

A Report on “Immigration and Redistribution” by Alesina et al. (2023)

Reviewer 2

February 21, 2026

v1



isitcredible.com

Disclaimer

This report was generated by large language models, overseen by a human editor. It represents the honest opinion of The Catalogue of Errors Ltd, but its accuracy should be verified by a qualified expert. Comments can be made [here](#). Any errors in the report will be corrected in future revisions.

I am wiser than this person; for it is likely that neither of us knows anything fine and good, but he thinks he knows something when he does not know it, whereas I, just as I do not know, do not think I know, either. I seem, then, to be wiser than him in this small way, at least: that what I do not know, I do not think I know, either.

Plato, *The Apology of Socrates*, 21d

To err is human. All human knowledge is fallible and therefore uncertain. It follows that we must distinguish sharply between truth and certainty. That to err is human means not only that we must constantly struggle against error, but also that, even when we have taken the greatest care, we cannot be completely certain that we have not made a mistake.

Karl Popper, 'Knowledge and the Shaping of Reality'

Overview

Citation: Alesina, A., Miano, A., and Stantcheva, S. (2023). Immigration and Redistribution. *Review of Economic Studies*, Vol. 90, No. 1, pp. 1–39.

URL: <https://doi.org/10.1093/restud/rdac011>

Abstract Summary: This article investigates how people perceive immigrants and how these perceptions influence their support for redistribution through large-scale surveys and experiments in six countries. It finds striking misperceptions about the number and characteristics of immigrants, with respondents generally overestimating immigrant populations and perceiving them as more culturally distant and economically weaker than they actually are. The study demonstrates that simply making respondents think about immigration reduces support for redistribution, and that narratives about immigrants' work ethic can somewhat counteract this negative priming effect, while factual information has limited impact.

Key Methodology: Large-scale online surveys and experiments in six countries (France, Germany, Italy, Sweden, the UK, and the US) were used to elicit perceptions and attitudes. Randomized priming and video treatments were employed to test causal relationships between immigration perceptions and support for redistribution.

Research Question: Does immigration change support for redistribution?

Summary

Is It Credible?

This article presents a large-scale investigation into how citizens in six developed countries perceive immigrants and how these views influence their support for the welfare state. Based on surveys and experiments with roughly 24,000 respondents, Alesina et al. make several major claims. First, they argue that there are “striking misperceptions” about the number, origins, and economic characteristics of immigrants, with respondents vastly overestimating their size and reliance on government transfers (p. 1). Second, they claim a causal relationship where simply making immigration salient reduces support for redistribution. Finally, based on video treatments, they conclude that narratives shape people’s views “more deeply than hard facts,” as factual information failed to shift policy preferences while an anecdote about a hard-working immigrant had some effect (p. 1).

The descriptive finding that the public holds highly inaccurate views about immigrant populations is robust and constitutes a major contribution. However, the construct validity of these “misperceptions” is slightly complicated by the study’s strict parameters. The survey explicitly asks respondents to consider only “documented immigrants” and to exclude “illegal immigrants” (p. 7). Because the colloquial understanding of “immigrant” often blends documented, undocumented, and even second-generation individuals, the massive overestimations reported might partly reflect a definitional mismatch rather than pure ignorance. While the authors demonstrate that overestimations persist even when factoring in these broader categories, the deliberate exclusion of undocumented immigration limits the study’s ability to fully explain the “vitriolic debate” that motivates the article, as real-world political anxieties often center precisely on unauthorized entry. The causal claim that immigration salience inherently depresses support for redis-

tribution is intriguing but warrants careful interpretation. The researchers induced salience by randomizing the order of the survey blocks, finding that respondents who completed a lengthy “immigration block” before a “redistribution block” expressed lower support for redistributive policies. Because the immigration block requires respondents to navigate a cognitively demanding battery of estimation tasks, the subsequent drop in redistribution support might be partially driven by subtle cognitive depletion or frustration rather than pure topical salience, even though the authors conduct dedicated tests that rule out overt survey fatigue (pp. A-36–A-37). Furthermore, while the authors transparently report treatment effects on individual policy components—revealing, for instance, that salience decreases support for the social safety net but increases support for education spending—their subsequent use of a composite index for correlational analysis averages out these conflicting dynamics (p. 29). Combining these opposing movements into a single metric obscures the complex ways in which immigration concerns interact with different facets of public spending.

The conclusion that narratives outweigh hard facts is also drawn from an experimental design that leaves room for alternative explanations. The study compares dry, statistical videos about immigrant shares and origins against a highly personalized, emotional anecdote about a specific “hard-working” immigrant. The stronger performance of the narrative treatment may simply reflect the well-documented psychological power of emotional appeals over abstract data, rather than a universal rule about narratives versus facts. Additionally, the failure of the factual videos to shift policy preferences might not mean that facts are useless, but rather that respondents with strong priors simply did not find the provided information credible.

Finally, the study’s correlational finding that the perception of immigrants as “free-riders” is the strongest predictor of opposition to redistribution is valuable, but it struggles to rule out reverse causality. It is entirely possible that individuals with a pre-existing ideological opposition to the welfare state selectively adopt negative

stereotypes about immigrants to justify their broader political preferences. Similarly, questions designed to measure bias—such as asking whether a hypothetical immigrant receives more transfers than an identical native—might actually capture respondents' flawed assumptions about how welfare rules operate rather than personal animus.

The Bottom Line

The study successfully and rigorously documents widespread public inaccuracies regarding immigrant populations across six major democracies, providing a highly valuable descriptive map of public sentiment. However, its mechanistic and causal claims—specifically that immigration salience inherently depresses redistribution support and that narratives universally trump facts—are less secure due to potential confounding factors like survey fatigue, source credibility, and the emotional weight of the specific narrative used. Ultimately, while the foundational survey data is highly credible, the broader conclusions about how to shift public policy preferences should be interpreted with a degree of caution.

Potential Issues

Construct validity of “misperception”: The study’s central finding of “striking misperceptions” about immigrants may be partly influenced by a definitional mismatch between the survey’s prompt and the public’s colloquial understanding of “immigrant.” The survey explicitly instructs respondents to consider only “documented immigrants” and to exclude “illegal immigrants” (p. 7). However, the large overestimations reported (e.g., a perceived 36% share in the US vs. a 10% actual share of documented immigrants) could partly arise from respondents answering based on their broader, folk concept of “immigrant,” which might include undocumented individuals or even second-generation citizens. The authors directly test this alternative explanation by comparing perceptions to a broader “ground truth” that includes first-generation, second-generation, and undocumented immigrants, and find that a very large overestimation persists, suggesting the folk concept does not fully account for the misperception (pp. 22–23). A related but less significant issue arises from an inconsistency in the “ground truth” data used for comparison at the sub-national level. While the survey asks about *documented* immigrants, the local-level data for the U.S., UK, France, and Germany includes both documented and undocumented immigrants (pp. 11, A-11). For these countries, a respondent’s accurate perception of the total foreign-born population in their area would be recorded as an overestimation of the *documented* population. The authors acknowledge this data limitation, and note that the share of undocumented immigrants in the European countries is less than 0.5%, making its impact on the results negligible (p. A-11).

External validity of the study’s scope: The article’s introduction frames the research as an investigation into the “vitriolic debate about immigration” in the context of Brexit and recent U.S. and European elections (p. 1). However, the study then deliberately narrows its scope to focus exclusively on “documented immigrants,” explicitly stating, “We are NOT considering illegal immigrants” in order to “abstract

from issues of law enforcement and border control” (p. 7). This creates a disconnect between the real-world political conflicts that motivate the article and the more constrained subject it actually investigates. Much of the public anxiety and political rhetoric driving these debates centers precisely on undocumented immigration, sovereignty, and the rule of law. By abstracting these core issues away, the study’s ability to explain the phenomenon it uses for motivation is limited. While the authors are transparent about this choice, it raises questions about the external validity of the findings and their applicability to the broader, more contentious aspects of the immigration debate.

Causal interpretation of the “salience effect”: The study’s main causal claim is that making immigration “salient” by asking about it before asking about redistribution reduces support for redistribution (p. 1). This is based on an experiment that randomizes the order of the survey’s “immigration block” and “redistribution block.” However, the immigration block is not a simple prime; it is a long and cognitively demanding battery of questions requiring detailed estimations (pp. 8–9). The observed reduction in support for redistribution could be driven by mechanisms other than pure “salience,” such as cognitive fatigue or negative affect arising from the difficulty of the task. A respondent who is depleted or frustrated by the difficult immigration questions may give more heuristic-based or less generous answers on the subsequent redistribution block. The authors conduct explicit, dedicated tests for survey fatigue and find no evidence of increased carelessness or reduced time spent on questions (pp. A-36–A-37, Table A-9). However, these tests may not capture more subtle forms of cognitive depletion. The finding that the reverse order has no effect is consistent with the authors’ interpretation but does not definitively rule out a cognitive load explanation, making the causal claim about “salience” less secure than presented (p. 29).

Interpretation of null effects from informational treatments: The article finds that two experimental treatments providing factual information on the share and ori-

gins of immigrants had “negative, mostly insignificant effects on support for redistribution” (p. 35). The authors interpret this by arguing that any positive effect of the information was overwhelmed by the negative “priming” effect that the videos themselves created by making immigration salient (p. 35). This interpretation, while plausible given the results of the order experiment, is difficult to falsify, as any informational treatment on this topic will inevitably increase its salience. The authors provide a manipulation check showing that the treatments did successfully shift respondents’ factual beliefs, demonstrating that the information was received and at least partially incorporated (p. 33, Table 5). However, the analysis does not measure whether respondents found the information credible. An alternative explanation for the null effect on policy views is that respondents, particularly those with strong priors, may have updated their factual beliefs but dismissed the source as untrustworthy, thus not allowing the new information to influence their deeper-seated attitudes. The authors acknowledge that “Some respondents may not have believed the info provided,” but the ambiguity remains a limitation (p. 34).

Use of a composite redistribution index: The article utilizes a “Redistribution Support Index” for correlational analysis to test mechanisms (Figure 10, p. 31). This index is constructed as an unweighted average of z-scores for six different variables. However, as the article’s own experimental results show, the components of this index do not always move together. The main “Order/salience” treatment causes a statistically significant *decrease* in support for the “Social budget” but a statistically significant *increase* in support for the “Education budget” (Table 4, p. 29). While the authors transparently report these opposing effects individually for their main treatments, averaging them into a single index for subsequent correlational analysis is methodologically debatable, as it obscures these important dynamics and makes the overall index difficult to interpret.

Interpretation of the “narrative” treatment: The article frames its experimental results as showing that “narratives shape people’s views... more deeply than hard

facts” (p. 1). This is based on the finding that an anecdotal video about a “hard-working” immigrant had stronger effects than videos presenting statistical information. This interpretation may be too broad, as the experiment does not cleanly isolate “narrative” from “facts.” The “Hard work” treatment is a personalized story designed to evoke empathy, while the “Share” and “Origins” treatments are dry, cognitive presentations of aggregate statistics. The experiment compares an emotional, personalized stimulus to an abstract, statistical one. The finding may therefore demonstrate the well-established psychological power of emotional appeals over abstract data, rather than a more general principle about “narratives versus facts.”

Interpretation of correlational mechanisms: The article investigates the “mechanisms” driving opposition to redistribution by correlating respondents’ perceptions with their policy views, concluding that the belief that immigrants are “free-riders” is the strongest “predictor” (p. 30). While the authors are generally careful to use correlational language for this part of the analysis, the framing sometimes implies causality (e.g., the title of Figure 10, “What drives support...”). This analysis does not rule out the possibility of reverse causality: individuals with a pre-existing ideological opposition to redistribution may be more likely to adopt negative stereotypes about welfare recipients (including immigrants) to justify their views. The authors provide evidence against this by showing that priming redistribution views first does not affect immigration views, but this is an indirect test that does not fully resolve the potential for the observed correlation to be driven by stable, underlying anti-redistribution attitudes (p. 29).

Sample representativeness: The article claims its sample is “close to representative,” but the data provided in Table 1 reveal substantial and systematic deviations from national population statistics on key demographic variables (p. 6). These appear to be more than the “small imbalances” the authors suggest (p. 7). For instance, the share of respondents with a college degree is dramatically higher than in the population for several countries, including France (50% in sample vs. 31% in population)

and Italy (36% vs. 16%). Similarly, the employment rate is consistently lower in the sample than in the population for most countries (e.g., US: 60% vs. 70%). The authors acknowledge these imbalances and demonstrate in an appendix that the main results are robust to re-weighting the sample to match population targets (pp. 7, A-68). However, the primary findings presented in the main body of the article are based on the unweighted, non-representative sample, which may limit the external validity of the reported point estimates.

Ambiguity in the “John vs. Mohammad” question: To measure bias, the study asks respondents to compare the taxes and transfers of “John” (a native) and “Mohammad” (an immigrant), who are described as identical in all other respects (p. 9). The article interprets any deviation from answering “the same” as evidence of “bias.” However, the question may not be a clean measure of personal animus. Respondents may answer differently based on their (correct or incorrect) folk theories of how tax and welfare systems actually treat immigrants (e.g., believing immigrants are eligible for specific integration assistance). Their answer may reflect a perception of policy rules, not necessarily personal prejudice. While social desirability bias would likely push respondents toward answering “the same,” making the finding of a difference more striking, the ambiguity in what is being measured remains.

Ambiguity of the charitable donation outcome: The article uses a reduction in real donations to general poverty charities as a key behavioral outcome demonstrating reduced support for redistribution (pp. 1, 29). The authors deliberately chose charities “not concerned with immigrants in particular” to measure general pro-sociality (p. 10). However, the mechanism is ambiguous. Priming respondents to think about immigrants might not reduce their overall generosity, but rather shift their focus. A respondent, now thinking about the specific needs of immigrants, might become less willing to donate to a *general* poverty charity because they would prefer to donate to an immigrant-specific charity (which was not an option), or because they are unsure if the general charity serves the group now salient in their mind. Therefore, a

reduction in donations to a general charity does not definitively prove a reduction in the overall desire to redistribute; it could reflect a shift in the perceived appropriate target for that help.

Presentation and transparency issues: Several minor issues related to the presentation of data affect the article's transparency. First, while the main results in Table 4 are based on regression models with control variables, the simple, unadjusted difference in means between treatment and control groups is not reported, which would have provided a more transparent view of the raw experimental effects (p. 29). Second, the claim in the main text that "each randomization is balanced" is slightly overstated, as a footnote and appendix table show several covariates with p-values below 0.10, indicating minor imbalances (p. 10, footnote 11; Table A-13, p. A-42).

Future Research

Disentangling salience from cognitive load: Future work could use much shorter, simpler priming mechanisms—such as a single news headline, a brief audio clip, or an image—rather than a lengthy and complex questionnaire to isolate the effect of immigration salience on redistribution preferences without the confounding risk of inducing survey fatigue.

Testing source credibility in factual treatments: Researchers could vary the attributed source of the statistical information provided to respondents (e.g., a partisan think tank versus a neutral statistical agency) to determine if the failure of “hard facts” to shift policy views is due to the inherent weakness of facts or a lack of trust in the messenger.

Isolating emotional appeal from narrative structure: To better test the hypothesis that narratives are more powerful than facts, future experiments should compare emotional narratives against equally emotional factual presentations, or dry narratives against dry facts. This would prevent the conflation of narrative structure with emotional resonance.

Refining measures of bias versus policy perception: Future surveys could separate personal animus from perceived institutional bias by explicitly asking respondents what they believe the law *actually* dictates regarding immigrant welfare eligibility versus what they believe *should* happen, thereby clarifying ambiguous measures of anti-immigrant bias.

© 2026 The Catalogue of Errors Ltd

This work is licensed under a

Creative Commons Attribution 4.0 International License

(CC BY 4.0)

You are free to share and adapt this material for any purpose,
provided you give appropriate attribution.

isitcredible.com