1.027	0.055
Executive function	13	0.111	0	0.254	0.153	0.078	0.281	2.895	5.709e+06	1.438	8.762
High-level cognition	7	0.029	-0.048	0.207	0.187	0.08	0.371	0.535	126.368	1.932	3.97
Windfall ≥ Monthly Income	21	0.057	-0.003	0.207	0.157	0.094	0.267	1.18	2.155e+14	3.287	4.033
Windfall < Monthly Income	5	0.075	0	0.165	0.025	0	0.214	3.042	0.2	1.128	0.293
Within routine	12	0.047	-0.025	0.21	0.167	0.095	0.295	0.865	1.987e+14	3.071	2.894
Not within routine	14	0.065	0	0.15	0.027	0	0.208	2.436	0.235	1.829	0.46

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 Hegde's g estimate. τ_{lower} and τ_{upper} indicate the lower and upper ends of the 95% confidence interval of the heterogeneity estimate.

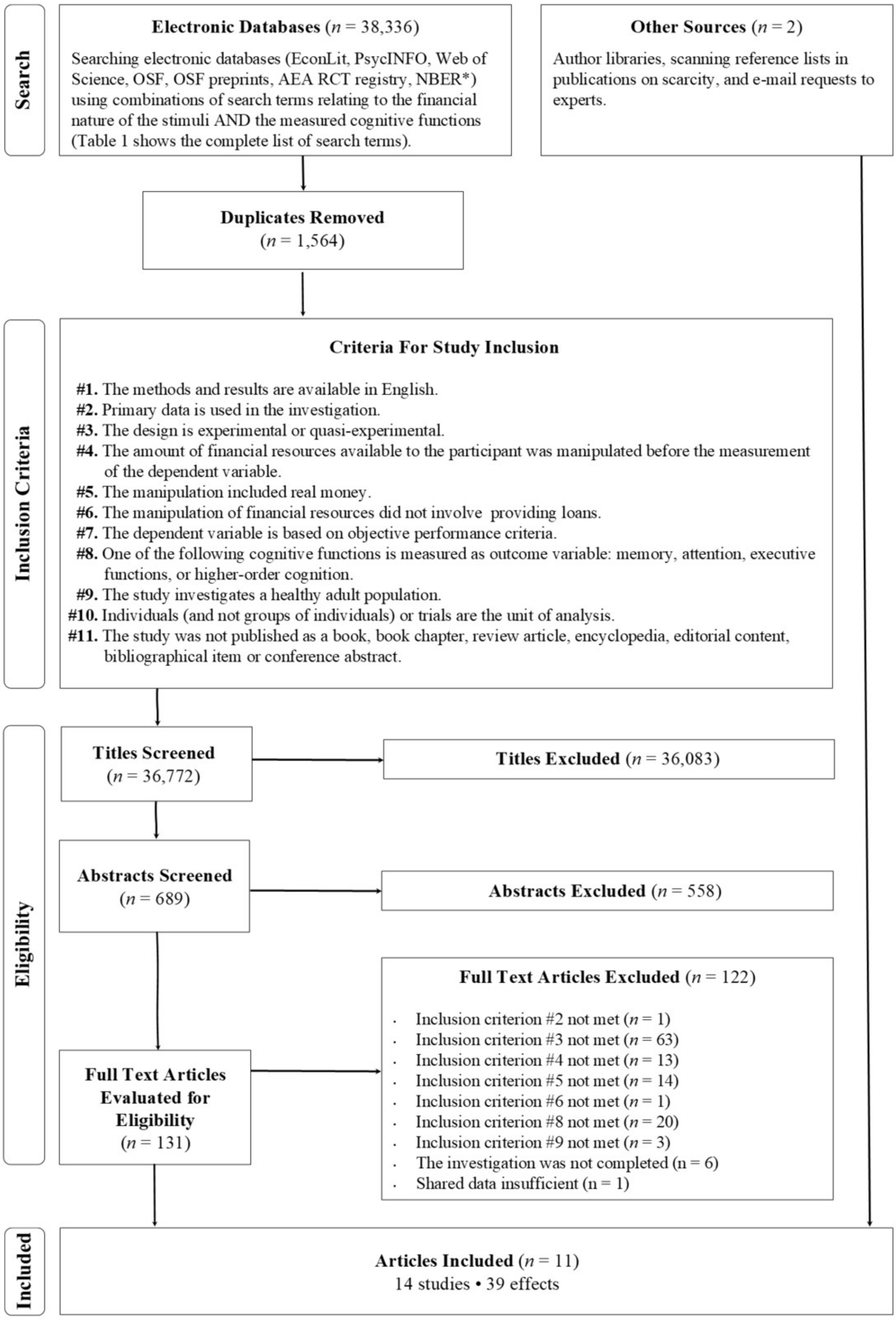

Figure 2. PRISMA Flow Diagram

Note. PRISMA flow diagram illustrating the study selection process, including the number of records identified, screened, excluded, and included at each stage, along with reasons for exclusions at the full-text stage. 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.

Cognitive Domain

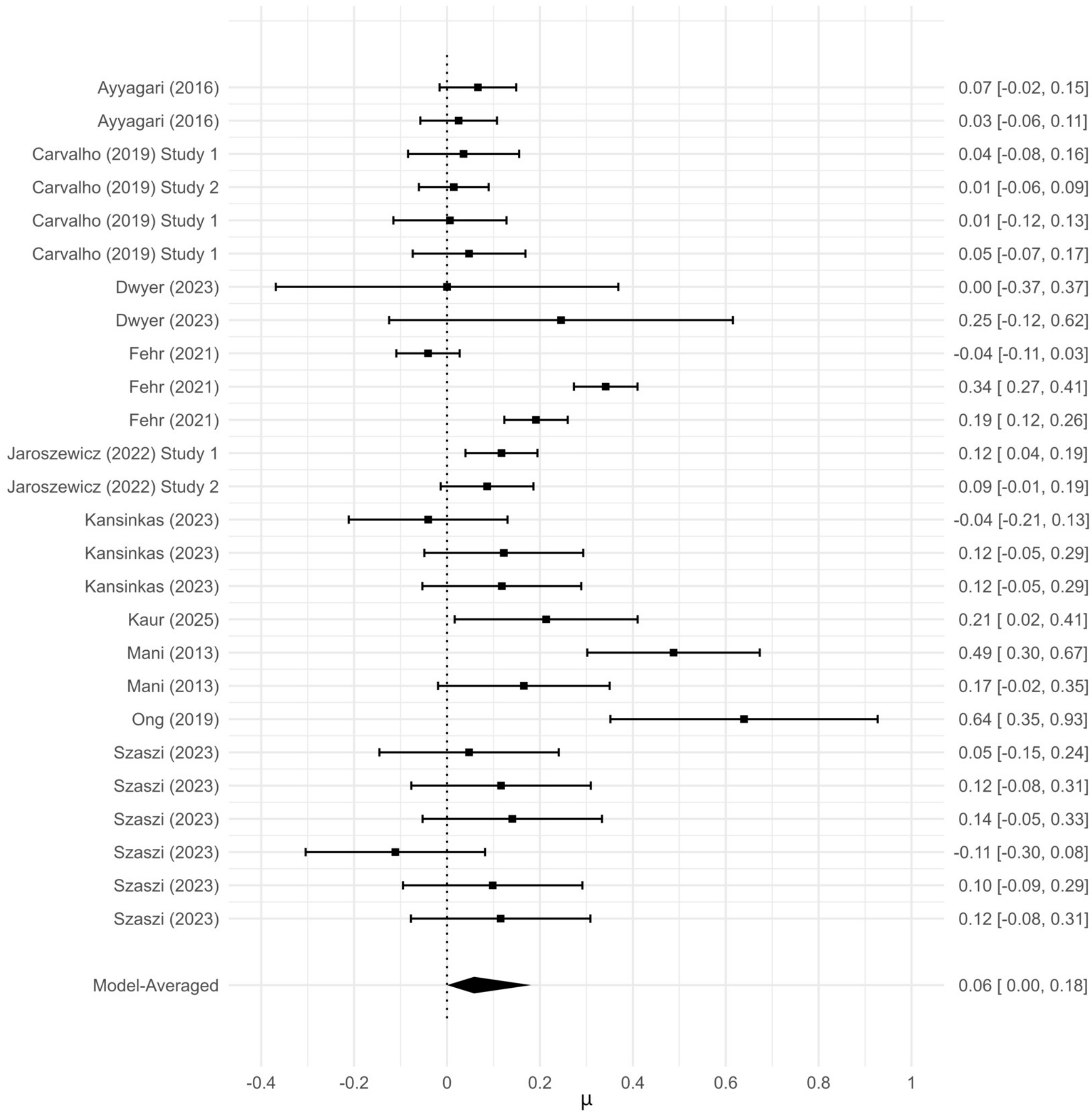

Figure 3. Forest Plot of the Primary Effect Size Estimate

Note. Forest plot displaying the individual study effect sizes and the overall pooled estimate. The x-axis represents Hedge's *g*. The right side of the plot displays the mean estimates along with the corresponding 95% confidence intervals.

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 (τ = 0.00 [0, 0.07], BF_{10} = 0.04), while very strong evidence for heterogeneity was observed in the executive function (τ = 0.15 [0.08, 0.28], BF_{10} = 5,708,554.01) and high-level cognition categories (τ = 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 ⅓ 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 Supplementary Table S8 and visualised in Fig. 6.

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-

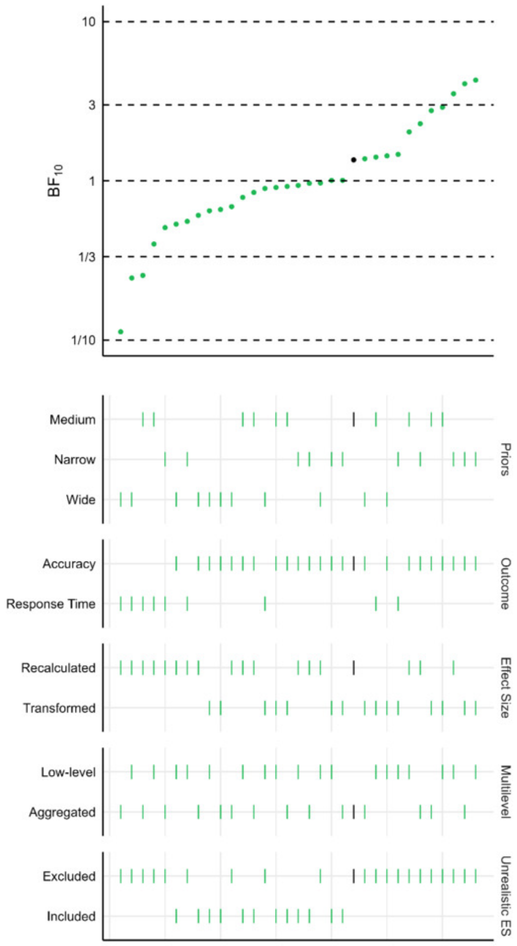

Figure 4. Distribution of Bayes Factors Across Analytical Specifications of the Primary Analysis

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.

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 (τ = 0.92 [0.47, 2.08], BF_{10} = 9.02e+68), and strong evidence against its presence in payday (τ = 0.00 [0, 0.08], BF_{10} = 0.04) and unconditional cash transfer studies (τ = 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 Supplementary Table S9 and visualised in Fig. 7.

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 (τ = 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 (τ = 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 Supplementary Table S10 and visualised in Fig. 8.

Manipulation Size

Studies using financial sums equivalent to at least a month's income provided inconclusive evidence (g = 0.06 [-0.00, 0.21], BF10 = 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 (τ = 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 (τ = 0.02 [0, 0.21], BF_{10} = 0.20).

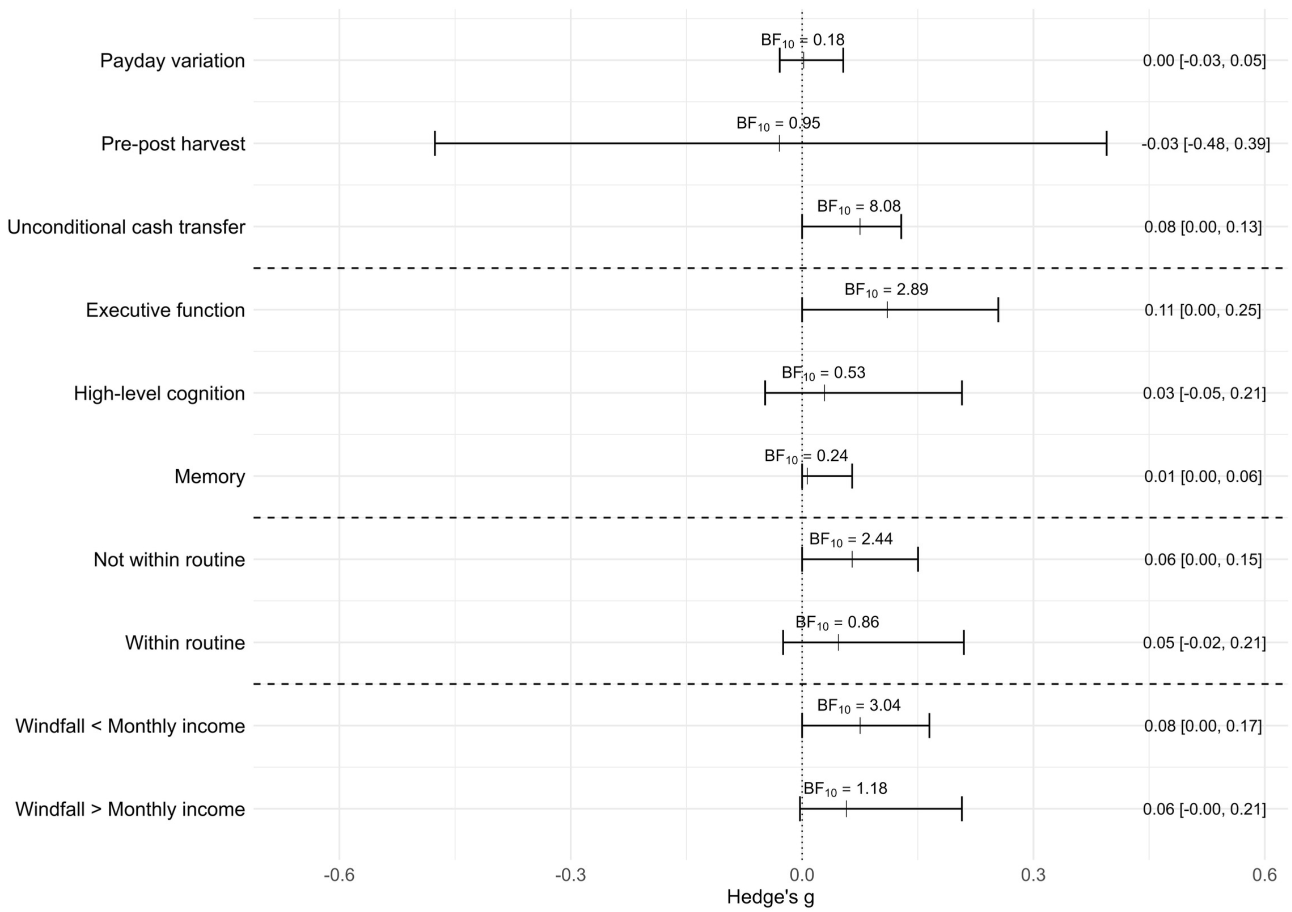

Figure 5. Estimates from Subgroup Analyses

Note. Effect size estimate plot displaying the individual overall pooled estimates in separate subgroups analyses. 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.

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 Supplementary Table S11 and visualised in Fig. 9.

Discussion

Our systematic review and meta-analysis yielded inconclusive findings 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 (1988) (τ = 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 support indicates that memory performance (including working-memory) is the least likely to be affected, while based on the relative distribution of Bayes factors (Fig. 6), executive functions (excluding working memory) are the most likely to be impacted. This contradicts the findings of Szaszi and colleagues (2022), who provided evidence suggesting that working memory is more strongly impacted than inhibitory

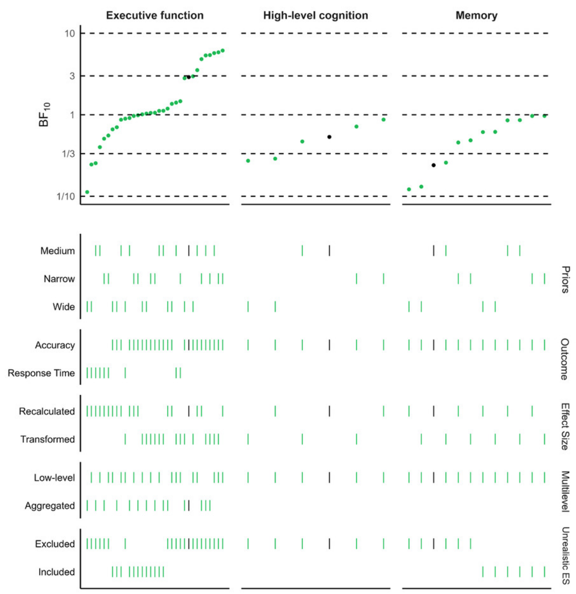

Figure 6. Distribution of Bayes Factors Across Analytical Specifications of the Cognitive Domain Moderator Analysis

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.

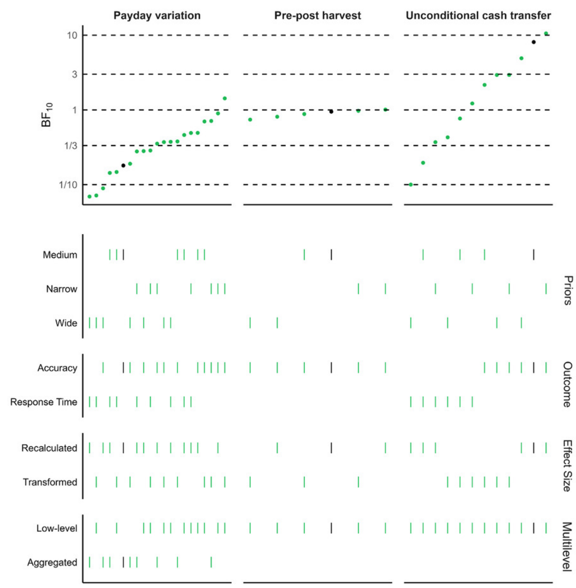

Figure 7. Distribution of Bayes Factors Across Analytical Specifications of the Empirical Approach Moderator Analysis

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.

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 support indicates that a financial windfall 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 (Mani et al., 2020). Included studies involving smaller windfalls seem to measure cognitive performance after the manipulation was administered following a shorter time interval (Supplementary Table S4), although this cannot be quantified and directly tested as these intervals were reported in broad intervals. Alternatively, the observed effect may be incidental, with even the larger windfalls being too small to plausibly generate a measurable improvement in cognitive performance.

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 body of findings 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, 2020)), a key factor highlighted in prior research (Lichand & Mani, 2020), as we assume that utilised manipulations affected uncertainty levels similarly across studies (the alignment with participants' routines might affects uncertainty, but investigating that aspect yielded inconclusive results). 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, 2020). However, because farmers

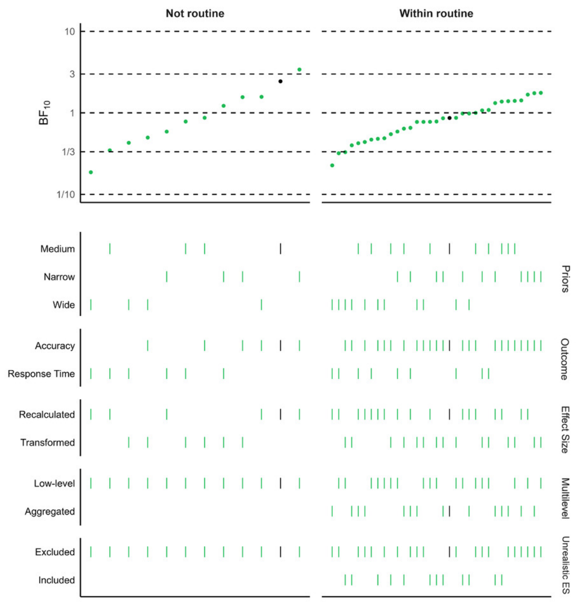

Figure 8. Distribution of Bayes Factors Across Analytical Specifications of the Financial Routine Moderator Analysis

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.

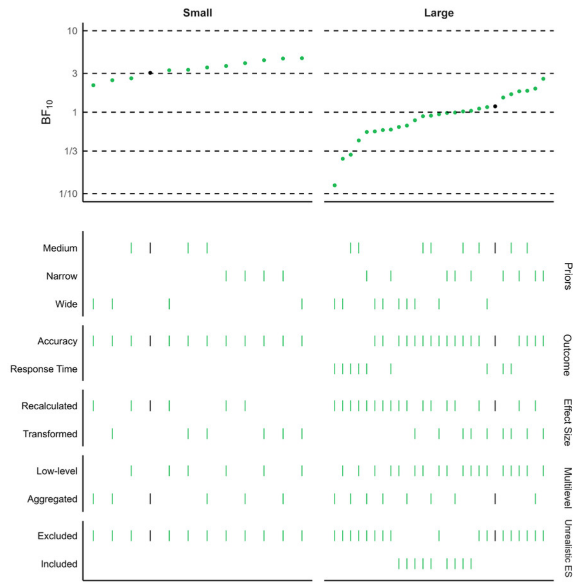

Figure 9. Distribution of Bayes Factors Across Analytical Specifications of the Manipulation Size Moderator Analysis

Note. The dots in the top panel represent Bayes factor estimates. 'Small' refers to analyses that included studies where manipulations involved less than one month of income, while 'Large' refers to analyses that included all other studies. 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.

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 (Ayyagari & Frisvold, 2016). 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 Hagenaars and de Vos (1988)). Despite the multiple possible channels, only 4 of the 11 included papers investigated how the subjective dimensions, and none, how the 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 (2025), 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 (Mullainathan & Shafir, 2013). The theory provides a broad framework describing how the feeling of not having enough to get by may affect a broad range of behaviours (Fehr et al., 2019; e.g., Mullainathan & Shafir, 2013; Shah et al., 2015), including cognitive performance. While researchers often use scarcity theory to explain the impact of changes in financial resources, 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, none of the included papers made systematic attempts to identify the most affected subgroups of the target populations assuming all (financially struggling) 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 (Fife & D'Onofrio, 2023) or lasso regression (Helwig, 2017), to identify influential variables from a broader dataset. For example, Farbmacher and colleagues (2021) reanalysed Carvalho and colleagues' data (2016) 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 & Frisvold, 2016). 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. Fifth, 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. Sixth, it must be considered in interpretation that many included studies rely on non-experimental shocks where multiple factors change simultaneously (Wicherts & Scholten, 2013). For example, pre/post-harvest or payday designs often attribute effects to liquidity while overlooking concurrent contextual changes. Seventh, the scarcity changes examined in the included studies are typically small relative to individuals' overall scarcity levels, which limits the ability to assess the full impact of scarcity on cognitive performance; our analysis is therefore focused on the effects of these smaller shifts rather than the broader consequences of persistent scarcity. Finally, the methods were not pre-registered.

Contributions

Contributed to conception and design: PS, AT, BS
Contributed to acquisition of data: PS, PK, AT, BS
Contributed to analysis and interpretation of data: PS, BS
Drafted and/or revised the article: PS, BS
Approved the submitted version for publication: PS, PK, AT, BS

Acknowledgements

We would like to thank our research assistants (Patrícia Dávid, Emma Kiss), and the 23 topic experts who kindly contributed to this study.

Funding Information

The work of BS was supported by the Eötvös Loránd University Excellence Fund (EKA), the National Excellence Scholarship, and the Scholarship of the Bolyai Award.

Ethics Statement

This study did not involve testing of human participants.

Competing Interests

The authors declare no potential conflict of interest.

Data Accessibility Statement

The extracted data, analysis codes, generated models, and supplementary materials can be accessed on the OSF page of the project at https://osf.io/qdtg4. Datasets obtained from other researchers are not publicly accessible but can be requested from the corresponding author of this paper, upon approval from the original data publishers. Additional tables and figures are provided in the Supplementary Materials.

Editors: Norman Farb (Senior Editor)

Submitted: August 13, 2025 PST. Accepted: September 19, 2025 PST. Published: December 10, 2025 PST.

References

Al Hazzouri, A. Z., Elfassy, T., Sidney, S., Jacobs, D., & Yaffe, K. (2017). Sustained economic hardship and cognitive function: The coronary artery risk development in young adults study. *American Journal of Preventive Medicine*, *52*(1), 1–9. https://doi.org/10.1016/j.amepre.2016.08.009

Ayyagari, P., & Frisvold, D. (2016). The impact of social security income on cognitive function at older ages. *American Journal of Health Economics*, *2*(4), 463–488. https://doi.org/10.1162/AJHE_a_00058

Barnett, W. S. (1998). Long-term cognitive and academic effects of early childhood education on children in poverty. *Preventive Medicine*, *27*(2), 204–207. https://doi.org/10.1006/pmed.1998.0275

Barr, M. S. (2012). *No slack: The financial lives of low-income Americans*. Rowman & Littlefield.

Bartoš, F., & Maier, M. (2020). *RoBMA: An R Package for Robust Bayesian Meta-Analyses*. https://CRAN.R-project.org/package=RoBMA

Bartoš, F., Maier, M., Quintana, D. S., & Wagenmakers, E.-J. (2022). Adjusting for publication bias in JASP and R: Selection models, PET-PEESE, and robust Bayesian meta-analysis. *Advances in Methods and Practices in Psychological Science*, *5*(3), 25152459221109259. https://doi.org/10.1177/25152459221109259

Bishop, J. (1992). The impact of academic competencies on wages, unemployment, and job performance. *Carnegie-Rochester Conference Series on Public Policy*, *37*, 127–194. https://doi.org/10.1016/0167-2231(92)90006-5

Blank, R. M., & Barr, M. S. (2009). *Insufficient funds: Savings, assets, credit, and banking among low-income households*. Russell Sage Foundation.

Braveman, P. A., Cubbin, C., Egerter, S., Williams, D. R., & Pamuk, E. (2010). Socioeconomic disparities in health in the United States: What the patterns tell us. *American Journal of Public Health*, *100*(S1), S186–S196. https://doi.org/10.2105/AJPH.2009.166082

Carvalho, L. S., Meier, S., & Wang, S. W. (2016). Poverty and economic decision-making: Evidence from changes in financial resources at payday. *American Economic Review*, *106*(2), 260–284. https://doi.org/10.1257/aer.20140481

Cassidy, R. (2018). *Are the poor so present-biased?* https://doi.org/10.1920/wp.ifs.2018.2418

Cawley, J., Heckman, J., & Vytlacil, E. (2001). Three observations on wages and measured cognitive ability. *Labour Economics*, *8*(4), 419–442. https://doi.org/10.1016/S0927-5371(01)00039-2

Chou, E. Y., Parmar, B. L., & Galinsky, A. D. (2016). Economic insecurity increases physical pain. *Psychological Science*, *27*(4), 443–454. https://doi.org/10.1177/0956797615625640

Cohen, J. (1988). *Statistical power analysis for the behavioral sciences* (2nd ed.). L. Erlbaum Associates.

de Almeida, F., Scott, I. J., Soro, J. C., Fernandes, D., Amaral, A. R., Catarino, M. L., Arêde, A., & Ferreira, M. B. (2024). Financial scarcity and cognitive performance: A meta-analysis. *Journal of Economic Psychology*, *101*, 102702. https://doi.org/10.1016/j.joep.2024.102702

de Bruijn, E.-J., & Antonides, G. (2022). Poverty and economic decision making: A review of scarcity theory. *Theory and Decision*, *92*(1), 5–37. https://doi.org/10.1007/s11238-021-09802-7

Dean, E. B., Schilbach, F., & Schofield, H. (2017). Poverty and cognitive function. In *The economics of poverty traps* (pp. 57–118). University of Chicago Press.

Duquennois, C. (2022). Fictional Money, Real Costs: Impacts of Financial Salience on Disadvantaged Students. *American Economic Review*, *112*(3), 798–826. https://doi.org/10.1257/aer.20201661

Dwyer, R., Palepu, A., Williams, C., Daly-Grafstein, D., & Zhao, J. (2023). Unconditional cash transfers reduce homelessness. *Proceedings of the National Academy of Sciences*, *120*(36), e2222103120. https://doi.org/10.1073/pnas.2222103120

Farbmacher, H., Kögel, H., & Spindler, M. (2021). Heterogeneous effects of poverty on attention. *Labour Economics*, *71*, 102028. https://doi.org/10.1016/j.labeco.2021.102028

Fehr, D., Fink, G., & Jack, B. K. (2022). Poor and rational: Decision-making under scarcity. *Journal of Political Economy*, *130*(11), 2862–2897. https://doi.org/10.1086/720466

Fehr, D., Fink, G., & Jack, K. (2019). *Poverty, Seasonal Scarcity and Exchange Asymmetries* (No. w26357; p. w26357). National Bureau of Economic Research. https://doi.org/10.3386/w26357

Feinstein, L. (2003). Inequality in the early cognitive development of British children in the 1970 cohort. *Economica*, *70*(277), 73–97. https://doi.org/10.1111/1468-0335.t01-1-00272

Fife, D. A., & D'Onofrio, J. (2023). Common, uncommon, and novel applications of random forest in psychological research. *Behavior Research Methods*, *55*(5), 2447–2466. https://doi.org/10.3758/s13428-022-01901-9

Haaf, J. M., & Rouder, J. N. (2021). Does every study? Implementing ordinal constraint in meta-analysis. *Psychological Methods*. https://doi.org/10.1037/met0000428

Hagenaars, A., & de Vos, K. (1988). The Definition and Measurement of Poverty. *The Journal of Human Resources*, *23*(2), 211–221. https://doi.org/10.2307/145776

Hamilton, R. W., Mittal, C., Shah, A., Thompson, D. V., & Griskevicius, V. (2019). How Financial Constraints Influence Consumer Behavior: An Integrative Framework. *Journal of Consumer Psychology*, *29*(2), 285–305. https://doi.org/10.1002/jcpy.1074

Haushofer, J., & Salicath, D. (2023). The Psychology of Poverty: Where Do We Stand? *Social Philosophy and Policy*, *40*(1), 150–184. https://doi.org/10.1017/S0265052523000419

Helwig, N. E. (2017). Adding bias to reduce variance in psychological results: A tutorial on penalized regression. *The Quantitative Methods for Psychology*, *13*(1), 1–19. https://doi.org/10.20982/tqmp.13.1.p001

Hurley, R. L. (1969). *Poverty and Mental Retardation: A Causal Relationship*.

Jaroszewicz, A., Jachimowicz, J., Hauser, O., & Jamison, J. (2024). How effective is (more) money? Randomizing unconditional cash transfer amounts in the US. *Randomizing Unconditional Cash Transfer Amounts in the US (July 20, 2024)*.

Jeffreys, S. H. (1961). *The Theory of Probability* (3rd ed.). Oxford University Press.

Kansikas, C., Mani, A., & Niehaus, P. (2023). *Evaluating Alternative Cash Transfer Designs in Kenya Using Behavioral Economics*.

Kaplan, G. A., Turrell, G., Lynch, J. W., Everson, S. A., Helkala, E.-L., & Salonen, J. T. (2001). Childhood socioeconomic position and cognitive function in adulthood. *International Journal of Epidemiology*, *30*(2), 256–263. https://doi.org/10.1093/ije/30.2.256

Karlamangla, A. S., Miller-Martinez, D., Aneshensel, C. S., Seeman, T. E., Wight, R. G., & Chodosh, J. (2009). Trajectories of cognitive function in late life in the United States: Demographic and socioeconomic predictors. *American Journal of Epidemiology*, *170*(3), 331–342. https://doi.org/10.1093/aje/kwp154

Kaur, S., Mullainathan, S., Oh, S., & Schilbach, F. (2025). Do financial concerns make workers less productive? *The Quarterly Journal of Economics*, *140*(1), 635–689. https://doi.org/10.1093/qje/qjae038

Keysers, C., Gazzola, V., & Wagenmakers, E.-J. (2020). Using Bayes factor hypothesis testing in neuroscience to establish evidence of absence. *Nature Neuroscience*, *23*(7), Article 7. https://doi.org/10.1038/s41593-020-0660-4

Kim, J., Sorhaindo, B., & Garman, E. T. (2006). Relationship between financial stress and workplace absenteeism of credit counseling clients. *Journal of Family and Economic Issues*, *27*, 458–478. https://doi.org/10.1007/s10834-006-9024-9

Lichand, G., & Mani, A. (2020). *Cognitive droughts*. https://doi.org/10.5167/UZH-185364

Lim, J., & Dinges, D. F. (2010). A Meta-Analysis of the Impact of Short-Term Sleep Deprivation on Cognitive Variables. *Psychological Bulletin*, *136*(3), 375–389. https://doi.org/10.1037/a0018883

Lüdecke, D. (2016). *esc: Effect Size Computation for Meta Analysis* (0.5.1) [Dataset]. https://doi.org/10.32614/CRAN.package.esc

Lynch, J. W., Kaplan, G. A., & Shema, S. J. (1997). Cumulative impact of sustained economic hardship on physical, cognitive, psychological, and social functioning. *New England Journal of Medicine*, *337*(26), 1889–1895. https://doi.org/10.1056/NEJM199712253372606

Maier, M., Bartoš, F., & Wagenmakers, E.-J. (2023). Robust Bayesian meta-analysis: Addressing publication bias with model-averaging. *Psychological Methods*, *28*(1), 107–122. https://doi.org/10.1037/met0000405

Mani, A., Mullainathan, S., Shafir, E., & Zhao, J. (2013). Poverty Impedes Cognitive Function. *Science*, *341*(6149), 976–980. https://doi.org/10.1126/science.1238041

Mani, A., Mullainathan, S., Shafir, E., & Zhao, J. (2020). Scarcity and cognitive function around payday: A conceptual and empirical analysis. *Journal of the Association for Consumer Research*, *5*(4), 365–376. https://doi.org/10.1086/709885

Moher, D., Shamseer, L., Clarke, M., Ghersi, D., Liberati, A., Petticrew, M., Shekelle, P., Stewart, L. A., & PRISMA-P Group. (2015). Preferred reporting items for systematic review and meta-analysis protocols (PRISMA-P) 2015 statement. *Systematic Reviews*, *4*(1), 1. https://doi.org/10.1186/2046-4053-4-1

Mullainathan, S., & Shafir, E. (2013). *Scarcity: Why having too little means so much*. Macmillan.

Neal, R. D., Lawlor, D. A., Allgar, V., Colledge, M., Ali, S., Hassey, A., Portz, C., & Wilson, A. (2001). Missed appointments in general practice: Retrospective data analysis from four practices. *British Journal of General Practice*, *51*(471), 830–832.

Oasis, K.-T., & Remy, B.-L. (2014). *Poverty and intelligence: Evidence using quantile regression*.

Ong, Q., Theseira, W., & Ng, I. Y. (2019). Reducing debt improves psychological functioning and changes decision-making in the poor. *Proceedings of the National Academy of Sciences*, *116*(15), 7244–7249. https://doi.org/10.1073/pnas.1810901116

Pande, S. (2023). *Decision Making Under Scarcity: An Inquiry into The Effects of Cognitive Load* [PhD Thesis]. University of East Anglia.

Rathbone, M., Parkinson, W., Rehman, Y., Jiang, S., Bhandari, M., & Kumbhare, D. (2016). Magnitude and variability of effect sizes for the associations between chronic pain and cognitive test performances: A meta-analysis. *British Journal of Pain*, *10*(3), 141–155. https://doi.org/10.1177/2049463716642600

Ridley, M., Rao, G., Schilbach, F., & Patel, V. (2020). Poverty, depression, and anxiety: Causal evidence and mechanisms. *Science*, *370*(6522), eaay0214. https://doi.org/10.1126/science.aay0214

Schoon, I., Jones, E., Cheng, H., & Maughan, B. (2012). Family hardship, family instability, and cognitive development. *J Epidemiol Community Health*, *66*(8), 716–722. https://doi.org/10.1136/jech.2010.121228

Shah, A. K., Mullainathan, S., & Shafir, E. (2012). Some Consequences of Having Too Little. *Science*, *338*(6107), 682–685. https://doi.org/10.1126/science.1222426

Shah, A. K., Shafir, E., & Mullainathan, S. (2015). Scarcity Frames Value. *Psychological Science*, *26*(4), 402–412. https://doi.org/10.1177/0956797614563958

Siddiqui, F., Salam, R. A., Lassi, Z. S., & Das, J. K. (2020). The Intertwined Relationship Between Malnutrition and Poverty. *Frontiers in Public Health*, *8*. https://doi.org/10.3389/fpubh.2020.00453

Smith, J. R., Brooks-Gunn, J., ... Klebanov, P. K. (1997). Consequences of living in poverty for young children's cognitive and verbal ability and early school achievement. *Consequences of Growing up Poor*, *132*, 189.

Steegen, S., Tuerlinckx, F., Gelman, A., & Vanpaemel, W. (2016). Increasing Transparency Through a Multiverse Analysis. *Perspectives on Psychological Science*, *11*(5), 702–712. https://doi.org/10.1177/1745691616658637

Szaszi, B., Szecsi, P., Palfi, B., Neszveda, G., Blattman, C., Jamison, J., Taka, A., & Sheridan, M. (2022). Does alleviating poverty increase cognitive performance? Short and long term evidence from a randomized controlled trial. *PsyArXiv*. https://doi.org/10.31234/osf.io/4gyzh

Wicherts, J. M., & Scholten, A. Z. (2013). Comment on "Poverty Impedes Cognitive Function." *Science*, *342*(6163), 1169–1169. https://doi.org/10.1126/science.1246680

Zhao, J., & Tomm, B. M. (2018). Psychological Responses to Scarcity. *Oxford Research Encyclopedia of Psychology*. https://doi.org/10.1093/acrefore/9780190236557.013.41

Supplementary Materials

Supplementary Materials

Download: https://collabra.scholasticahq.com/article/147236-the-impact-of-financial-resources-on-cognitive-performance-a-systematic-review-and-meta-analysis/attachment/310080.docx?auth_token=C4EMIBPS-S9dUU3GmZ5U

Peer Review Communication

Download: https://collabra.scholasticahq.com/article/147236-the-impact-of-financial-resources-on-cognitive-performance-a-systematic-review-and-meta-analysis/attachment/310081.docx?auth_token=C4EMIBPS-S9dUU3GmZ5U