Coefficients to be Confirmed: LLM-Automated Social Science Replication

So Kubota[1], Hiromu Yakura[2], Samuel Coavoux[3], Sho Yamada[4], and Yuki Nakamura[5]

[1]Graduate School of Economics, Tohoku University

[2]Max Planck Institute for Human Development

[3]ENSAE, CREST

[4]Graduate School of Public Policy, The University of Tokyo

[5]Faculty of Engineering, The University of Tokyo

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Abstract

While large language models (LLMs) have accelerated scientific production by streamlining writing, coding, and reviewing, the cost of verifying published results remains persistently high, contributing to what is known as the "replication crisis." We propose enhancing reproducibility through LLM-automated verification and present a working prototype that automatically reproduces statistical analyses in social science. Quantitative social science is particularly well-suited to automation because it relies on standardized statistical models, shared public datasets, and uniform presentation formats such as regression tables and summary statistics. We introduce a method that iterates LLM-based text interpretation, code generation, execution, and discrepancy analysis, and we demonstrate its capabilities by reproducing key results from a seminal sociology paper. We also outline deployment scenarios including pre-submission checks, peer-review support, and meta-scientific audits, positioning AI verification as assistive infrastructure that strengthens research integrity.[1]

1. Introduction

Large language models (LLMs) are rapidly changing how scientists produce research. They now assist with literature screening, survey design, data cleaning, code generation, and even peer review (Gilardi et al., 2023; Chen et al., 2021; Du et al., 2025; Conroy, 2023; Hosseini et al., 2025). Agentic systems extend these capabilities by chaining retrieval, planning, and execution, enabling models to run analyses, generate figures, and iteratively revise outputs (Wang et al., 2024; Yao et al., 2023; Wang et al., 2025). Researchers have also begun using LLMs as simulated participants

[1]Code and replication outputs are available at https://github.com/kubotaso/AI_Social_Replication. We thank Chishio Furukawa for helpful comments. This work was supported by JST CREST Grant JPMJPR2364.

in experiments and as components in measurement workflows (Aher et al., 2023; Argyle et al., 2023; Brand et al., 2023; Park et al., 2023); in the natural sciences, related systems can design experimental procedures and interface with lab automation (Bran et al., 2024). Recent work has even demonstrated "AI Scientists" that autonomously generate research ideas and draft papers (Lu et al., 2024).

This acceleration promises major productivity gains, but it also creates a verification bottleneck. If AI reduces the cost of producing papers faster than it reduces the cost of checking them, the supply of scientific claims may outpace what the research community can credibly evaluate, threatening scientific integrity (Ioannidis, 2005; Naudet et al., 2018). This asymmetry amplifies what is often framed as a "replication crisis" (Open Science Collaboration, 2015; Freese and Peterson, 2017).

Social science faces closely related problems, now documented across economics, political science, psychology, and sociology (Camerer et al., 2016, 2018; Brodeur et al., 2024b; Open Science Collaboration, 2015; Freese and Peterson, 2017). In psychology, large multi-laboratory projects show that some influential findings replicate robustly while others fail, often with substantial heterogeneity across settings (Klein et al., 2014, 2018; Hagger et al., 2016). In economics, replication has sometimes surfaced problems in papers that were both widely cited and politically salient, including the Reinhart and Rogoff austerity debate and more recent controversy around an MIT-linked AI productivity study (Reinhart and Rogoff, 2010; Herndon et al., 2014; Toner-Rodgers, 2024).

These problems persist in part because verification produces public goods while imposing private costs. Replication attempts can be time-consuming, hard to publish, and professionally risky (Vanpaemel et al., 2015; Freedman et al., 2015; Gherghina and Katsanidou, 2013). Editorial policies requiring data and code have improved availability, but compliance and usability vary widely, and the remaining effort to run and understand another team's workflow is still substantial (Trisovic et al., 2022; Hardwicke et al., 2018). Even reproducing a published table from the same data can fail due to missing code, ambiguous documentation, proprietary data, or brittle software environments (Peng, 2011; Sandve et al., 2013; Goodman et al., 2016). As a result, producing a complex analysis is often easier than checking whether it actually runs.

This paper proposes using LLMs not to write papers but to check them. We develop an *AI-verifier* that, given a published article, its dataset, and a codebook, translates the methods section into executable code, runs that code, and compares outputs to the published tables and figures. When results differ, the system generates a discrepancy report and iteratively debugs the code. We demonstrate the approach with a case study replicating a seminal statistical analysis in sociology, Bryson (1996), using data from the General Social Survey. The value lies not only in successful reproduction but also in informative failure: when the system cannot match published outputs, it surfaces both errors in statistical code implementation and underspecified elements in the main article (such as undocumented preprocessing steps, ambiguous variable definitions), all of which make independent verification difficult.

Quantitative social science is particularly well-suited to automated verification. The field's reliance on standardized workflows, such as linear regression and its extensions, familiar covariates

from public individual/household surveys, and conventional reporting formats like summary and regression tables, makes statistical analyses legible to machines. More importantly, this standardization potentially enables verification at scale: the same automated logic that checks one paper can be applied to hundreds or thousands.

The remainder of this paper proceeds as follows. We clarify the conceptual boundary between computational reproducibility and replicability, using case studies to illustrate where LLM verification can help and where it cannot. We then describe the design of our automated system, which integrates large language models with a code-execution sandbox to iteratively translate methods text into executable code. Next, we present the full case study replicating Bryson (1996). We conclude by assessing current technical limitations, outlining deployment scenarios for authors and journals, and discussing how verification infrastructure can reshape scientific incentives.

2. Background: The Reproducibility Crisis and AI Capabilities

Discussions of the "reproducibility crisis" often conflate failures that have different causes and require different solutions. Following Goodman et al. (2016) and the National Academies report (National Academies of Sciences, Engineering, and Medicine, 2019), we distinguish between *replicability* and *computational reproducibility*. Replicability asks whether a finding holds when researchers collect new data, perhaps in different populations or settings. Computational reproducibility asks a simpler question: given the *same* data and the same procedures, can other researchers get the same results?

Freese and Peterson (2017) offer a finer breakdown for quantitative social science, identifying four types of replication:

- **Verifiability:** Checking whether reported results can be reproduced from the same data and code. This matches computational reproducibility in Goodman et al. (2016)'s terms.
- **Robustness:** Testing whether findings hold under different analytic choices.
- **Repeatability:** Collecting new data using the original procedures to see whether the effect reappears.
- **Generalizability:** Testing whether similar findings appear with different methods or in different settings.

Each type faces different challenges. *Verifiability* failures usually stem from everyday technical problems: missing or incomplete code, unclear data-cleaning steps, undocumented software settings, changing software versions, or simple coding errors (Peng, 2011; Sandve et al., 2013; Collberg and Proebsting, 2016). Fixing these problems requires better infrastructure, incentives, and tools. *Robustness* failures happen when results depend heavily on particular analytic choices (the many small decisions researchers make that may not be fully reported) (Gelman and Loken, 2013; Simonsohn et al., 2014). *Repeatability* failures often reflect studies with too few subjects or publication bias that inflates early effect estimates (Ioannidis, 2005; John et al., 2012). *Generalizability* failures

occur when findings do not hold for new populations, time periods, or measurement approaches. Solutions for these last three types focus on better study design, stronger theory, preregistration, and coordinated replication projects.

Data fabrication. At the extreme end of replicability (or repeatability) failures lies data fabrication. High-profile cases span many fields, from Hwang Woo-suk's fabricated human stem-cell results to Jan Hendrik Schön's anomalous measurement patterns in condensed-matter physics (Campos-Varela and Ruano-Raviña, 2019a,b). Indeed, replication attempts fail trivially if the underlying data are fabricated. Social science is not exempt. The widely cited study on honesty and signing at the beginning of forms (Shu et al., 2012) was later retracted after forensic analysis by Data Colada reported statistical anomalies, including implausibly uniform distributions and duplication artifacts, in one dataset (Data Colada, 2021). The Diederik Stapel affair (Levelt et al., 2012) and recent concerns about AI productivity experiments (Toner-Rodgers, 2024) similarly involve data whose regularities are difficult to reconcile with plausible data-generating processes.

These fabrication cases are conceptually important because they clarify a boundary: even flawless code cannot rescue invalid data. Forensic methods can sometimes flag suspicious patterns, but they are inherently limited. In psychology, tools such as GRIM and SPRITE evaluate whether reported means and related summary statistics are arithmetically compatible with integer-valued data (Brown and Heathers, 2017; Heathers et al., 2018). In microbiology and related fields, image-integrity systems such as Proofig AI and Imagetwin use computer vision to detect duplicated or manipulated blots and microscopy images (Proofig Ltd., 2024; Imagetwin, 2024). These approaches can highlight internal inconsistencies or apparent manipulation, yet they cannot verify that data collection occurred as described.

Verifiability failures. This paper focuses on verifiability (computational reproducibility failures where the data are, at least in principle, genuine) when the reported results cannot be regenerated from the shared materials. Survey evidence suggests that such failures are common and that confidence in the reproducibility of published work is low (Baker, 2016). In biomedicine, concerns about irreproducible preclinical findings have motivated systematic replication projects and methodological reforms (Begley and Ellis, 2012; Errington et al., 2021). A vivid illustration comes from Baggerly and Coombes (2009), who reconstructed the computational methods behind influential gene-expression signatures intended to guide cancer treatment. Their reanalysis uncovered misaligned samples, mislabeled arrays, and other data-handling errors that invalidated key conclusions and, in some cases, posed direct risks to patients. More broadly, empirical audits across fields show how frequently shared artifacts fail to run or fail to support published analyses (Collberg and Proebsting, 2016; Trisovic et al., 2022; Hardwicke et al., 2018).

Verifiability failures are especially consequential in empirical social science research, where quantitative results routinely inform policy debates and public narratives. The Reinhart–Rogoff paper on public debt and economic growth (Reinhart and Rogoff, 2010) became central to austerity debates by claiming that debt-to-GDP ratios above 90% are associated with sharply lower growth.

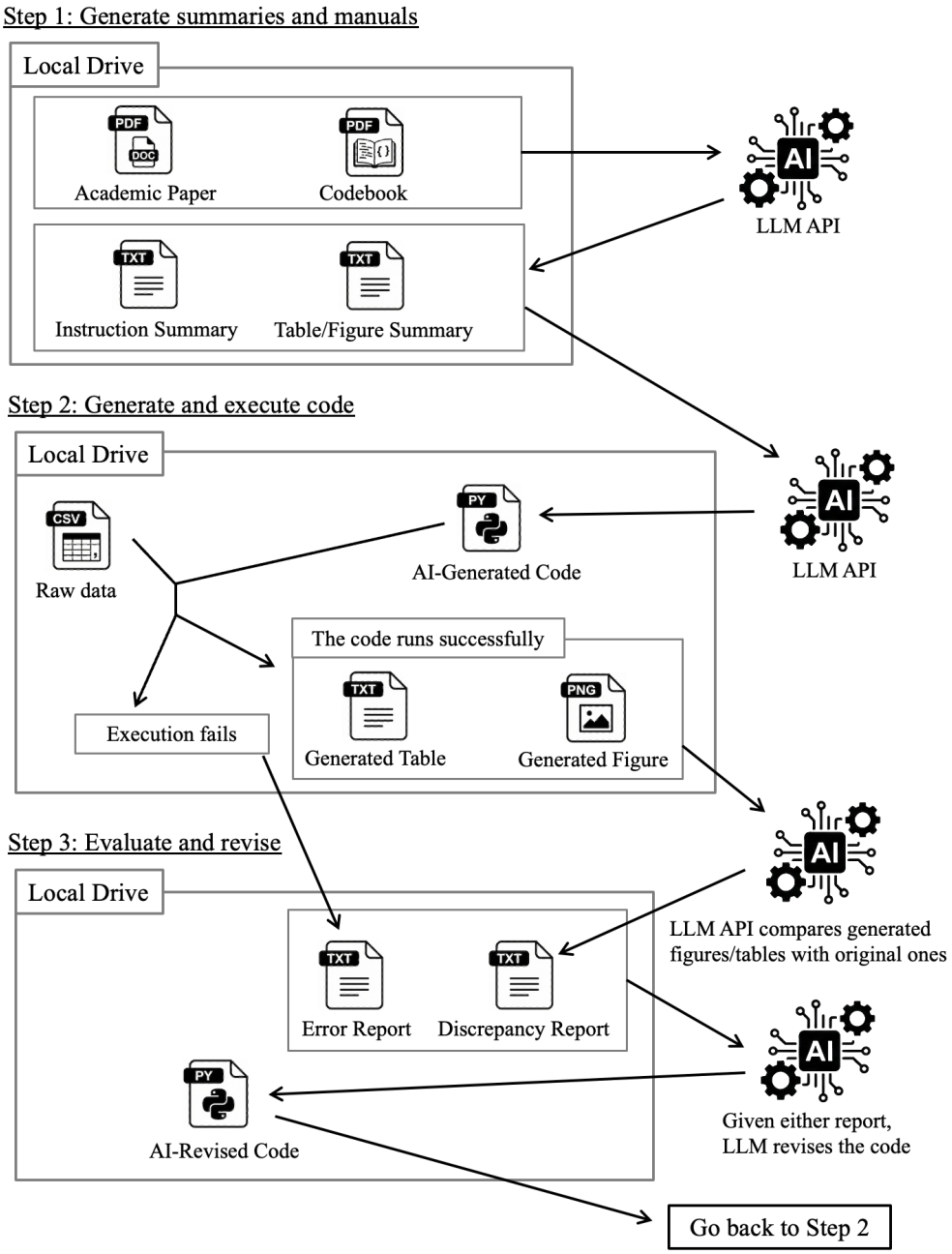
A graduate-student replication by Herndon et al. (2014) revealed that the headline conclusion depended on a spreadsheet error, selective exclusion of available observations, and unconventional weighting of country-year data; correcting these issues substantially weakened the result. The influential abortion–crime hypothesis (Donohue III and Levitt, 2001) likewise faced serious challenges when Foote and Goetz (2008) identified coding errors and specification choices that attenuated the estimated effect. In development economics, the colonial-origins literature (Acemoglu et al., 2001) provoked methodological criticism when Albouy (2012) showed that core results were sensitive to how historical variables were coded and which colonies were included. Large, coordinated replication efforts in psychology also reveal heterogeneous replication rates and substantial variation in effect sizes across labs (Open Science Collaboration, 2015; Klein et al., 2014).

Why LLM for social science? Quantitative social science offers an ideal environment for automated verification because applied work across economics, political science, sociology, and related disciplines follows standardized patterns. First, researchers rely on widely shared datasets, such as public opinion surveys (GSS, WVS, ANES), census and demographic data (Population Census, DHS), and household surveys (CPS, ATUS), with organized data structures and comprehensive documentation. These data are also accessible through public data dissemination systems, for example, IPUMS and the GSS Data Explorer. Second, analyses employ conventional, explicitly specified models: ordinary least squares, logit and probit models, and other generalized linear models. Third, results appear in canonical formats, such as regression tables with coefficients and standard errors, summary statistics panels, and standardized figures, that machines can systematically parse and verify.

These features substantially reduce the semantic gap between natural-language methods descriptions and executable workflows. The remaining ambiguities (missing-value treatment, sample restrictions, variable transformations) are precisely the tacit choices that cause reproducibility failures. An LLM verification system can make these ambiguities explicit by attempting execution and reporting discrepancies. This approach is feasible because modern large language models are increasingly competent at reading and writing statistical code and at repairing code when given concrete execution errors (Chen et al., 2021; Jimenez et al., 2024; Zhou et al., 2024). They can also act as flexible interpreters between modalities, mapping method descriptions to variable definitions and synthesizing step-by-step execution plans (Yao et al., 2023; Wang et al., 2025). Compared to data fabrication, computational reproducibility failures are both more common and more tractable: they arise from incomplete specification and technical brittleness rather than deliberate deception. This motivates a verifier that attempts to rerun analyses, records where and how execution fails, and produces structured discrepancy reports anchored in observable outputs.

Although the conceptual distinction between replicability and computational reproducibility is important, the terms *replication*, *replicability*, and *reproducibility* are often used inconsistently across fields (Rougier et al., 2017; Plesser, 2018). In this article, we use "replication" in its broader, colloquial sense to encompass computational reproduction, relying on context to preserve the narrower distinctions when needed.

Figure 1: Overview of the Automated Replication Procedure



3. System Design: An Automated Replication System

We developed a prototype method to test whether fully automated replication is possible. Unlike simple bots that run a script once, our system uses an agentic workflow that mimics a researcher who plans, codes, checks their work, and fixes errors.

Figure 1 summarizes the proposed automated replication procedure as a three-step loop: (1)

generate a structured replication specification from the paper and codebook, (2) generate and execute analysis code against the local dataset, and (3) evaluate and revise using machine-generated diagnostics until the regenerated outputs match the paper. The entire process consists of repeated interactions between the local code execution environment and the LLM API for code and report generation.

Step 1: Generate summaries and manuals The whole process is written in a single Python script. For convenience, we refer to this script as the *Main Script* running on a local computer. The Main Script's directory also houses three primary inputs: (i) the target academic article (typically a PDF), (ii) a dataset codebook or variable documentation, and (iii) the locally stored replication dataset (in formats such as CSV, R, Stata, or SPSS). In Step 1, the Main Script converts these unstructured research materials into a structured replication roadmap. It sends these materials to the LLM API and receives two specification notes as text files.

The first is a *Table/Figure summary:* a compact, machine-readable representation of each target output, including which estimands appear (coefficients, standard errors, fit statistics), which variables are included, and how results are formatted. The second is a *Procedure manual:* a step-by-step plan describing the target analyses (models, samples, exclusions), the sequence of transformations, and the required outputs (e.g., "Table 1, Models 1–3"). These manuals are critical because, even when the statistical model is standard, details about missing-value handling, weighting, item coding, and index thresholds are often unclear until implementation attempts begin.

Step 2: Generate and execute code Next, the Main Script sends the two specification notes from Step 1 to the LLM API to generate executable analysis code that implements the statistical analysis using the local dataset. This LLM-generated code is written as a function (in Python, for this prototype) so that the Main Script can evaluate the derived outputs. The Main Script then runs this code in the local environment, accessing the raw data from the local drive. When the code runs successfully, it produces generated tables (as structured text) and figures (as image files). If execution fails, the Main Script captures the exception and stack trace, producing an *error report* that includes the failing module, the exception message, and relevant context (e.g., variable names present, dimensions of the current dataframe).

Step 3: Evaluate and revise When execution in Step 2 succeeds, the Main Script sends the generated tables and figures back to the LLM API for comparison with the original paper's outputs. The LLM API generates a *discrepancy report* detailing any differences between the regenerated outputs and the original paper. For regression tables, the report records (when available) sample size N, coefficient estimates, standard errors, goodness-of-fit statistics such as R^2, and whether specifications include intended covariates. For figures, the LLM API interprets underlying numeric values in the visual data (e.g., data points in scatterplots, bar heights in bar charts) and compares them to the regenerated values.

Finally, the Main Script sends either a discrepancy report or an error report back to the LLM

API, which produces revised executable code addressing the issues raised. The loop returns to Step 2, where the Main Script executes the revised code and evaluates the outputs again. The loop terminates when the LLM API determines that the outputs match the original paper sufficiently closely or when a maximum number of iterations is reached.

4. Case Study: Replicating Bryson (1996)

We evaluate feasibility using Bryson (1996), a classic sociology paper on musical dislikes and symbolic exclusion, using the 1993 General Social Survey. This paper is representative of a common empirical pattern in social science: it relies on a widely used shared dataset, applies standard regression models, and constructs non-trivial variables (including multi-item indices) that require careful interpretation of documentation. Bryson's main results include regression tables with multiple specifications and a figure that effectively repeats a similar model across many music genres and then aggregates the results into a comparative visual summary. From the perspective of Figure 1, the task stresses all three stages: extracting a correct target specification, compiling it into code that runs, and reconciling discrepancies that arise from underspecified preprocessing and variable construction.

In Step 1, we place three files on the local drive: (i) the Bryson paper PDF, (ii) the GSS 1993 data file (CSV format), and (iii) the relevant GSS codebook documentation (PDF). We use the GPT-5 API (ChatGPT 5) for all LLM interactions. The full GSS 1993 dataset is too large and exceeds current LLM input size limits, so we create a compact replication dataset and a corresponding codebook (in CSV format) containing only the variables required for the replication. We use R's gssr package (Healy, 2024) for this preprocessing step.

The Bryson paper contains three tables and one figure. We write separate and independent Python programs for each target. Consider Table 1 as an example. It reports standardized OLS coefficients from three nested specifications predicting musical exclusiveness (the count of music genres a respondent dislikes) from socioeconomic status (SES), demographics, and political intolerance. The dependent variable is constructed from 18 GSS items asking respondents to rate genres on a five-point scale; responses of "dislike" or "dislike very much" count toward the index, while "don't know" responses are treated as missing (dropping the respondent from the analysis). Model 1 includes only SES variables (education, household income per capita, occupational prestige); Model 2 adds demographic controls (gender, age, race/ethnicity indicators, religion indicators, region); Model 3 adds a political intolerance scale constructed from 15 Stouffer-style civil-liberties items asked of two-thirds of the sample.

The LLM-generated Table/Figure summary extracts the information visible in the published output: it records the dependent variable definition, lists each independent variable for all three specifications, and transcribes the reported coefficients, R^2 values, significance markers, and sample sizes for each model. The Procedure Manual, by contrast, maps these analysis concepts to specific GSS variable names and recoding rules derived from the codebook. It specifies, for example, that musical exclusiveness equals the sum of 18 binary indicators (one per genre, coded 1 if the response

is 4 or 5 on the Likert scale). The Procedure Manual also specifies sample restrictions (year equals 1993, listwise deletion by model) and the standardization procedure required to obtain beta coefficients.

Table 1: Original vs. LLM-Generated Table 1 from Bryson (1996)

	Model 1: SES		**Model 2: Demographic**		**Model 3: Intolerance**	
Variable	**Original**	**LLM**	**Original**	**LLM**	**Original**	**LLM**
Education	-0.322***	-0.330***	-0.246***	-0.284***	-0.151***	-0.191**
Income (Per Capita)	-0.016	-0.034	-0.038	-0.059	-0.021	-0.026
Occ. Prestige	0.009	0.029	-0.002	0.011	-0.038	-0.024
Female	–	–	-0.106**	-0.094*	-0.126***	-0.114*
Age	–	–	0.129***	0.125**	0.093*	0.081
Race (Ref: White)						
Black	–	–	0.037	0.034	0.041	0.045
Hispanic	–	–	-0.037	-0.047	-0.043	-0.039
Other	–	–	0.010	0.008	0.014	0.012
Religion/Region						
Cons. Protestant	–	–	0.038	0.043	0.014	0.018
No Religion	–	–	-0.025	-0.016	-0.011	-0.009
Southern	–	–	0.076*	0.079*	0.085*	0.082*
Political Intolerance	–	–	–	–	0.231***	0.211***
Model Statistics						
Sample Size (N)	787	793	647	651	353	370
R^2	0.10	0.108	0.14	0.152	0.16	0.167
Constant	11.235	10.833	8.941	9.155	7.744	7.930

Note: Table reports standardized coefficients. Significance: *** $p < .001$, ** $p < .01$, * $p < .05$.

In our execution with the GPT-5 API, the system iterated 100 times through Steps 2 and 3. Although it did not converge to fully reproduce the exact numbers in Table 1, the program nevertheless replicated the table at an acceptable level. Of 100 trials, 72 successfully generated the table, while 28 attempts stopped due to runtime errors in Step 2. Interestingly, the program generated reasonable results for the first and second specifications on the second trial. After that, the program attempted minor improvements to these specifications but struggled with the third specification, particularly with constructing the Political Intolerance variable correctly. The discrepancy reports primarily identified inconsistencies in the number of observations, statistical significance levels, and model fit. We also obtained good results for Tables 2 and 3; see the Appendix for details.

We also test the image handling capabilities of this LLM replication by replicating Figure 1. This figure plots logistic regression coefficients for musical tolerance and overlays average education levels for each genre's audience. The Procedure Manual is similar to those for tables, but the Table/Figure summary adds the figure structure in addition to the statistical models. The figure structure describes visual properties such as the labels and scales of the axes, the properties of lines, and the annotation texts with arrows. Our LLM system perfectly replicates the figure in terms

Figure 2: Original Figure 1 vs. LLM-Generated Replication

Original Figure 1 from Bryson (1996)

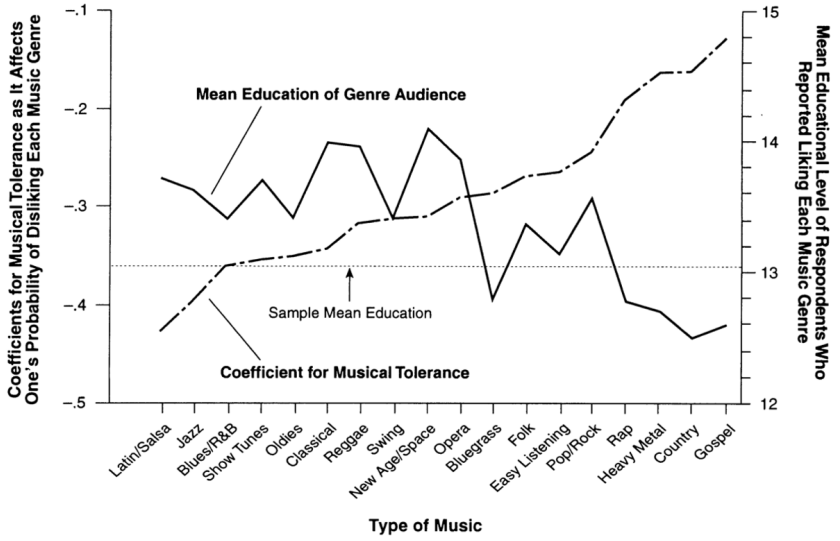
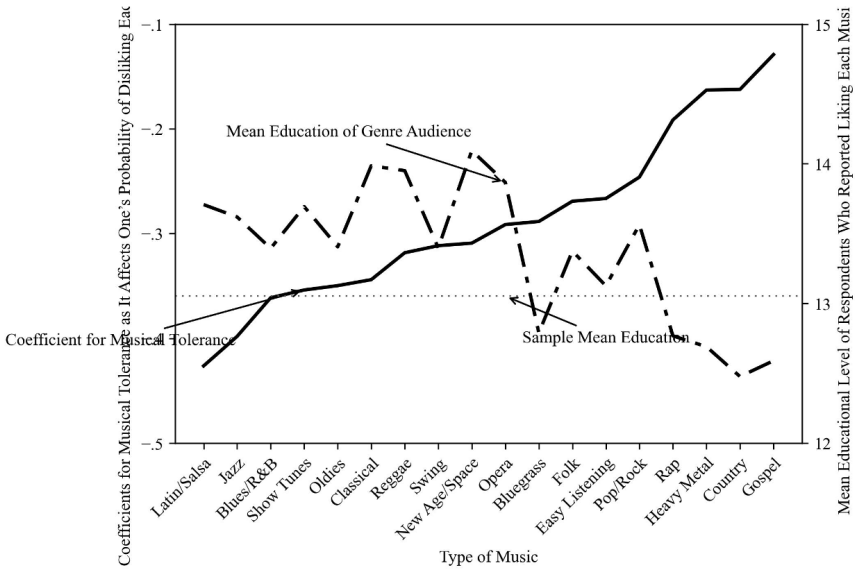


Figure 1. The Effect of Being Musically Tolerant on Disliking Each Music Genre Compared to the Educational Composition of Genre Audiences

AI-Generated Figure



of statistical results: the plotted lines match the original figure exactly. The visual properties are somewhat limited: the text locations are mismatched, the labels are too long, and the line properties are flipped. Although these failures are repeatedly pointed out in the discrepancy reports, the system could not fix them. Since these are minor cosmetic issues, we conclude that our method is capable of statistical plot replication.

Overall, the exercise of Bryson (1996) stresses the system's ability to interpret methods text,

map concepts to variable definitions, handle non-trivial variable construction (multi-item indices), and resolve ambiguities through iterative debugging. The system successfully navigates these challenges to produce results that closely match the published outputs.

5. Current Capabilities and Limitations

The exercise of Bryson (1996) illustrates that LLM-assisted verification is feasible for replicating major statistical results common in social science papers: summary tables of data, regression tables, and figures. We emphasize that while this is a preliminary step, it is applicable to a wide range of empirical social science papers. The data source, GSS, is a standard public microdata source sharing a basic structure common to many other social science datasets, such as WVS, CPS, and ANES. The statistical models (ordinary least squares and logistic regression) are canonical in applied social science. Because our AI-driven method builds on techniques that constitute the empirical backbone of quantitative social science, the approach demonstrated here is inherently scalable.

Technical limitations. However, several limitations remain. First, the LLM-driven system struggles with *underspecified methodological details*. For example, in Bryson (1996), the coding of the Hispanic variable is not explicitly described. It is likely created from an ethnic variable about the country of family origin; however, the paper does not specify which countries are treated as Hispanic. Second, our prototype currently operates on *single rectangular datasets*, whereas many research designs require merging multiple data sources or linking distinct survey waves. For example, many studies using WVS create country-level variables as the mean of individual-level responses within each country and merge them with external macroeconomic or demographic data. Such multi-dataset workflows introduce additional complexities in data integration, matching logic, and provenance tracking that are not yet addressed in the current system. Third, the present workflow is *optimized for cross-sectional data*; adapting it to panel data would introduce complexities related to time-series structure and repeated measures that are not yet fully addressed. Analyzing Panel Study of Income Dynamics (PSID) or National Longitudinal Survey of Youth (NLSY) data would be a future challenge. Fourth, our prototype focuses on *canonical statistical models* (OLS, logit, probit, and their extensions); extending it to handle more complex models (e.g., hierarchical models, structural equation models, machine learning algorithms) would require additional development. Finally, while the system can identify discrepancies in outputs, it does not yet provide detailed diagnostics or explanations for why mismatches occur, limiting its utility for debugging complex workflows.

Epistemic boundary of LLM-assisted reproducibility. Aside from these technical limitations, it is more important to clarify the epistemic boundary of LLM-assisted reproducibility. If this system succeeds in reproducing published results, there are two possibilities: the LLM system replicates by following the main text, or it engages in a trial-and-error process that deviates from the workflow in the main text but still arrives at the results. The former is ideal, but the latter is

also valuable because it reveals underspecification in the original article. We also emphasize that even in the former case, successful reproduction does not guarantee the validity or ground truth of the scientific claim. It only verifies the procedural consistency given the dataset and chosen method within the paper's scope. Large-scale replication projects have shown that many influential results fail to replicate when new data are collected, even when the original analysis was computationally sound (Open Science Collaboration, 2015; Klein et al., 2018; Camerer et al., 2016). This distinction is particularly relevant for archival data research. Delios et al. (2022) attempted to replicate findings from 110 strategic management papers using archival sources; they found that while many results could be computationally reproduced, their generalizability to extended time periods or alternative specifications was much lower. Our position is that computational reproducibility is not the endpoint of scientific evaluation, but it is a necessary prerequisite for credible debate. An AI verifier should be understood as assistive infrastructure that checks mechanical consistency rather than as an arbiter of scientific truth (Freese and Peterson, 2017).

The meaning of a failed reproduction is more nuanced, and does not automatically imply that the original finding is wrong. Reproduction failures can arise for many reasons, and the most common one is the incapability of LLM. The technical limitations described above are still too substantial to overcome. In some cases, the failure simply reflects underspecification: the methods section omitted details about variable coding, sample restrictions, or estimation options that were obvious to the original authors but not recoverable from the text.

However, some reproduction failures do signal deeper issues. High-profile cases such as the Reinhart–Rogoff spreadsheet error (Herndon et al., 2014) show that seemingly minor computational errors can alter substantive conclusions. When an LLM verifier fails to reproduce a result and the discrepancy persists across multiple debugging iterations, this should trigger closer human investigation. The value of automated verification is not that it pronounces papers "correct" or "incorrect," but that it systematically surfaces inconsistencies that warrant further investigation. A discrepancy report becomes a starting point for dialogue: authors can clarify their workflow, reviewers can assess whether differences matter substantively, and readers can make informed judgments about how much weight to place on the findings.

In short, reproducibility is a reliability check, not a validity proof. The goal of LLM-assisted verification is to make the former cheaper and more routine, thereby freeing human attention for the latter.

6. Use Cases and Deployment Scenarios

If LLM-assisted reproducibility is to matter for scientific practice, it must fit into real workflows. We outline three deployment scenarios, each with distinct governance needs.

Local pre-submission checks by authors. The lowest-friction use is as a local "reproduce-and-compare" tool for authors. Authors run the system before submission to detect missing files, inconsistent seeds, or silent sample-size changes. If the LLM cannot reproduce results from the

manuscript and data, it signals that documentation is incomplete, allowing authors to fix ambiguities before peer review. This turns reproducibility into a pre-submission quality check rather than a post-publication crisis.

Institutional verification systems. The LLM-based verifier can also be integrated into institutional workflows. The most straightforward case is journal-run verification during peer review. AI systems have supported the peer review process, from screening for reporting quality to generating reviewer comments (Saito et al., 2024; Hosseini et al., 2025; Zhang et al., 2024). The LLM-based verifier adds one more layer to verify that the statistical procedure matches the main text. Academic journals increasingly require data and code, and some are experimenting with formal code review as part of peer review (Nature Human Behaviour Editorial, 2021). For example, the American Economic Association has built repositories and editorial processes that require replication packages (American Economic Association, 2024). However, this effort only confirms that the submitted program is runnable and produces the same results. The review process can focus on substance after the methodological correctness is confirmed by the LLM-based system. It helps reviewers focus on interpretation and scientific judgment.

This system can also be extended to a service that routinely attempts to reproduce published papers, creating a continuous audit of the scientific record. This aligns with recent proposals such as the "Replication Engine" by the Institute for Progress, which envisions AI agents automatically verifying results at the moment of publication (Institute for Progress, 2025). Such automated infrastructure complements the work of the Institute for Replication (I4R), which organizes large-scale human replication efforts and is increasingly moving toward routine checks (I4R, 2024). As Brodeur et al. (2024b) argue, institutionalizing these checks is critical to solving the supply problem of replication; automated tools can scale this institutional capacity by handling the mechanical verification tasks that currently bottleneck human replicators.

Forensic verification. A distinctive application of LLM-assisted verification is as a *forensic tool* for legacy research. The vast majority of social science papers published before the mid-2000s lack replication packages: the American Economic Association adopted its first data availability policy only in 2005, and most sociology journals have no such requirements even today (Freese and Peterson, 2017). A recent study of papers using the German Socio-Economic Panel found that only 6% have replication code available, with availability sharply lower for older publications (Fink et al., 2025). For influential papers from this era, the original code may be lost, stored on obsolete media, or written in software versions that no longer run. Yet many of these studies used publicly available datasets that remain accessible. An LLM verifier can attempt to reconstruct the analysis workflow from the methods section alone, generating code that approximates what the original authors likely ran.

In addition to recovering the past, this infrastructure serves as a forensic tool for disputed findings. When results are questioned, an automated reproduction attempt can quickly determine whether concerns are about simple rerun failures (missing code, wrong file versions) or about deeper

inconsistencies. When errors are suspected, as in the Reinhart-Rogoff case, automated tools can provide rapid forensics.

7. Conclusion

Large language models are transforming scientific production, creating a risk that the supply of plausible-sounding claims will outpace the community's capacity to verify them. This paper argues that the same technologies driving this acceleration can be harnessed to strengthen scientific integrity. We introduce an automated verification system that functions as a replication compiler, translating natural-language methods into executable code. By applying this system to a classic sociology study (Bryson, 1996), we demonstrated that current LLMs can successfully reproduce key statistical results from widely used public datasets, while also surfacing the ambiguities and tacit knowledge that often hinder human replication efforts.

Several technical directions appear promising. One is tighter integration with research repositories and data providers, including standardized metadata and executable environments. Another is extending the method to handle common but more complex structures (survey weights, panels, and multi-source merges) by combining LLMs with domain-specific templates. A third is community-driven libraries of procedure-manual patterns for canonical datasets, analogous to shared codebooks but focused on analysis recipes. Finally, as models improve, verifiers may become capable not only of reproducing tables, but also of checking robustness specifications and sensitivity analyses in a standardized way (Brodeur et al., 2024a,b).

Ultimately, because verification is a public good (Freedman et al., 2015), it should not be an act of heroism by individual researchers but a routine feature of the scientific infrastructure. If we can lower the cost of checking basic consistency, we free human attention for the deeper tasks of interpretation and theory building. By treating reproducibility as a machine-actionable property, we can ensure that the next era of quantitative social science, though faster and more automated, remains firmly grounded in verifiable evidence.

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A. Additional Tables

Table 2: Original vs. LLM-Generated Table 2 from Bryson (1996)

	Dislike of Rap, Reggae, Blues/R&B, Jazz, Gospel, Latin		Dislike of the 12 Remaining Genres	
Independent Variable	Original	LLM	Original	LLM
Racism score	.130**	.135***	.080	.012
Education	−.175***	−.188***	−.242***	−.254***
Household income per capita	−.037	−.034	−.065	−.095*
Occupational prestige	−.020	−.010	.005	.042
Female	−.057	−.056	−.070	−.056
Age	.163***	.165***	.126**	.083*
Black	−.132***	−.119**	.042	.118**
Hispanic	−.058	– (omitted)	−.029	– (omitted)
Other race	−.017	−.004	.047	.061
Conservative Protestant	.063	– (omitted)	.048	– (omitted)
No religion	.057	.033	.024	−.001
Southern	.024	.030	.069	.091*
Constant	2.415***	2.469***	7.860	5.597***
R^2	.145	.137	.147	.138
Adj. R^2	.129	.124	.130	.124
Number of cases	644	667	605	627

Notes: Original values are from Bryson (1996), Table excerpt shown in Figure screenshot. LLM (std) values are standardized coefficients from the automated replication output. Hispanic and Conservative Protestant were omitted because the coding rules for these variables were not explicitly specified in the original paper, preventing the system from constructing them reliably. $^{*}p < .05$, $^{**}p < .01$, $^{***}p < .001$ (two-tailed tests).

Table 3: LLM-Generated Replication of Table 3 from Bryson (1996)

	Attitude	Music Genre					
		Latin	Jazz	Blues/R&B	Show Tunes	Oldies	Classical
(1)	Like very much	85	254	221	235	405	281
(2)	Like it	325	540	669	562	688	478
(3)	Mixed feelings	416	393	367	369	213	371
(4)	Dislike it	403	297	220	281	172	263
(5)	Dislike very much	144	69	61	68	77	136
(M)	Don't know much about it	0	0	0	0	0	0
(M)	No answer	0	0	0	0	0	0
	Mean	3.14	2.61	2.50	2.59	2.25	2.67
		Reggae	Swing	New Age	Opera	Bluegrass	Folk
(1)	Like very much	84	269	48	73	145	130
(2)	Like it	362	588	186	257	562	553
(3)	Mixed feelings	340	290	269	359	411	472
(4)	Dislike it	297	230	429	515	255	274
(5)	Dislike very much	217	53	368	306	59	87
(M)	Don't know much about it	0	0	0	0	0	0
(M)	No answer	0	0	0	0	0	0
	Mean	3.15	2.45	3.68	3.48	2.67	2.76
		Easy Listen.	Pop/Rock	Rap	Heavy Metal	Country	Gospel
(1)	Like very much	251	206	44	48	385	356
(2)	Like it	698	645	159	123	592	571
(3)	Mixed feelings	323	296	284	189	364	364
(4)	Dislike it	200	245	433	400	167	197
(5)	Dislike very much	49	152	614	766	66	71
(M)	Don't know much about it	0	0	0	0	0	0
(M)	No answer	0	0	0	0	0	0
	Mean	2.41	2.67	3.92	4.12	2.32	2.39

Notes: Values are frequency counts from LLM-generated output. Mean is the average rating on the 1–5 scale where 1 = Like very much and 5 = Dislike very much. The categories "Don't know much about it" and "No answer" are not separately recorded in the available GSS 1993 data and are shown as 0.

Figure 3: Original Table 3 from Bryson (1996)

Table 3. Frequency Distributions for Attitude toward 18 Music Genres: General Social Survey, 1993

Attitude	Music Genre					
	Latin/Salsa	Jazz	Blues/ R&B	Show Tunes	Oldies	Classical/ Chamber
(1) Like very much	85	254	221	235	405	281
(2) Like it	325	540	669	562	688	478
(3) Mixed feelings	416	393	367	369	213	371
(4) Dislike it	403	297	220	281	172	263
(5) Dislike very much	144	69	61	68	77	136
(M) Don't know much about it	221	38	56	77	41	66
(M) No answer	12	15	12	14	10	11
Mean	3.14	2.61	2.50	2.59	2.25	2.67
	Reggae	Swing/ Big Band	New Age/ Space	Opera	Bluegrass	Folk
(1) Like very much	84	269	48	73	145	130
(2) Like it	362	588	186	257	562	553
(3) Mixed feelings	340	290	269	359	411	472
(4) Dislike it	297	230	429	515	255	274
(5) Dislike very much	217	53	368	306	59	87
(M) Don't know much about it	295	164	292	83	163	78
(M) No answer	11	12	14	13	11	12
Mean	3.15	2.45	3.68	3.48	2.67	2.76
	Easy Listening	Pop/ Contemporary Rock	Rap	Heavy Metal	Country/ Western	Gospel
(1) Like very much	251	206	44	48	385	356
(2) Like it	698	645	159	123	592	571
(3) Mixed feelings	323	296	284	189	364	364
(4) Dislike it	200	245	433	400	167	197
(5) Dislike very much	49	152	614	766	66	71
(M) Don't know much about it	72	50	61	70	22	35
(M) No answer	13	12	11	10	10	12
Mean	2.41	2.67	3.92	4.12	2.32	2.39