A Report on "Republican Support and Economic Hardship: The Enduring Effects of the Opioid Epidemic" by Arteaga and Barone (2026)

Reviewer 2

February 09, 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: Arteaga, C. and Barone, V. (2026). Republican Support and Economic Hardship: The Enduring Effects of the Opioid Epidemic. *Quarterly Journal of Economics*, Vol. 141, No. 1, pp. 499–558.

URL: https://doi.org/10.1093/qje/qjaf051

Abstract Summary: This article establishes a causal link between the opioid epidemic and political realignment, showing that exposure to the epidemic, which induced economic hardship, led to substantial and continuous increases in the Republican vote share in House, presidential, and gubernatorial elections from the mid-2000s to 2022.

Key Methodology: Quasi-exogenous geographic variation in exposure to the opioid epidemic, proxied by 1996 cancer mortality rates at the commuting zone (CZ) level, is used in a difference-in-differences event-study framework.

Research Question: What are the enduring effects of the opioid epidemic on the political landscape of the United States?

Summary

Is It Credible?

Arteaga and Barone present a compelling argument establishing a "causal connection" between the opioid epidemic and the political realignment of the United States. Their headline claim is substantial: they estimate that a "one standard-deviation increase" in exposure to the epidemic, proxied by 1996 cancer mortality rates, led to a "4.5 percentage point increase in the Republican vote share" in House elections by 2022 (p. 499). They further argue that this political shift was driven by "induced economic hardship," specifically increased reliance on public transfer programs, and amplified by conservative media narratives.

The credibility of these claims rests entirely on the validity of their novel instrument: using 1996 cancer mortality rates to proxy for Purdue Pharma's initial marketing targets. The logic is that pharmaceutical companies targeted cancer patients to overcome stigma, creating a supply shock in those specific communities that later spilled over into the general population. The authors provide a robust defense of this strategy, showing that high-cancer areas saw disproportionate increases in opioid prescriptions and mortality (p. 501). However, the exclusion restriction—the assumption that 1996 cancer mortality affects voting patterns *only* through the opioid crisis—remains a point of vulnerability. Cancer mortality is not randomly assigned; it correlates with age, structural economic decline, and general public health. While the authors control for many of these factors, including "deaths of despair" and economic shocks like the "China shock," it is difficult to fully rule out the possibility that the instrument captures a broader trajectory of community distress that would have favored the Republican platform regardless of the opioid crisis (pp. 504, 546).

The evidence linking the instrument to the mechanism is logically sound but empirically indirect in places. The validation of the marketing strategy relies heavily on state-level correlations and qualitative evidence from internal documents, while the main analysis operates at the Commuting Zone (CZ) level (p. 512). This requires the reader to infer that the state-level marketing strategies were applied uniformly enough at the local level to drive the observed results. Furthermore, while the article documents significant increases in "economic hardship" via SNAP and disability applications, the supplementary materials reveal "no economically meaningful effects on labor market outcomes" such as unemployment (pp. 524, S35). This distinction is crucial: the "hardship" driving the political shift appears to be characterized by increased welfare dependence and health crises rather than a collapse in employment levels, a nuance that slightly reshapes the interpretation of the economic mechanism.

The proposed mechanisms regarding media and policy preferences are supported by data that is often correlational or cross-sectional. For instance, the finding that exposure to the crisis predicts support for law enforcement is based on 2020 survey data, which lacks the temporal depth of the main event-study analysis (p. 538). Similarly, the argument regarding media influence relies on the assumption that media markets align sufficiently with Commuting Zones to avoid spillover effects, a limitation that likely biases estimates conservatively but introduces measurement noise. Despite these limitations, the study's extensive robustness checks and the magnitude of the estimated effects suggest the core finding—that the opioid crisis acted as a catalyst for Republican support—is credible, even if the precise pathways remain complex.

The Bottom Line

The study provides strong evidence that communities historically targeted for opioid marketing shifted significantly toward the Republican Party over the last two decades. The magnitude of this shift is considerable, though the causal interpretation depends on accepting that 1996 cancer mortality rates influence modern voting behavior primarily through the opioid crisis rather than through correlated factors like general population health or structural decline. The narrative of “economic hardship” is well-supported regarding increased welfare reliance, though notably, the epidemic did not appear to drive aggregate job losses in these areas.

Potential Issues

Potential violation of the exclusion restriction: The study's identification strategy rests on the assumption that 1996 cancer mortality rates affect the outcomes of interest—economic hardship and political realignment—only through the channel of Purdue Pharma's marketing of OxyContin. This assumption, known as the exclusion restriction, may not hold. The 1996 cancer mortality rate is likely correlated with a range of pre-existing, slow-moving structural factors, such as industrial decline, population aging, poor public health, and other conditions associated with "deaths of despair." These factors could have their own direct causal pathways to the outcomes studied, meaning the instrument may be capturing the effects of broader community decline rather than isolating the impact of the opioid epidemic. The authors acknowledge this limitation, stating that "the validity of our identification strategy does not rely on cancer mortality being randomly assigned" and that it "reflects underlying demographic, environmental, and socioeconomic factors" (p. 516). To mitigate this concern, they conduct a series of robustness checks, including controlling for contemporaneous health trends and economic shocks and testing for pre-trends in related outcomes like suicide (pp. 541–546). While these are appropriate steps, the possibility remains that unobserved aspects of pre-existing community distress confound the estimates.

Inferred link between instrument and marketing mechanism: The study's causal claim is predicated on Purdue Pharma's marketing strategy precisely targeting high-cancer-mortality areas at the Commuting Zone (CZ) level in 1996. However, the article lacks direct evidence of marketing expenditures or sales force deployment at this geographic level for the critical launch period. The evidence for this "first stage" link is indirect, relying on internal corporate documents indicating strategic intent, state-level correlations between cancer mortality and prescriptions of a precursor opioid, and marketing data from much later periods (pp. 509, 512, S31). The authors are transparent about this data limitation, noting that "county- or CZ-level data on prescription rates are not available" for the pre-launch period and that they use cancer mortality as a "proxy" for the targeted markets (p. 511). While the provided evidence is logically consistent with their argument, the foundation of the instrument rests on an inferred, rather than directly observed, link between corporate strategy and the instrument at the CZ level during the crucial initial years of the epidemic.

Instrument validation at an inconsistent geographic level: The article's main analysis is conducted at the Commuting Zone (CZ) level, but the primary quantitative evidence validating the instrument is presented at the state level. Table I establishes a correlation between 1996 cancer mortality and 1994 prescription rates for MS Contin, a precursor opioid, but these are explicitly "STATE-LEVEL REGRESSIONS" (p. 512). The study assumes that this state-level relationship holds at the much finer CZ level, which is a necessary condition for the instrument to be valid for the main analysis. The authors acknowledge this is due to data availability, stating that "county- or CZ-level data on prescription rates are not available" for this period (p. 511). While this is a pragmatic approach given data constraints, it introduces a potential disconnect between the level at which the instrument is validated and the level at which it is deployed.

Debatable risk of media market spillovers: The article proposes that conservative media framing was a key mechanism driving the political shift. However, the analysis is conducted at the Commuting Zone (CZ) level, while media markets, particularly for national television, do not align with these boundaries. This geographic misalignment could lead to spillover effects, where residents of a "low-exposure" control CZ are influenced by media from a neighboring "high-exposure" treatment CZ. Such spillovers would violate the Stable Unit Treatment Value Assumption (SUTVA) and could contaminate the control group. The article does not explicitly discuss or test for these cross-unit spillovers. It is debatable whether this is a significant issue. On one hand, it introduces measurement error. On the other hand, such spillovers would likely bias the estimated effects toward zero, meaning the significant results reported in the article may be a conservative estimate of the true effect.

Causal claims about policy preferences based on cross-sectional data: The article argues that exposure to the opioid epidemic caused a shift in policy preferences, such as increased support for police (p. 503). The evidence for this claim, however, is based on a cross-sectional regression using 2020 survey data that correlates the 1996 cancer mortality rate in a respondent's CZ with their stated preferences in 2020 (Table V, p. 538). This design is correlational and lacks the pre-post structure of the main event-study analysis, making it insufficient to establish a causal relationship. The authors acknowledge the cross-sectional nature of this specific analysis in the notes to Table V. While this evidence is consistent with their proposed mechanism, the causal language used to describe it may overstate the strength of the finding.

Potential for ecological fallacy: The study's primary analysis is conducted at the aggregate Commuting Zone (CZ) level, which means that inferences about individual behavior must be made with caution. The finding that more-exposed CZs shifted toward the Republican party does not necessarily mean that the individuals directly harmed by the crisis were the ones who changed their vote. An alternative possibility is that the political shift was driven by other residents reacting to the crisis's community-level externalities, such as perceived crime and social decay. The authors acknowledge this limitation and attempt to address it by using individual-level survey data, which shows the political shift occurred across various demographic groups (Table IV, p. 528). This finding is consistent with a broad, community-wide response, but it cannot definitively distinguish between the behavior of those directly affected and those reacting to the effects on their community.

Omission of null labor market findings from the main text: The article's narrative of "economic hardship" focuses on strong, significant effects on the receipt of public assistance (SNAP, SSDI, SSI). However, the supplementary materials report null effects on core labor market indicators, stating "we do not observe economically meaningful effects on labor market outcomes" such as the unemployment rate or the employment-to-population ratio (p. S35). While the authors disclose this finding in a footnote and the appendix, its omission from the main body of the article may create an incomplete picture of the economic consequences of the crisis (p. 524, fn. 24). A more balanced presentation would require integrating the null findings on employment with the significant findings on transfer payments in the main narrative.

Data transparency and clerical issues: Several minor issues related to data presentation and transparency appear in the article. First, the analysis of disability applications relies on a sample of 438 CZs, a 29.5% reduction from the main sample of 621, which is attributed to "data suppression constraints" without further detail on the characteristics of the excluded areas (p. 520). Second, a footnote reveals that when local-level SNAP data are unavailable, the state-level share is imputed, but the extent of this imputation is not quantified (p. 520, fn. 21). Third, in the demographic heterogeneity analysis, the number of CZs in the sample fluctuates slightly across subgroups without explanation, from 616 in the full sample to 612 for college-educated respondents (Table IV, p. 528). While these are likely minor issues stemming from data constraints, greater transparency would improve the replicability and clarity of the analysis.

Future Research

Direct marketing verification: Future work could validate the first-stage mechanism by acquiring and analyzing local-level marketing expenditure or sales force data from the late 1990s, rather than relying on mortality proxies or state-level aggregates to infer corporate targeting strategies.

Individual-level linkages: To address potential ecological fallacies, researchers could link individual-level voting records with administrative health data to determine if the political shift is driven by those directly affected by opioid use or by community members reacting to the crisis's externalities.

Media market analysis: Future studies could rigorously test the media mechanism by exploiting the geographic mismatch between Commuting Zones and Designated Market Areas to isolate the causal effect of conservative news exposure on opioid-related political polarization.

isitcredible.com