Does Democratization Reduce Inequality? A Critical Appraisal of the Middle-Ground Theory

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Abstract

Arguing against prior theories that democratization has no impact on income inequality, Dorsch and Maarek (2019) contend in an *APSR* article that democratization causes extreme income distributions to move towards a "middle ground," reducing inequality in highly unequal autocracies while increasing inequality in relatively egalitarian ones. Central to the study's evidence is an instrumental variable strategy that leverages the regional share of democracies, and its interaction with initial inequality levels, to identify both the effect of democratization and the democracy–inequality interaction. We provide a critical replication of this study, making two central contributions. First, we show that the generated instrument violates the exclusion restriction and, second, even when properly constructed, the same linear function cannot be used to identify two causal effects. We then demonstrate how the same source of exogenous variation can be used to identify multiple causal effects using a generalized additive model, with non-linearities in the first stages serving as additional instruments. Across all specifications, the data do not support the middle-ground theory proposed by the authors: neither democracy nor its interaction with initial inequality has a statistically significant effect on the Gini coefficient. Our findings are consistent with an extensive literature in economics and political science that has struggled to uncover a systematic democracy–inequality link. The replication method we employ offers a practical tool for other studies in contexts where valid instruments are scarce or the exclusion restriction is difficult to satisfy.

Keywords: democratization; income inequality; instrumental variable; causal inference

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1 Introduction

Does democratization reduce income inequality? A broad literature has theorized the redistributive role of democracy either as a function of the median voter's preferences (Meltzer and Richard 1981; Boix 2003) or the preferences of newly empowered elites (Ansell and Samuels 2010). Dorsch and Maarek (2019) instead hypothesize a conditional effect for democratization, with more unequal societies becoming more equal after democratizing, and more equal societies becoming more unequal. The central premise of this claim is that autocracies are not monolithic but differ according to the size of their ruling coalitions. These regimes can display wide variation in pre-transition inequality levels, such that democratization may plausibly generate redistribution in some settings while increasing inequality in others. The argument holds that, as the winning coalition broadens with democracy, it encompasses a larger share of the income distribution, thereby shifting the new median toward the center. The authors hence argue for a "middle-ground" theory. While this conditional effect is intuitive, it remains novel in this context and therefore requires empirical scrutiny.

To identify causal effects, Dorsch and Maarek (2019) rely on an instrumental variable (IV) design. Drawing on research about democratic waves (Huntington 1993; Persson and Tabellini 2009), the authors instrument for democratization with the regional share of democracies, as well as the sixth-year lag of this measure. In addition, they multiply the instrument with the preexisting five-year average of the Gini coefficient. Thus, the IV strategy uses the same instruments to identify both democratization and its interaction with inequality, which in linear settings (a) violates the rank condition and yields singularities when estimated jointly and (b) results in post-instrument bias when estimated separately. This design, they argue, generates plausibly exogenous variation in democratization and permits identification of a conditional effect (democracy $\times$ inequality). The same design has subsequently been applied by Leipziger (2024) in the study of ethnic inequality, yielding a similar "middle-ground" pattern.

This replication reconsiders the original identification strategy with a careful analysis of the underlying logic of instrumentation. We argue that (1) increasing the number of instruments by interacting one with a potentially endogenous variable induces endogeneity in the first-stage relationship, and (2) when the same source of exogenous variation is used to identify multiple endogenous variables, standard linear IV techniques may produce biased estimates. In such contexts, exploiting functional form in the first stage can help recover identification without violating the exclusion restriction (Schwarz et al. 2026). We apply this insight to the study by Dorsch and Maarek (2019), first examining the functional form of the first-stage relationships and re-estimating their key models using generalized additive models (GAMs) to capture the non-linear associations in the first-stage regression. Employing a diagnostic test to estimate precision loss, we then show the rank-condition constraints in the use of multiple instruments to identify multiple conceptually-related variables. Our central finding is that, once the endogenous terms are properly instrumented, the results in Dorsch and Maarek (2019) do not reach conventional significance levels and the sign on the main coefficient (democracy $\times$ inequality) reverses.

This work contributes to the literature on democratization and inequality, as well as the methodological literature on causal inference in observational studies. First, it questions Dorsch and Maarek's (2019) theoretical claim that, if egalitarian autocracies democratize, they become more income unequal. Second, it highlights the importance of carefully modeling first-stage relationships in IV designs, especially when using composite instruments. Finally, it demonstrates how exploiting non-linearities in the first stage can restore identification and yield substantively different conclusions from those obtained using standard two-stage least squares (2SLS). The replication method we employ offers a practical tool for other studies in contexts where valid instruments are scarce or the exclusion restriction is difficult to satisfy.[1]

1. Finding valid instruments is one of the most persistent difficulties in social science research, and scholars often return to a small set of familiar choices. When the same or closely related instruments are used across multiple outcomes, their credibility is undermined (Bazzi and Clemens 2013). Gallen (2020) document that six popular instruments have appeared in hundreds of economics articles over recent decades. In political science, Mellon (2025) identifies 194 potential exclusion-restriction violations from the use of weather as an instrument and the implications for many published findings. Likewise, Haveresch et al. (2024) report 126 studies employing 56 different instruments based on topographic variation, each carrying risk of similar violations.

2 Theoretical Motivation

There is a basic unresolved question: Does democratization have a systematic distributive effect? The canonical median-voter logic predicts that expanding political rights should increase pressure for redistribution when the median voter is poorer than the mean voter (Meltzer and Richard 1981). Redistributive theories of regime change build on this logic by treating democratization as a response to conflict between elites and excluded groups, especially where inequality makes the political stakes of democracy high (Boix 2003; Acemoglu and Robinson 2006). Yet Collier (1999) shows that democratization in Western Europe and Latin America was often driven by elite-initiated transitions and working-class incorporation rather than redistributive pressure from below. Similarly, Ansell and Samuels (2010) argue that rising economic elites push for democracy primarily to protect their property rights from predatory autocrats, knowing that expropriation can be used as a tool for control (Sharif et al. 2025). These theories imply different empirical expectations. If democratization empowers poorer majorities, inequality should fall. However, if democratization reflects elite bargains or constrained transitions, the distributive consequences may be weak, delayed, or conditional on the organization of political actors.

Dorsch and Maarek's (2019) "middle-ground" theory is attractive because it offers a general reconciliation of these competing expectations. Democracy need not always reduce inequality; instead, it should move countries away from distributive extremes. Highly unequal autocracies should become more equal because democratization incorporates poorer citizens into the winning coalition. Relatively egalitarian autocracies should become more unequal because democratization moves policy away from narrow authoritarian coalitions that had previously sustained low inequality. This argument gives democratization a systematic conditional effect and turns pre-transition inequality into the key moderator. If correct, it would provide a parsimonious answer to why earlier studies find inconsistent democracy-inequality relationships. However, the theory also depends on a strong simplifying assumption: that the distributive consequences of democratization can be predicted primarily from the level of inequality before transition.

There are good reasons to doubt that assumption. A large literature in comparative politics shows that redistribution depends not only on formal regime type but also on parties, labor incorporation, state capacity, welfare institutions, and the historical organization of class conflict. Rueschemeyer et al. (1992), for instance, emphasize the role of organized subordinate classes in democratization, and Huber and Stephens (2012) show that the redistributive consequences of democracy in Latin America depend heavily on party systems, left governments, and social-policy institutions. Pribble (2013) similarly argues that welfare expansion is conditioned by programmatic parties, electoral competition, and the institutional legacies through which social policy is built. These explanations do not deny that democracy can matter. But they suggest that political inclusion must be converted into policy through organized actors and state institutions. Without those channels, democratization may expand rights without substantially altering the income distribution.

If the relevant mechanisms for income redistribution are parties, labor movements, fiscal capacity, and welfare institutions, then the same transition to electoral democracy can have different distributive consequences across cases with similar levels of pre-transition inequality. Elites may concede political rights to minorities to forestall organized political violence (Quinn et al. 2023), but at the same time retain sufficient institutional control to resist redistributive demands. Many case study comparisons that use the most-similar approach report patterns consistent with this interpretation. Costa Rica and Guatemala, for instance, shared an agrarian context, but Costa Rica's more inclusive state-building and later social-democratic institutions created pathways from democratic competition to social policy, whereas Guatemala's coercive labor regime and exclusionary state formation limited the distributive potential of political opening after democratization (Yashar 1997; Mahoney 2002).

Similarly, Chile and Uruguay both experienced military rule and democratic restoration, but Uruguay's stronger labor-linked left and more universalistic welfare trajectory made democracy more closely tied to social incorporation, while Chile's institutional constraints and privatized social-policy architecture limited post-transition redistribution (Huber and Stephens 2012; Pribble 2013; Chouhy 2022). Outside Latin America, South Korea and Taiwan likewise show that democratization after developmental authoritarianism did not automatically generate sustained redistribution; labor weakness and welfare stagnation constrained the egalitarian effects of democratic competition (Nam and Mah 2025). These comparisons support the theoretical basis for our replication: democratization's effect on inequality is historically and institutionally mediated, not mechanically determined by whether a country begins above or below a distributive middle ground.

3 Critical Reappraisal: Causal Identification with Multiple Endogenous Terms

The core identification challenge in Dorsch and Maarek (2019) is that the study leverages three instruments to identify democratization (X_1) and its interaction with inequality ($W \times X_1$). The instruments for X_1 are: Z_0 = lagged regional democracy and Z_1 = regional democracy. The underlying logic for the first two instruments is that democratization waves in a region are exogenous shocks to a country's democratic transition, affecting inequality only through domestic regime change. Then, $W \times X_1$ is identified by a third instrument: $W \times Z_1$ = the interaction of regional democracy with a country's mean pre-democracy Gini coefficient. The authors cite Wooldridge (2010) for the decision to multiply both the instrument Z_1 and the endogenous variable X_1 by the same W to identify the conditional effect of democratization.

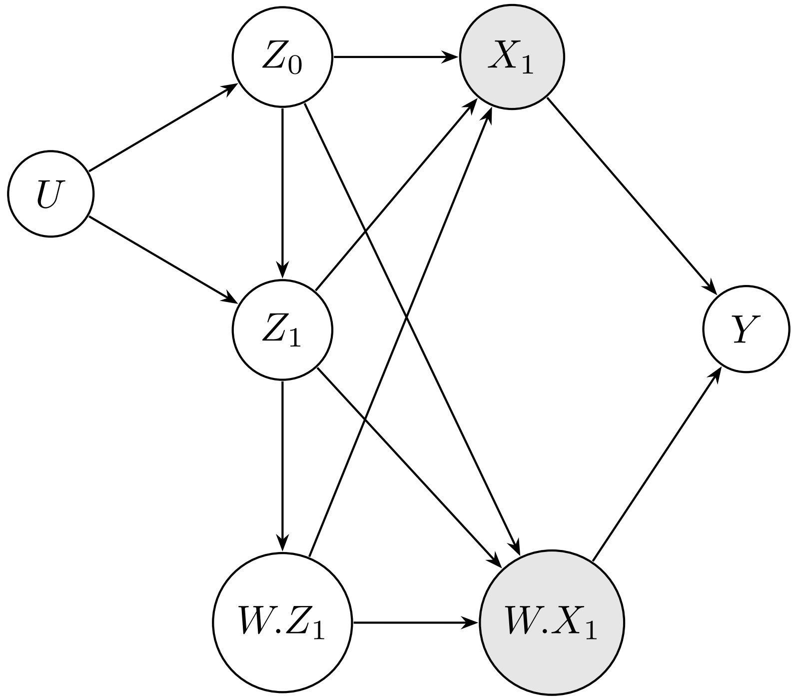
We question the validity of this strategy on two fronts. First, the construction of an additional instrument by interacting regional democracy with average inequality ($W \times Z_1$) introduces risk of endogeneity because W itself is unlikely to be exogenous. Average inequality levels in the years preceding democratization are shaped by the same unobserved political and economic shocks that can simultaneously influence both the likelihood of domestic democratization and the trajectory of inequality (Papaioannou and Siourounis 2008b). In this sense, the instrument inherits W's endogeneity, contaminating the identifying variation that the design relies upon. As Wooldridge (2010, p. 107) emphasizes, "unlike OLS under a zero conditional mean assumption, IV methods are never unbiased when at least one explanatory variable is endogenous in the model."

The difference between the authors' specification and recommendation by Wooldridge (2010) can be illustrated with an example. To study the effects of education, race, and their interaction on wages, Wooldridge (2010, p. 133) writes: the instrument for education – growing up near a four-year college – naturally generates an instrument for the interaction, since "if *nearc4* is valid as an instrumental variable for *educ*, then a natural instrument for *black*educ* is *black*nearc4*." The divergence from this textbook case is clear. Unlike a person's race, countries are not born with certain income inequality profiles. Thus, in Dorsch and Maarek's (2019) design, the constructed instrument ($W \times Z_1$) is likely correlated with unobserved determinants of inequality.[2]

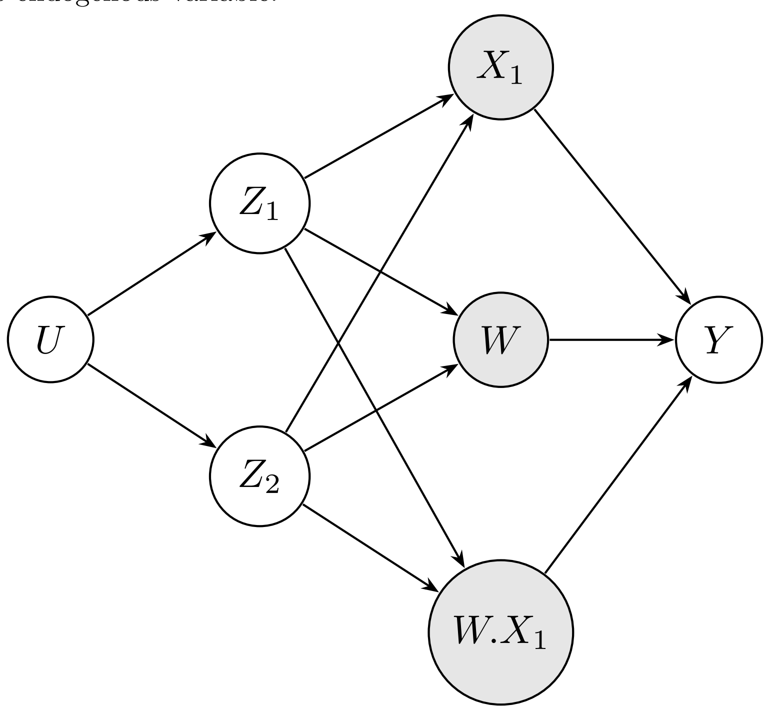
Figure 1 compares Dorsch and Maarek's (2019) identification strategy to the approach we use in the replication. Panel A reproduces the original structure: domestic democracy (X_1) and its multiplied product with average inequality ($W \times X_1$) are treated as endogenous (shaded in gray). Both are instrumented with two measures of regional democratization: regional democracy lagged by six years (Z_0) and by one year (Z_1). The arrow from Z_0 to Z_1 reflects the temporal dependence. To obtain a third instrument, the authors multiply Z_1 with the mean pre-democracy Gini coefficient of every observation, creating $W \times Z_1$. Note that W is a term shared by both the instrument and the endogenous variable. The design assumes that the unobserved regional factor U affects regional democracy (Z) but not inequality (W).[3]

2. Moreover, the authors omit W from the second-stage regression. Dropping constituent terms in this way is problematic in its own right (Brambor et al. 2006), but more importantly, it creates a channel through which the source of exogenous variation can affect the outcome directly, rather than exclusively through the endogenous variables it is meant to instrument.

3. It is not customary in DAGs to explicitly represent interaction terms as nodes; we do so here to clarify the sources of exogeneity.



(a) Original identification: **W** is the average pre-democracy Gini value multiplied with both the instrument and the endogenous variable.



(b) Replication: **W** is an endogenous term identified by the two instruments. $\mathbf{Z_0}$ (the sixth lag of regional democracy) is excluded due to collinearity with $\mathbf{Z_1}$ (regional democracy). $\mathbf{Z_2}$ (regional Gini) is added to identify **W** and the interaction term.

Figure 1: Comparison of Causal Structures for Instrumental Variable Models in Dorsch and Maarek's (2019) and the Replication

In our replication, illustrated in Panel b, we treat average inequality (W) as endogenous, since it is plausibly influenced by the same forces driving democratization (Papaioannou and Siourounis 2008b). In addition, because X_1 is a binary variable (democracy), the value of $W \times X_1$ switches off to 0 in the case of autocracies. Therefore, $W \times Z_1$ is only instrumenting for democracies ($X_1 = 1$); i.e., the autocratic observations in Dorsch and Maarek's (2019) study are not instrumented for. We thus drop the invalid $W \cdot Z_1$, and instead use the regional Gini coefficient (Z_2) as a second instrument. The regional confounder U, which affects regional democracy, also influences regional inequality, given spatial and institutional interdependence. This restores the necessary rank condition for identification.

We also drop Z_0, since this instrument (the six-year lag of regional democracy) is collinear with Z_1 (the one-year lag), causing the first-stage system to become ill-conditioned. Using the same underlying source of exogenous variation (regional democracy and its lag) to identify multiple endogenous regressors (X_1 and $W \times X_1$) may induce bias. If both X_1 and $W \times X_1$ are driven by nearly identical functions Z_0 and Z_1, the rank condition is violated and 2SLS estimates become unstable. Figure 2 presents the first-stage relationships between the three instruments and the two endogenous variables, using a smooth function that allows for flexible non-linear patterns. The top row shows that regional democracy and its lag predict democratization with very similar functional forms in terms of both curvature and coefficients, which is unsurprising given that the second instrument (Z_1) is simply a lag of the first (Z_0). The middle row shows that the instruments do not predict inequality (W) at all. In addition, the instruments provide little independent variation for the interaction term (compare top row with bottom row). We thus drop Z_0, since the lack of functional heterogeneity weakens the first stage and creates rank condition problems.

Panel b illustrates a further theoretical insight: even with a single source of exogenous variation (e.g., regional democracy, Z_1), it is possible to identify more than one endogenous variable, such as X_1 and $W \times X_1$, if the functional relationships between the instrument and each endogenous regressor are sufficiently non-linear and distinct. The next section formalizes

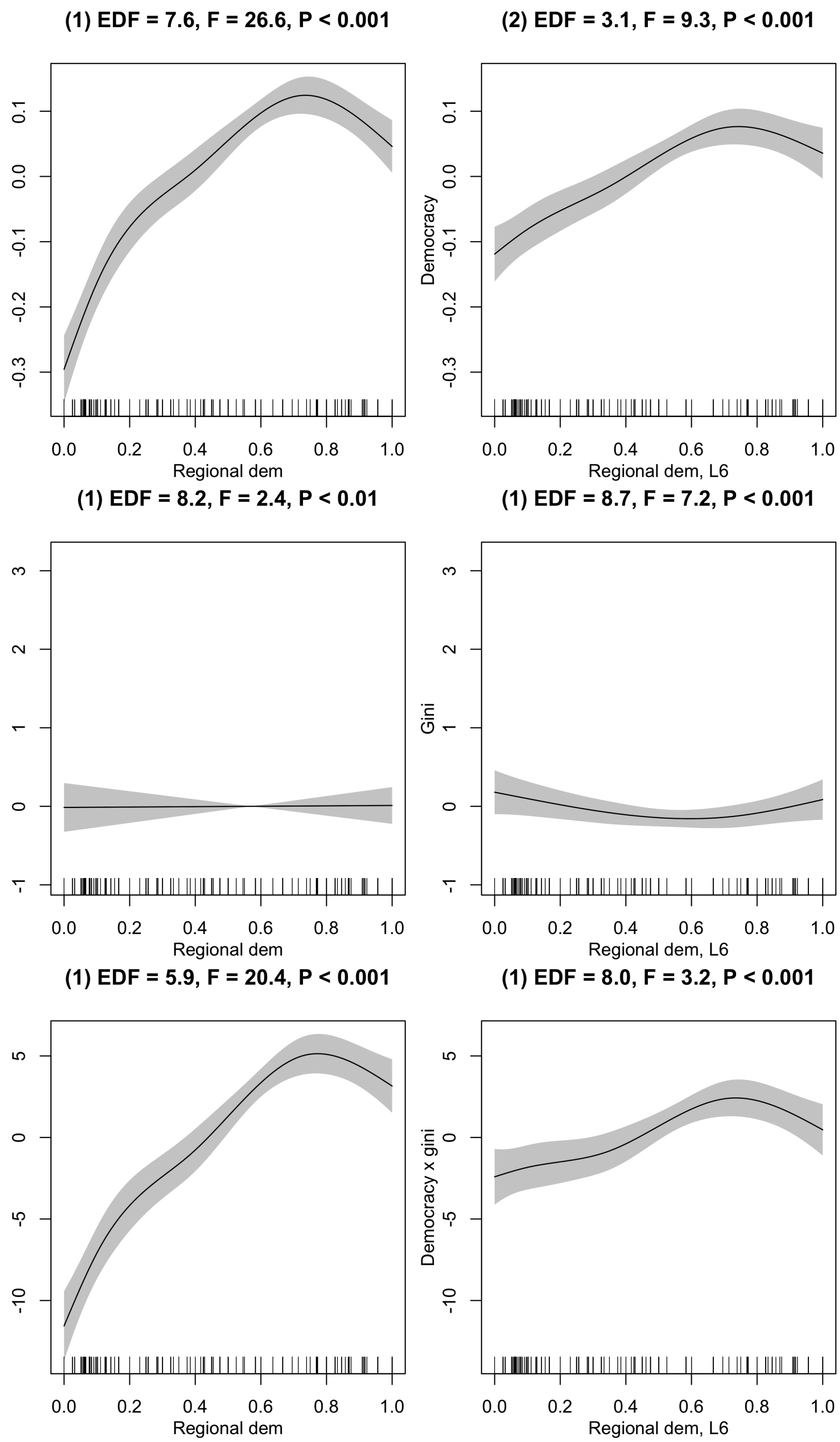


Figure 2: Smoothed first-stage relationships from generalized additive models (GAMs). Rows correspond to endogenous outcomes: democracy, inequality, and their interaction. Columns correspond to instruments: regional democracy and regional democracy lagged by six years.

this argument: as long as the first-stage fits for X_1 and $W \times X_1$ are sufficiently "wiggly" and non-collinear, the three effects can be consistently identified with only two instruments. We propose that regional democracy (Z_1) and regional inequality (Z_2) suffice to identify both endogenous variables and their interaction. We thus do not include $Z_1 \times Z_2$ as an instrument in our preferred specification. Section 4 presents the results with and without the extra instrument. While the results are substantively similar, the model without $Z_1 \times Z_2$ contains less precision error.

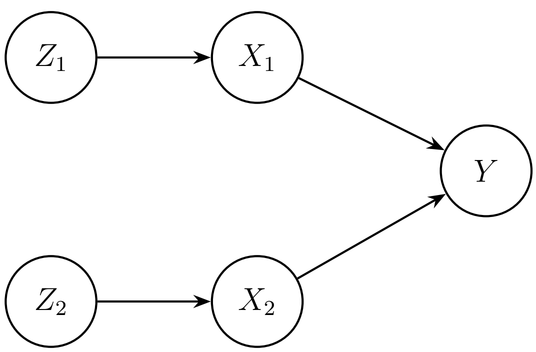
4 Estimation Strategy: The Control Function Approach

This section presents the approach we use to identify the three endogenous effects with only two instruments. The standard 2SLS method in IV estimation depends on two core assumptions: relevance (the instrument must predict the endogenous regressor) and exclusion (the instrument affects the outcome only through its effect on the endogenous regressor). While extensive attention has been devoted to finding valid instruments, relatively little scrutiny has been given to how functional form assumptions shape the identification power of instruments (Martens et al. 2006; Bazzi and Clemens 2013; Mellon 2025). Following Schwarz et al. (2026), we draw attention to an underutilized but powerful insight: even a single source of exogenous variation can, under specific conditions, serve to identify more than one endogenous effect if the first-stage relationships are sufficiently non-linear and functionally distinct. This section outlines the theoretical basis for the two-stage residual inclusion (2SRI) estimation strategy and its practical implications.[4]

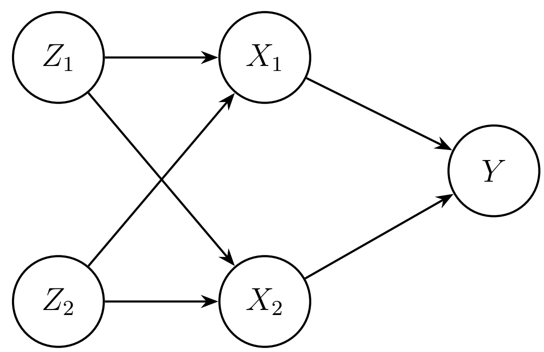
Figure 3 illustrates our strategy with three simple DAGs. Panel a presents the textbook setup: two exogenous instruments, Z_1 and Z_2, each exclusively identify their respective endogenous variables, X_1 and X_2, which in turn affect the outcome Y. In this ideal case, the exclusion restriction holds by construction, as each instrument enters only through its

4. The full formal treatment of this approach, including proofs of consistency, Monte Carlo evidence on finite-sample performance, and guidance on spline specification, is available in Schwarz et al. (2026).

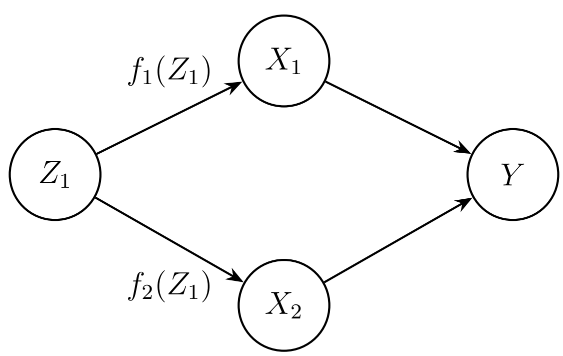
assigned endogenous regressor. Panel b allows both instruments (Z_1, Z_2) to influence both X_1 and X_2. Identification is still possible under the standard exclusion restriction, namely the instruments affect the outcome Y only through their effects on X_1 and X_2. The difficulty arises when the two instruments are themselves highly correlated; e.g., regional democracy and its lag. In that case, the information that one provides about the endogenous regressors is almost entirely replicated by the other, so the additional instrument adds little independent variation. This redundancy renders the first-stage design matrix nearly singular, or at least severely ill-conditioned, which in turn destabilizes the 2SLS estimates. Simply discarding one of the instruments is not a remedy either, because the system then reverts to being under-identified, with fewer instruments than endogenous variables.



(a) Ideal: separate identification



(b) Possible: instruments identify both treatments



(c) Proposed: single instrument with different, non-linear relationships

Figure 3: Simple directed acyclic graphs (DAGs) illustrating different identification strategies for multiple endogenous regressors.

Panel c demonstrates identification with a single instrument (Z_1) that influences both X_1 and X_2 via different functional forms $f_1(Z_1)$ and $f_2(Z_1)$. Here, functional form is crucial. As long as the non-linear relationships between Z_1 and each endogenous regressor are sufficiently distinct, and Z_1 is otherwise exogenous, it is possible to recover the effects of multiple endogenous treatments (Schwarz et al. 2026). This highlights the central argument of our replication: with careful modeling of nonlinearities, a single source of exogenous variation can be leveraged to identify multiple causal effects, thus expanding the scope of instrumental variable strategies in applied work.

Formally, consider a standard setting with two endogenous regressors, x_1 and x_2, and a single continuous instrument z. Let the structural model of interest be:

$$y = \alpha + \beta_1 x_1 + \beta_2 x_2 + \epsilon,$$

where ϵ is a mean-zero error term correlated with both x_1 and x_2. Traditional 2SLS estimation would be infeasible without separate instruments for each endogenous variable. However, suppose that x_1 and x_2 are generated by distinct, non-linear functions of z:

$$x_1 = f_1(z) + \nu_1,$$
$$x_2 = f_2(z) + \nu_2,$$

where f_1 and f_2 are smooth, flexible functions of z, and ν_1, ν_2 are error terms potentially correlated with ϵ. If $f_1(z)$ and $f_2(z)$ are not linear transformations of one another—that is, if they are not collinear—then they offer separate and functionally distinct variation that can be leveraged to identify β_1 and β_2, even though both functions originate from the same source z.

The estimation proceeds through a control function approach Marra and Radice 2011; Wooldridge 2015.[5] First, we estimate the reduced-form equations non-parametrically, using

5. 2SRI is an alternative to the more common two-stage predictor substitution (2SPS) and has the

spline regression or another non-linear smoother:

$$\hat{x}_1 = \hat{f}_1(z), \quad \hat{x}_2 = \hat{f}_2(z)$$

We then compute the first-stage residuals:

$$\hat{\nu}_1 = x_1 - \hat{f}_1(z), \quad \hat{\nu}_2 = x_2 - \hat{f}_2(z),$$

and estimate the second stage as:

$$y = \hat{\alpha} + \hat{\beta}_1 x_1 + \hat{\beta}_2 x_2 + \hat{\gamma}_1 \hat{\nu}_1 + \hat{\gamma}_2 \hat{\nu}_2 + \hat{\epsilon}.$$

Under standard assumptions (e.g., z is exogenous, $f_1(z)$ and $f_2(z)$ are sufficiently distinct), this procedure yields consistent estimates of β_1 and β_2.

The advantage of this approach is in its flexibility. Rather than relying on exclusion at the level of Z, it relies on the separability of functional relationships between Z and the endogenous variables. This shifts the burden of identification from finding multiple exogenous shocks to appropriately modeling the transformations of a single one. As the replication results demonstrate, this strategy uncovers meaningful differences in estimates that would otherwise be obscured or invalidated by multicollinearity and functional misspecification.

4.1 Sufficient Non-Linearity for Identification in the First Stage

The flexibility gained by allowing non-linear first-stage relationships is not unlimited; i.e., it is not such that an unlimited number of endogenous variables can be identified with the same source of exogeneity. Consistent second-stage estimation is based on a familiar requirement:

advantage of being consistent for models with non-linear link functions (Terza et al. 2008).

the matrix of first-stage fitted values,

$$\mathbf{F}(z) = \big[\hat{f}_1(z),\ \hat{f}_2(z),\ \ldots,\ \hat{f}_K(z)\big],$$

must have full column rank K. Intuitively, each endogenous regressor has to be driven by *some* variation of z that is not already perfectly captured by the other regressors' first–stage functions. When two columns of $\mathbf{F}(z)$ are (nearly) linear combinations of one another, the model confronts exactly the same rank condition violation that plagues standard linear IVs: the reduced-form variation cannot be cleanly attributed to the individual endogenous variables, resulting in uninformative or very unstable estimates.

A natural diagnostic for evaluating whether a set of non-linear relationships is sufficiently diverse to achieve identification is the log condition number, or precision loss (c.f. Belsley et al. 1980). Formally, let $\mathbf{B}(z)$ denote the spline basis employed in the first stage and let $\hat{f}_j(z) = \mathbf{B}(z)\,\hat{\boldsymbol{\theta}}_j$ be the fitted curve for the jth endogenous variable. Stacking the K fitted curves gives

$$\mathbf{F}(z) = \mathbf{B}(z)\,\big[\hat{\boldsymbol{\theta}}_1\ \hat{\boldsymbol{\theta}}_2\ \cdots\ \hat{\boldsymbol{\theta}}_K\big] \equiv \mathbf{B}(z)\,\hat{\boldsymbol{\Theta}}.$$

Identification thus requires that rank of $\mathbf{F}(z) = K$, or, equivalently, that the $K \times K$ Gram matrix $\hat{\boldsymbol{\Theta}}^{\top}\mathbf{M}\hat{\boldsymbol{\Theta}}$, where $\mathbf{M}$ is the usual projection matrix of $\mathbf{B}(z)$, is non-singular. Because $\mathbf{B}(z)$ can be chosen to be of very high, even arbitrary, dimension, the binding constraint is not the number of available basis functions, but whether the columns of $\mathbf{F}(z)$ are *sufficiently distinct*. When functional forms are too similar, they yield little new information and the rank condition fails.[6]

How can we detect whether the rank condition is close to failing in practice? We compute

6. This barrier may be overcome in sufficiently large samples, even nearly identical fitted functional forms may be useful for identification. Regardless, the condition number picks up on this insofar as larger samples lead to better first-stage fits which are statistically distinguishable.

the spectral condition number of the first-stage fitted-values matrix $\mathbf{F}(z)$,

$$\kappa_2(\mathbf{F}) \;=\; \frac{\sigma_{\max}(\mathbf{F})}{\sigma_{\min}(\mathbf{F})},$$

where $\sigma_{\max}$ and $\sigma_{\min}$ are the largest and smallest singular values of $\mathbf{F}$ (equivalently, $\kappa_2^2(\mathbf{F}) = \lambda_{\max}(\mathbf{F}^\top\mathbf{F})/\lambda_{\min}(\mathbf{F}^\top\mathbf{F})$). Following Cheney and Kincaid (2008), we report $\log_{10}\kappa_2$, which approximates the number of base-10 digits lost to rounding when solving the normal equations. In double precision, numerical instability becomes severe once $\log_{10}\kappa_2$ approaches about 16. The smaller the value of κ_2, the stronger is the first stage. $\log_{10}\kappa_2$ near 7 indicate potential problems. Beyond that level, additional parameters are unlikely to be recoverable without greater functional heterogeneity in the first stage.

5 Replication

We replicate Dorsch and Maarek's (2019) study by estimating non-linear first-stage functions of democracy and its interaction term on Z_1 and Z_2 using spline bases. This allows us to generate $\hat{f}_1(Z_1)$, $\hat{f}_2(Z_1)$, $\hat{f}_1(Z_2)$, and $\hat{f}_2(Z_2)$. First-stage equations are estimated as generalized additive models using tensor product smooths. Smoothing parameters are selected via generalized cross-validation (GCV). The basis dimension is set to the default of $k = 5$ per marginal for tensor product smooths. Although the functional forms are derived from the same source, they are functionally distinct. If sufficiently different, they can serve as valid instruments for X_1 and X_2. We then apply the control function estimator to recover the effect of democracy and its conditional interaction with inequality on income redistribution.

We estimate the following models:

$$X_1 = f_1(Z_1) + f_2(Z_2) + \nu_1, \quad W = f_3(Z_1) + f_4(Z_2) + \nu_2, \quad W.X_1 = f_5(Z_1) + f_6(Z_2) + \nu_3$$

After flexibly estimating $f_k(\cdot)$ with GAMs, we extract residuals ν_1, ν_2, and ν_3 that capture the endogenous components of X_1, W, and $W \times X_1$. These residuals are included in the second stage to correct for endogeneity. This approach is only possible if f_k are *not* nearly collinear, even if they originate from the same Z, and it avoids imposing the problematic interaction term $W \times Z_1$.

Figure 4 presents the smoothed first-stage relationships from three generalized additive models, with two regional predictors – regional democracy and regional inequality – as well as their interaction, used to explain domestic democracy (X_1), domestic inequality (X_2), and their interaction. In the top row, regional democracy has a concave relationship with X_1: moving from zero to around 40% democratic neighbors increases latent democracy by roughly 0.25, after which the effect plateaus. Regional inequality is also a strong, non-linear predictor of democracy, and the interaction term adds further curvature. All three smooths are significant ($p < 0.001$), with effective degrees of freedom (EDF) exceeding 7.5 and strong F-statistics.

In the middle row, regional democracy does not predict domestic inequality ($F = 0.026$, $p = 0.87$), but regional inequality shows a clear oscillatory effect (EDF $= 5.6$), and the interaction term is significant as well. Thus, our choice of a new instrument – regional inequality (Z_2) – to instrument for inequality seems appropriate. In the bottom row, the interaction outcome ($X_1 \times X_2$) is strongly predicted by all three instruments, each showing substantial non-linearity and statistical significance. These results confirm that the excluded instruments generate meaningful variation in all three endogenous terms, supporting the identification strategy discussed in the previous section.

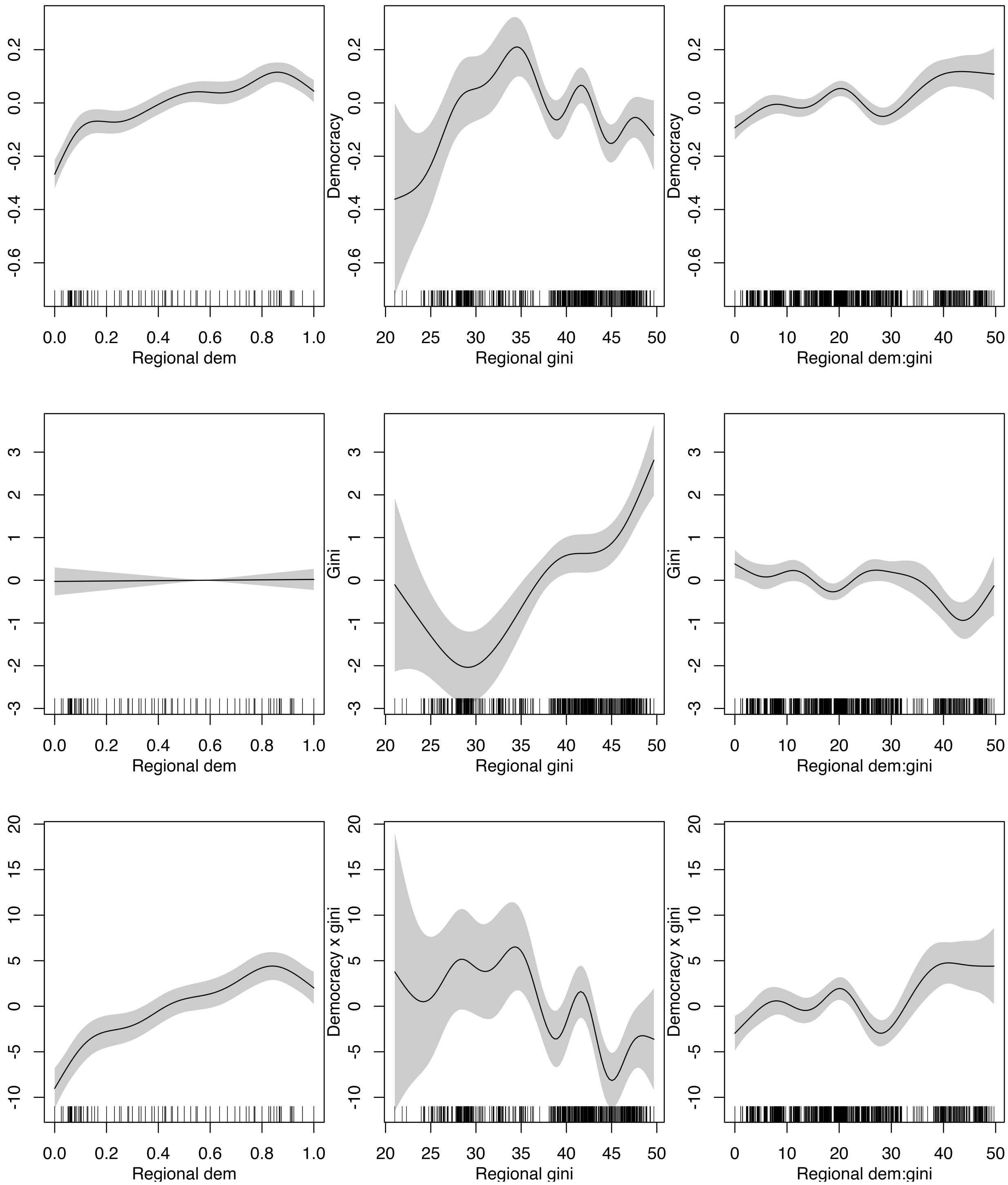


Figure 4: Smoothed first-stage relationships from generalized additive models. Rows correspond to endogenous outcomes: domestic democracy (X_1), inequality (X_2), and their interaction ($X_1 \times X_2$). Columns correspond to instruments: regional democracy, regional inequality, and their interaction. Shaded areas represent 95% confidence intervals.

In the second-stage equation,

$$Y = \alpha + \beta_1 X_1 + \beta_2 W + \beta_3 (W \times X_1) + \gamma_1 \hat{\nu}_1 + \gamma_2 \hat{\nu}_2 + \gamma_3 \hat{\nu}_3 + \epsilon,$$

the primary estimand is the average marginal effect of democratic transition on the Gini coefficient, evaluated at the mean of the pre-transition inequality distribution:

$$\frac{\partial\, E[Y \mid X_1, W]}{\partial X_1} = \beta_1 + \beta_3 \bar{W}, \tag{1}$$

where β_1 captures the baseline effect of democratization and β_3 captures how that effect varies with pre-transition inequality W, averaged over its empirical distribution.[7] With a linear second stage and no endogenous interactions, 2SRI and 2SLS are mathematically equivalent.

5.1 Data

The replication employs the identical country–year panel assembled by Dorsch and Maarek (2019), spanning 147 countries from 1960 to 2010. The dependent variable is the Gini coefficient of disposable household income, drawn from the Standardized World Income Inequality Database (SWIID, v. 7.1). The key explanatory variable is a dichotomous measure of democracy that mirrors the coding rule introduced by Papaioannou and Siourounis (2008a) and adopted by Acemoglu et al. (2019). A country–year is coded as democratic when two conditions hold simultaneously: (i) Freedom House classifies the country as *Free* or *Partly Free* in a given year and (ii) the composite *Polity2* score in the Polity IV dataset (Marshall et al. 2010) is strictly positive. To estimate the conditional effect of democratization, the authors construct an interaction term between democracy and a five-year average of Gini coefficients. To address missingness, Dorsch and Maarek (2019) use median imputation across time and countries, and we follow the same strategy to ensure exact replication of the

7. The coefficient on W does not have a clean causal interpretation given the dynamic structure of the panel: W is a five-year average of lagged Gini values and is retained as the constitutive term of the interaction.

original results. All other covariates, including lagged GDP per capita and regional democracy measures, are retained in their original form. The sample consists of 3,905 observations, consistent with the main specification in the original study.

5.2 Results

Table 1 presents the OLS and 2SLS estimates from Dorsch and Maarek's (2019) alongside our replication results using the two-stage residual inclusion (2SRI) approach with bootstrapped standard errors. In the original models (Columns 1–4), democracy on its own does not have a statistically significant effect on income inequality, and in some cases even switches sign depending on the inclusion of interaction terms or instruments. As noted previously, the authors omit $\bar{Gini}$ – a constitutive term of the interaction – from their model, which is why there are no coefficients associated with this variable. When the model includes the interaction between democracy and pre-democracy income inequality ($\bar{Gini}$) in Models 2 and 4, democracy appears to increase inequality in more egalitarian societies and reduce it in those with higher initial inequality. The authors interpret these results as evidence for their "middle ground" hypothesis: democratization moves both highly unequal and highly equal autocracies toward moderate, centrist levels of income distribution. The inclusion of the interaction term, which is negative and significant in the original study, is central to this interpretation.

Our replications, shown in Columns 5–7, do not substantiate the same conditional effect. Model 5 only includes democracy as the endogenous term: the coefficient remains statistically insignificant. Model 6 mirrors the structure presented in the DAG (Figure 1b).[8] The interaction between democracy and pre-democracy inequality fails to attain significance in any of the replicated models. The sign of the interaction term flips to become positive. The

8. Standard errors in the 2SRI columns are computed via a case-resampling bootstrap that reruns the full two-stage procedure on each resample, with 1,000 bootstrap draws. Point estimates are taken from the original sample; standard errors are the standard deviation of the bootstrap coefficient distribution; and 95% confidence intervals are quantile-based, taken at the 2.5th and 97.5th percentiles. This procedure correctly propagates the estimation uncertainty introduced in the first stage, which naive second-stage standard errors from a single OLS run do not account for (Wooldridge 2015).

only consistently significant predictors of inequality are lagged inequality levels and lagged GDP per capita, whose coefficients remain stable and robust across specifications. Model 7 includes the interaction term $Z_1 \times Z_2$ in the first stage. The results remain substantively similar to Model 6, but the log condition number κ increases from 3.92 to 4.06, indicating precision loss with the added instrument. Thus, Column 6 is the preferred specification. These results show that the conditional effect of democracy on income inequality is fragile and sensitive to model specification and instrument choice. The lack of statistical significance for the endogenous interaction term, once properly instrumented, implies that democratization does not *cause* countries to become either more or less income equal.

Table 1: The Effect of Democracy on Income Inequality

	Dorsch and Maarek (2019)				Replication		
	OLS		2SLS		2SRI		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\text{Democracy}_{\text{t-1}}$	-0.060	1.465***	0.219	1.446***	0.086	-0.512	1.003
	(0.10)	(0.33)	(0.34)	(0.45)	(0.205)	(1.054)	(0.615)
$\bar{\text{Gini}}$	–	–	–	–	–	-0.241	-0.005
						(0.161)	(0.069)
$\text{Democracy}_{\text{t-1}}\times\bar{\text{Gini}}$	–	-0.038***	–	-0.034***	–	0.018	-0.028
		(0.01)		(0.01)		(0.030)	(0.017)
GDP $\text{pc}_{\text{t-1}}$ (log)	0.408**	0.309*	0.404**	0.316*	0.406***	0.559**	0.338***
	(0.16)	(0.16)	(0.16)	(0.17)	(0.090)	(0.186)	(0.102)
$\text{Gini}_{\text{t-1}}$	0.887***	0.892***	0.890***	0.893***	0.888***	1.018***	0.893***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.008)	(0.086)	(0.037)
$\text{Democracy}_{\text{t-1}}$ residuals	–	–	–	–	-0.163	5.506***	4.018***
					(0.217)	(1.114)	(0.678)
Gini residuals	–	–	–	–	–	0.653***	0.421***
						(0.162)	(0.070)
$\text{Dem.}_{\text{t-1}}\times\bar{\text{Gini}}$ residuals	–	–	–	–	–	-0.139***	-0.095***
						(0.031)	(0.018)
Adj. R^2	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Kappa	–	–	–	–	0	3.92	4.06
Num. instruments	–	–	2	3	1	2	3
Num. obs.	3905	3905	3905	3905	3905	3905	3905

Note: Standard errors in parentheses. *** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$. Model 6 includes only the constitutive components as instruments (the preferred model), while Model 7 adds the regional democracy × regional Gini interaction as an instrument, which increases the κ.

6 Conclusion

Our replication paints a markedly different empirical picture from that offered by Dorsch and Maarek (2019). Once we correctly model the instrumented relationships, the "middle-ground" hypothesis is not supported by the data: neither democracy nor its interaction with initial inequality exerts a statistically discernible impact on the post-democratization income inequality levels. These null results are consistent with a much broader body of research that has struggled to uncover a systematic democracy–inequality link. A comprehensive review by

Acemoglu et al. (2015) documents how estimated effects vary as widely as the estimation methods themselves, concluding that no persuasive pattern emerges across fixed-effects, dynamic panels, or synthetic control designs. Subsequent work has reinforced this skepticism. Studies of nineteenth-century Europe and twentieth-century Latin America find little support for distributive theories of franchise extension (Acemoglu and Robinson 2001), and cross-sectional or event-history analyses likewise fail to identify robust egalitarian dividends from regime change (Perotti 1996; Gradstein and Milanovic 2004; Aidt and Jensen 2009). Even when democracies do redistribute, the direction and magnitude of the effect are often contingent on auxiliary institutions and fiscal choices that can either mitigate or aggravate inequality (Piketty 2013). Our replication therefore joins a large chorus pointing toward indeterminate aggregate consequences of democratization for income distribution.

Finally, the replication results invite a return to theory. If democratization's redistributive consequences are at best episodic, scholars may focus less on regime type and more on the political coalitions, fiscal architectures, and social policies that mediate the translation of political rights into material outcomes. Future work could, for instance, examine whether newly empowered median voters demand progressive taxation only when embedded in party systems that lower coordination costs, or whether economic crises condition the willingness of elites to concede redistribution irrespective of regime form. A further theoretical avenue concerns the assumption that the winning coalition in autocracies is drawn from a single class position in the income distribution. Selectorate theory instead emphasizes loyalty and affinity as the bases for coalition formation, implying that minimal winning coalitions may be small, cross-cutting, and income-diverse (Benuno de Mesquita et al. 2003). If it is cheaper to secure support from a coalition that spans income groups, a rational autocrat will do so, and redistributional pressures may not follow the simple logic of median income shifts. Until such mechanisms are more clearly specified and empirically validated, researchers should remain cautious about framing democratization as inherently egalitarian or inegalitarian.

Data Availability Statement

All data, replication files, and instructional code for using the methodology will be made available on the Harvard Dataverse.

Ethical Considerations

The project did not involve research with human subjects.

Competing Interests Declaration

The authors report no conflict of interest.

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