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Political power and wealth distribution: how anti-corruption reforms reshape inequality

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Abstract
This study examines the causal effects of anti-corruption reforms on wealth concentration. Using data from the World Inequality Database, the World Governance Indicators, and V-Dem across more than 150 countries, I employ a staggered difference-in-differences approach to identify the impact of sustained drops in measured corruption on wealth distribution. Sustained anti-corruption reforms significantly reduce wealth concentration at the top while increasing shares at the bottom of the distribution. Ten years after implementation, the top 1% wealth share falls by approximately 2 percentage points and the top 10% share by 1.5 percentage points, while the bottom 50% share rises by 0.4 percentage points and the bottom 20% share by 0.15 percentage points. These effects emerge gradually, becoming statistically significant within four years of reforms and persisting throughout the observation period. The results are robust across alternative estimators, alternative corruption indicators, and specifications that control for lagged dependent variables, national income, the top 1% income share, political violence, and inflation. A synthetic control analysis of Georgia illustrates how reforms disrupt elite patronage networks, while the subsequent policy reversal caused distributional gains to stagnate. The findings extend the political economy literature by showing that elite-disrupting mechanisms dominate offsetting concerns about informal redistribution and that reforms reshape wealth distribution through expanded access to financial services and property rights rather than through direct fiscal transfers.

Keywords Corruption · Wealth inequality · Institutional reform · Political economy

JEL Classification D31 · D73 · H11 · O43 · P16

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1 Introduction

Recent decades have witnessed growing wealth concentration globally, with the top 1% accumulating 38% of all additional wealth since the mid-1990s while the bottom 50% captured just 2% (Chancel et al. 2022). This trend raises fundamental questions about the political determinants of inequality and whether institutional reforms can reshape distributive outcomes. These questions lie at the heart of political economy research, which examines how political power and institutions structure economic relations (Acemoglu and Robinson 2012; Boix 2003).

The relationship between corruption—the misuse of public power for private gain— and economic inequality has attracted considerable scholarly attention, though with conflicting findings and important limitations. Previous studies examining corruption and *income* inequality are constrained by small country samples, dated data, and a lack of causal identification. Moreover, the results are contradictory: while Dobson and Ramlogan-Dobson (2012) find that lower corruption is associated with *higher* income inequality in Latin America, others—including Gupta et al. (2002), studying 38 countries over 1980–1997, and Gyimah-Brempong (2002), examining African countries—find the opposite relationship. Berggren and Bjørnskov (2020) further complicate the picture by demonstrating that the relationship depends on institutional stability, with corruption contributing to a more equal distribution of consumption when political institutions have been stable for longer periods.

What distinguishes this study is its focus on *wealth* rather than income inequality. Wealth and income, while related, operate through distinct mechanisms and accumulation dynamics. Wealth represents the cumulative stock of assets built over time, whereas income captures annual flows. Corruption's effects on wealth-building opportunities— through restricted access to capital markets, insecure property rights, and exclusion from formal economic institutions—compound over years and even generations, potentially producing larger and more persistent distributional effects than those observed for income. This distinction is critical: the mechanisms linking corruption to wealth distribution may differ substantially from those affecting current income. The study therefore focuses on three key mechanisms through which reforms reshape wealth distribution: (1) disruption of rent-seeking networks that benefit economic elites; (2) reduced state capture by wealthy interests; and (3) democratization of economic opportunity through reduced corruption costs for lower-income groups. Importantly, the theoretical relationship between corruption and wealth distribution is ambiguous. While grand corruption and state capture clearly concentrate wealth among connected elites, some forms of corruption—such as the selective non-enforcement of regulations affecting the poor—may function as informal redistribution (Holland 2017). This ambiguity means that the net effect of anti-corruption reforms on wealth distribution is fundamentally an empirical question.

To address this question causally, I employ a staggered difference-in-differences approach on panel data, using "jumps" in corruption indicators as proxies for successful reforms. This methodological strategy allows me to identify the causal effect of anti-corruption reforms on wealth shares for different segments of the distribution. The analysis uses data from the World Inequality Database on wealth shares for the top 1%, top 10%, bottom 50%, and bottom 20%, combined with corruption indicators from the World Bank and V-Dem datasets. This represents a substantial advance over previous studies: I examine more than 150 countries with data extending to 2020 and employ modern causal inference methods (LP-DiD,

synthetic control) that address the identification challenges plaguing earlier correlational work (Hartwig and Sturm 2024).

Case evidence from a successful reformer complements the quantitative analysis. Georgia's anti-corruption reforms after the Rose Revolution disrupted entrenched corruption systems, and a synthetic control analysis reveals clear effects on wealth distribution—effects that reversed when policymakers scaled back these reforms after 2012. This case illustrates how political reforms reshape power relations in ways that affect wealth distribution, while also demonstrating the fragility of reform gains.

The findings from the quantitative analysis reveal that anti-corruption reforms have significant distributional consequences. Following successful reforms, the wealth share of the top 1% and top 10% decreases, while the share of the bottom 50% and bottom 20% increases. These effects emerge gradually, becoming statistically significant within four years of reforms and persisting for at least a decade. The results are robust across alternative estimators (Callaway and Sant'Anna 2021; Wooldridge 2024), alternative corruption indicators (V-Dem), and even two-way fixed effects models.

This study makes three contributions to political science research. First, it provides causal evidence on how political institutional reforms shape economic inequality, advancing our understanding of the political determinants of economic outcomes. Second, it demonstrates how corruption affects different segments of the wealth distribution, revealing the heterogeneous effects of political institutions across socioeconomic groups. Third, it contributes to debates about the relationship between corruption and inequality by showing that the empirical relationship between institutional quality and wealth distribution— while theoretically ambiguous—favors redistributive effects of anti-corruption reforms in practice.

The paper proceeds as follows. Section 2 develops a political economy framework linking corruption to wealth concentration, situating this relationship within debates about political institutions and inequality. Section 3 describes the data and discusses the methodological approach, with particular attention to causal identification. Section 4 presents the empirical results, including a synthetic control case study of Georgia, the main difference-in-differences analysis, and robustness checks. Section 5 discusses the implications for political science research and policy.

2 Political institutions, corruption, and wealth distribution

2.1 The politics of inequality and institutional quality

Corruption represents a critical dimension of institutional quality with potential distributional implications. A substantial literature examines the relationship between corruption and income inequality, though with conflicting findings that highlight the complexity of this relationship.

Several studies find that corruption exacerbates income inequality.Gupta et al. (2002), studying 38 countries over 1980–1997, document that corruption increases both income inequality and poverty through mechanisms including reduced progressivity of taxation and lower social spending. Gyimah-Brempong (2002), focusing on African countries, and Dincer and Gunalp (2008), examining U.S. states, reach similar conclusions. However, Dobson and Ramlogan-Dobson (2012) challenge this consensus, finding that lower corrup-

tion is associated with *higher* income inequality in Latin America—a result they attribute to corruption's role in enabling informal economic participation among the poor. Pavlik and Callais (2024), who examine income inequality using modern causal methods, find that reductions in corruption increase incomes for the top 80% but do not significantly affect incomes of the bottom 20%, with inconsistent effects at the very top. More recent studies emphasize contextual factors that may explain these conflicting results. Berggren and Bjørnskov (2020), using comprehensive cross-country data, demonstrate that the relationship between corruption and inequality depends on institutional stability. They find that corruption contributes to a more equal distribution of consumption when political institutions have been stable for longer periods, suggesting that distributional effects are contingent on broader institutional arrangements. Hartwig and Sturm (2024) further complicate the picture by finding that factors such as financial development, demographic structure, unemployment, and capital intensity may matter more for inequality than corruption itself.

These conflicting findings likely reflect both methodological limitations in earlier work—small samples, dated data, and correlational designs—and genuine heterogeneity in corruption's effects across contexts. As Fisman and Golden (2017) observe, corruption channels resources toward politically connected insiders, but the identity and breadth of these "insiders" vary considerably across political systems, producing different distributional consequences depending on who benefits from corrupt arrangements.

2.1.1 Why wealth? Distinguishing wealth from income inequality

While the literature on corruption and income inequality is substantial, the relationship between corruption and *wealth* inequality has received far less attention. Wealth and income, while related, operate through distinct mechanisms and accumulation dynamics that may respond differently to corruption and anti-corruption reforms.

Four key distinctions motivate this study's focus on wealth. First, **accumulation horizons** differ fundamentally: wealth represents the cumulative stock of assets accumulated over time—often across generations—while income captures annual flows. Corruption's effects on wealth-building opportunities compound over years, potentially producing larger and more persistent distributional effects than those observed for income.

Second, **capital market access** is essential for wealth accumulation and is particularly vulnerable to corruption. Accessing mortgages, business loans, and investment opportunities typically requires navigating formal financial institutions where corruption can create significant barriers. Less wealthy individuals who lack political connections face higher costs and greater uncertainty in these markets, constraining their ability to build wealth even when their incomes improve. Capasso et al. (2025) demonstrate that corruption causally limits access to formal financial services, with particularly pronounced effects for those outside elite networks.

Third, **property rights and asset security** are foundational to wealth accumulation. Holding and transferring assets requires secure property rights that are consistently enforced. In corrupt systems, property rights may be selectively enforced based on political connections, creating uncertainty that discourages wealth accumulation among those outside elite networks. This mechanism operates independently of current income: individuals may earn adequate incomes but remain unable to accumulate wealth if their property rights are insecure.

Fourth, corruption shapes the **formal versus informal economy** divide, with important implications for wealth accumulation. Corruption pushes economic activity into informality, where wealth accumulation opportunities are severely limited (Capasso et al. 2025). Those operating informally cannot access formal banking services, cannot use assets as collateral for loans, cannot establish clear legal title to property, and face constant risk of expropriation. By reducing corruption, reforms can shrink the informal sector and enable broader participation in formal economic activities where wealth accumulation is more feasible.

2.2 Theoretical framework: corruption and wealth distribution

The theoretical relationship between corruption and wealth distribution is genuinely ambiguous, with plausible arguments for effects in either direction. This ambiguity motivates treating the question as fundamentally empirical rather than assuming a predetermined relationship.

One perspective, advanced by Rothstein and Uslaner (2005) and Uslaner (2008), suggests that corruption exacerbates inequality by creating a social trap where corruption and inequality reinforce each other. According to this view, corruption transfers resources from the general public to elites and weakens the state's ability to enforce contracts and property rights impartially. This perspective predicts that anti-corruption reforms would reduce wealth concentration.

A contrasting view suggests that corruption may benefit the poor in some contexts. Holland (2017) argues that selective non-enforcement of laws—what she terms "forbearance"— functions as informal redistribution when formal welfare programs are inadequate. Politicians may deliberately overlook illegal street vending, informal housing, or utility theft to secure electoral support from poor constituents. Anti-corruption reforms that eliminate such forbearance without providing formal alternatives could paradoxically harm those at the bottom of the distribution. Similarly, Dobson and Ramlogan-Dobson (2012) argue that in contexts where formal institutions systematically exclude the poor, corruption may provide alternative pathways to economic participation.

A third view, proposed by Li et al. (2000), suggests a non-linear relationship where inequality is highest in moderately corrupt countries and lower in either highly corrupt or very clean countries.

These competing perspectives suggest that the net effect of anti-corruption reforms on wealth distribution depends on the specific forms of corruption addressed and the broader institutional context. Reforms targeting grand corruption and elite state capture would be expected to reduce top wealth concentration, while reforms eliminating petty corruption and informal forbearance arrangements could have more complex effects at the bottom of the distribution. This theoretical ambiguity frames the present study as addressing a genuinely open empirical question, allowing the data to reveal whether elite-disrupting effects dominate or whether more complex patterns emerge.

2.3 Mechanisms linking anti-corruption reforms to wealth distribution

Building on these perspectives, I develop a political economy framework identifying mechanisms through which anti-corruption reforms affect wealth distribution. Critically, these mechanisms operate on wealth specifically—through access to asset markets, property

rights, and financial institutions—rather than merely affecting current income flows. This distinction matters because wealth accumulation requires sustained access to institutions that corruption can systematically distort.

2.3.1 Mechanisms reducing wealth concentration

Corruption creates opportunities for economic elites to extract rents through preferential access to government contracts, regulatory privileges, and protection from competition, allowing wealth accumulation at rates exceeding what transparent, competitive environments would permit. Anti-corruption reforms disrupt these networks by increasing transparency in government procurement, strengthening regulatory independence, and reducing opportunities for preferential treatment. As Hellman and Kaufmann (2001) observe, corruption allows economic elites to capture the state, using political influence to shape policies and regulations that benefit their interests. Wealthy individuals and corporations often exercise disproportionate influence over policy-making in corrupt systems, securing favorable tax treatment, subsidies, and regulatory exemptions. This creates a self-reinforcing cycle where economic power translates into political power, which in turn reinforces economic advantage. Anti-corruption reforms can disrupt this cycle by increasing the independence of regulatory agencies, strengthening conflict-of-interest rules, and enhancing transparency in political financing. As McMann et al. (2021) demonstrate, state capture by economic elites is particularly pronounced in hybrid regimes and weak democracies, where formal democratic procedures coexist with corruption networks.

Perhaps the most direct wealth-specific mechanism operates through access to formal financial services. In corrupt systems, obtaining loans, mortgages, business credit, or investment accounts often requires bribes or political connections that systematically exclude those without resources or networks (Capasso et al. 2025). Bank officers may demand payments to process loan applications; property registrars may require bribes to record titles; investment advisors may reserve opportunities for connected clients. When corruption declines, financial institutions can evaluate creditworthiness based on transparent criteria—income, collateral, credit history—rather than connections, enabling broader participation in wealth-building financial products such as mortgages, business loans, and investment accounts. Gupta et al. (2002) document that corruption reduces access to financial services for the poor, suggesting that anti-corruption reforms should expand this access.

Corruption also creates substantial barriers to registering assets, obtaining legal title to property, and participating in formal markets. Entrepreneurs may operate informally to avoid corrupt licensing officials; property owners may lack formal title because registration requires bribes; savings may remain outside the banking system because account opening involves corrupt procedures. This informality traps wealth in forms that cannot be leveraged for further accumulation—informal businesses cannot access credit, untitled property cannot serve as collateral, and unbanked savings earn no returns while facing theft risk. When corruption barriers fall, individuals can move previously informal wealth into the official economy by registering property titles, formalizing businesses, and accessing investment vehicles. This formalization both protects existing wealth through secure property rights and enables further accumulation through access to credit using formalized assets as collateral.

Finally, corruption functions as a regressive tax, with bribes consuming a larger share of income and wealth for poorer individuals (Hunt and Laszlo 2012). A fixed bribe for a busi-

ness license represents a trivial cost for a wealthy entrepreneur but a prohibitive barrier for someone with few resources. By reducing these costs, anti-corruption reforms disproportionately benefit those at the bottom of the distribution, enabling wealth accumulation that was previously impossible.

2.3.2 Offsetting mechanisms

While the mechanisms above suggest that anti-corruption reforms should reduce wealth inequality, important offsetting mechanisms may operate in the opposite direction. Berggren and Bjørnskov (2020) note that corruption may benefit the relatively poor if it allows them to circumvent regulations or other economic barriers set up to protect elite special interests. Licensing requirements, zoning restrictions, professional certifications, and formal sector regulations often serve to limit competition and protect incumbents. When corruption allows individuals to bypass these barriers—operating businesses without licenses, building housing without permits, practicing trades without credentials—it may enable economic participation and wealth accumulation that formal rules would prevent. Anti-corruption reforms that enforce these regulations more stringently may paradoxically harm those who previously evaded them, displacing street vendors who operated through bribing inspectors, demolishing informal housing, or shutting down unlicensed practitioners. Unless reforms simultaneously address the exclusionary nature of underlying regulations, eliminating corruption in enforcement may concentrate wealth further by protecting incumbent interests.

Anti-corruption reforms that increase state capacity and enforcement may also be captured by elites who redirect strengthened institutions to serve their interests (Hellman 1998). Transparent procurement processes may still favor well-connected firms with sophisticated bidding capacity; strengthened property rights may primarily benefit those who already hold formal title; improved contract enforcement may serve creditors more than debtors. If elites can shape reform implementation to protect their privileges while eliminating corruption that benefited others, the net distributional effect could favor the top. Even when reforms ultimately benefit the poor, transition periods may involve costs that fall disproportionately on vulnerable groups. As informal arrangements are dismantled and formal systems are constructed, small informal businesses may fail before formal credit becomes available, informal workers may lose livelihoods before formal employment expands, and informal property may lose value before formal title is accessible.

2.3.3 Contextual variation and hypotheses

These competing mechanisms suggest that the relationship between anti-corruption reforms and wealth distribution is contingent on context and reform design. The type of corruption addressed matters considerably: reforms targeting grand corruption and elite state capture are more likely to reduce top wealth concentration, while reforms focused on petty corruption and street-level enforcement may have more ambiguous effects at the bottom. Complementary institutional reforms also shape outcomes, as anti-corruption reforms accompanied by expanded formal financial access, simplified business registration, and inclusive property rights systems are more likely to benefit the poor than reforms that simply increase enforcement of exclusionary regulations. Political context affects implementation as well: in democratic settings, anti-corruption reforms often emerge from electoral competition and

civil society pressure, with broader constituencies shaping reform design (Mungiu-Pippidi 2015), while in autocratic contexts, reforms may be more selective, preserving core rent-seeking opportunities for ruling elites (Bauhr and Charron 2018).

Given this theoretical ambiguity, not all mechanisms lead anti-corruption efforts to reduce wealth inequality; the direction and magnitude of effects require empirical investigation. I propose the following hypotheses as testable propositions about which mechanisms dominate in practice:

H1A *Anti-corruption reforms lead to decreased concentration of wealth at the top of the distribution, particularly affecting the top 1% and 10% wealth shares.*

H1B *Anti-corruption reforms lead to increased concentration of wealth at the bottom of the distribution, particularly affecting the bottom 50% and 20% wealth shares.*

These hypotheses reflect the expectation that mechanisms disrupting elite rent-seeking and expanding access to wealth-building institutions dominate the offsetting mechanisms discussed above. However, the empirical analysis examines effects separately for each segment of the wealth distribution, allowing for the possibility that reforms reduce top concentration while having null or even negative effects at the bottom if forbearance mechanisms are important. The staggered difference-in-differences design also permits examination of dynamic effects, testing whether any negative transition costs are followed by longer-term benefits as formal institutions mature.

3 Data and methodology

3.1 Data

My analysis uses a panel dataset covering more than 150 countries from the 1970s to the present, with more than 3,000 country-year observations. I combine data on wealth inequality from the World Inequality Database with corruption indicators and political variables from the World Bank, V-Dem, and other sources.

For dependent variables, I use wealth concentration measures from the World Inequality Database (WID), which provides comprehensive wealth distribution data based on a combination of administrative (tax) and survey sources. While limitations exist regarding data comparability across countries and time periods, the WID represents the most comprehensive cross-country dataset on wealth inequality available. The primary dependent variables include the top 1% wealth share, the top 10% wealth share, the bottom 50% wealth share, and the bottom 20% wealth share. These measures capture wealth concentration at both ends of the distribution, allowing for a nuanced analysis of how anti-corruption reforms affect different segments of society. Table 1 presents descriptive statistics for these variables, showing that on average the top 1% controls 30.3% of a country's total wealth, while the top 10% holds 63.5%. The bottom 50% holds only 3.6% of wealth, and the bottom 20% has a negative mean (−1.3%), indicating that debts exceed assets for this group.

As key independent variables used to identify "jumps" as proxies for successful anti-corruption initiatives, I use two primary corruption indicators. The Control of Corruption

Table 1 Variable descriptions and descriptive statistics

Variables	Mean	Std. dev	Min	Max
Dependent variables				
Top 1% Share of Wealth	0.303	0.080	0.125	0.582
Top 10% Share of Wealth	0.635	0.076	0.408	0.909
Bottom 50% Share of Wealth	0.036	0.023	−0.081	0.160
Bottom 20% Share of Wealth	−0.013	0.012	−0.096	0.092
Corruption indicators				
Control of Corruption Estimate (WGI)	−0.036	1.035	−1.869	2.470
Public Sector Corruption Index (V-Dem)	0.462	0.305	0.001	0.986
Control variables				
Political Violence (V-Dem)	−0.508	1.493	−3.625	3.882
Adjusted National Income (USD, mln)	415,836.2	1,470,614	−387,974.6	1.72e+07
Inflation Rate (%)	18.671	253.323	−30.199	12,338.66
Top 1% Share of Income	0.160	0.051	0.025	0.359
Treatment variable				
Corruption "Jumps" (Sustained Reform)	0.144	0.295	0	1

Estimate (World Bank WGI) is an aggregate measure of perceptions of the extent to which public power is exercised for private gain. This measure ranges from approximately −2.5 (highly corrupt) to 2.5 (very clean). The Public Sector Corruption Index (V-Dem) is an expert-coded measure capturing the extent to which public sector employees grant favors in exchange for bribes and misappropriate public resources. This measure ranges from 0 (highly corrupt) to 1 (very clean).

The conditional models incorporate four control variables that address potential confounding factors identified in the literature on corruption and inequality. First, the *inflation rate* is measured as the annual percentage change in the GDP deflator, sourced from the World Bank's World Development Indicators (WDI). Inflation may affect wealth distribution by eroding the real value of nominal assets and debts differently across wealth groups (Easterly and Fischer 2001). Second, the *top 1% income share* captures the percentage of pre-tax national income accruing to the top 1% of earners, obtained from the World Inequality Database. This variable controls for the relationship between income and wealth concentration, as high income inequality may drive wealth accumulation at the top independently of corruption dynamics (Piketty 2014). Third, *adjusted national income* (in millions

of USD) from the World Bank WDI measures gross national income minus consumption of fixed capital and depletion of natural resources. This variable controls for economic development, as wealthier countries tend to have both lower corruption and different wealth distribution patterns. Fourth, *political violence* is measured using the V-Dem physical violence index (v2caviol), which captures the extent to which political organizations use violence against individuals or groups. This variable ranges from low (−3.6) to high (3.9) levels of violence and controls for political instability that could affect both corruption opportunities and wealth distribution through mechanisms such as asset destruction, capital flight, or disruption of economic activity (Alesina and Perotti 1996). To identify anti-corruption reforms, I follow the method proposed by Grier and Grier (2021), which identifies "jumps" in an institutional indicator that can be interpreted as proxies for successful reforms. It should be noted that Grier and Grier (2021) apply this method to identify large policy reforms ("Washington Consensus"-style liberalization episodes) rather than anti-corruption reforms specifically; I adapt their approach to the corruption domain. Specifically, I code a reform as occurring when a country experiences an increase exceeding one standard deviation in the Control of Corruption (WGI) indicator within a five-year period, which is then sustained for at least five years. This approach captures meaningful, persistent improvements in corruption control rather than short-term fluctuations.

The logic behind this identification strategy offers several advantages over alternative approaches. First, it focuses on actual outcomes rather than merely announced policies, which may not be effectively implemented. Many countries formally adopt anti-corruption laws and institutions without meaningful enforcement or implementation (Rose-Ackerman and Palifka 2016). Second, the requirement for sustained improvement (five years) filters out ephemeral changes that might result from measurement error, short-term political calculations, or window-dressing efforts aimed at international audiences (Bauhr 2017). Third, the one-standard-deviation threshold establishes that the change is substantial within the country's historical context, distinguishing reforms that represent genuine institutional transformation from incremental adjustments (Mungiu-Pippidi 2015). Finally, this data-driven approach allows for comparable identification of reforms across diverse institutional and cultural contexts where the specific form and content of anti-corruption efforts may vary considerably.

This process identified 54 anti-corruption reforms, primarily occurring in the early 2000s and 2010s. The reforms span different regions, with 6 countries from Eastern Europe and Central Asia, 13 from Latin America and the Caribbean, 24 from Sub-Saharan Africa, and 9 from Asia and the Pacific region experiencing significant positive shifts in corruption control. These "jumps" in the Control of Corruption Estimate (WGI) yield a treatment-indicator mean of 0.144, meaning that 14% of country-year observations fall within the post-reform window of a sustained jump (as illustrated in Fig. 7 in the Appendix).

The 54 reform episodes I identify differ in number from closely related studies applying reform-jump designs to different institutional dimensions. Grier and Grier (2021) analyze large policy reforms ("Washington Consensus"-style liberalization episodes), not anti-corruption reforms, and thus examine a fundamentally different set of events; any difference in the number of reforms they identify is a consequence of studying a different phenomenon, not of methodological divergence. The more directly comparable study is Bologna Pavlik et al. (2023), which applies the same jump-identification approach to corruption specifically and identifies fewer reform episodes than the present paper. That difference reflects data

coverage rather than methodology: the present study uses the World Governance Indicators (WGI), which span more than 150 countries from 1996 to the present, whereas Bologna Pavlik et al. (2023) rely on a different corruption dataset with narrower coverage. To verify that this difference is indeed data-driven rather than mechanical, I conduct a robustness check using the V-Dem Public Sector Corruption Index as an alternative treatment indicator. The V-Dem specification identifies substantially fewer reforms (27 episodes)—more comparable to the numbers reported in related studies—yet the results remain robust for the bottom shares. As shown in Table 5 in the Appendix, the V-Dem-based and WGI-based treatments overlap in 133 post-reform country-years, providing reassurance that the findings are not artifacts of the specific corruption indicator chosen. While this study focuses on successful anti-corruption reforms—those that achieved sustained improvements—it is important to acknowledge that many reform attempts do not meet these criteria. Unsuccessful reforms include cases where improvements in corruption indicators either failed to reach the one-standard-deviation threshold or were not sustained for the required five-year period. These failures can result from several factors: insufficient political will, elite capture of reform institutions, or reversal following political transitions. For example, several countries in Sub-Saharan Africa and Latin America experienced modest improvements in corruption control during the 2000s that dissipated within two to three years, suggesting reform announcements without effective implementation. Additionally, some countries that initially met the criteria for successful reforms subsequently experienced backsliding. As illustrated in the Georgia case study in Section 4.2, corruption indicators can decline after an initial reform period when political changes lead to the hollowing-out of anti-corruption institutions. Such reversals that are not sustained are excluded from the analysis. The 54 successful reforms identified thus represent a subset of all attempted reforms, specifically those that achieved meaningful and lasting improvements in corruption control.

3.2 Identification strategy and empirical approach

Estimating the causal effect of anti-corruption reforms on wealth concentration presents several challenges. Corruption is endogenous to political and economic conditions, and reforms may be implemented in response to changing economic circumstances.

To address these challenges and to bolster causal inference, I employ a staggered difference-in-differences approach using the "jumps" in corruption indicators as treatment events. Standard two-way fixed effects difference-in-differences estimators can produce biased estimates in settings with staggered treatment timing and heterogeneous treatment effects (De Chaisemartin and d'Haultfœuille 2020; Goodman-Bacon 2021). To address this issue, I implement the Local Projection Difference-in-Differences (LP-DiD) estimator proposed by Dube et al. (2023):

$$\Delta_h Y_{it} = country_i^h + \beta^{hLP} \Delta D_{it} + e_{it}^h \; for \; h = 1, 2, ...H \tag{1}$$

Here $\Delta_h Y_{it}$ is the difference in the wealth share between periods t and $t + h$, country^h represents country fixed effects, ΔD_{it} is the change in treatment status, and e^h is the error term. This approach estimates the dynamic treatment effects of anti-corruption reforms on wealth concentration for different time horizons after the reform. The coefficient $\beta^{h\,LP}$ represents the average treatment effect on the treated (ATET) for horizon h.

I also implement alternative estimators proposed by Callaway and Sant'Anna (2021) and Wooldridge (2024) as robustness checks. I additionally include standard two-way fixed effects (TWFE) models (both unconditional and conditional) to establish correlational patterns and motivate the causal analysis, rather than as the main causal estimates. These alternative approaches address the challenges of heterogeneous treatment effects in staggered difference-in-differences designs through different methodological strategies, providing a check on the sensitivity of results to the specific estimator used.

The causal identification strategy relies on three key assumptions. First, the parallel trends assumption holds that, in the absence of treatment, average outcomes in treated and control units would follow parallel paths. I test this assumption by examining pretreatment trends. Second, the no-anticipation assumption requires that units do not adjust their behavior with respect to the outcome variable (inequality) in anticipation of future treatment. This assumption is plausible because anti-corruption reforms typically result from complex political processes, including regime changes, external pressures from international organizations, and responses to unexpected corruption scandals. Empirical evidence from cases such as Georgia's post-Rose Revolution initiatives supports this claim, as these reforms resulted from political transitions whose specific timing and reform content were not predictable to economic actors sufficiently in advance to permit adjustments in wealth-holding patterns. Moreover, the primary aim of anti-corruption reforms is to combat corruption; reductions in inequality emerge only as a secondary, inherent outcome. Third, the Stable Unit Treatment Value Assumption (SUTVA) requires that one unit's treatment does not affect outcomes for other units. Anti-corruption reforms are primarily country-specific policy interventions. While cross-country spillovers are possible, the global dispersion of reforms across different time periods makes large spillover effects unlikely.

One potential concern with the WGI indicator is that the set of primary data sources used to construct the aggregate measure can change from year to year, raising questions about comparability across time. However, several features of the identification strategy mitigate this concern. First, the requirement that improvements exceed one standard deviation means that identified jumps are substantially larger than the typical year-to-year variation that might result from changes in underlying sources. Second, the sustainability requirement (improvements maintained for at least five years) filters out short-term fluctuations that could reflect measurement artifacts rather than genuine institutional change. Third, the robustness of results when using the V-Dem indicator—which is constructed using a consistent expert-coding methodology—provides reassurance that findings are not driven by idiosyncrasies in WGI measurement. Nevertheless, I acknowledge that perception-based measures have limitations, and the Georgia case study presented in Section 4.2 demonstrates that measured improvements corresponded to concrete policy changes and behavioral outcomes documented in independent surveys.

To assess the validity of these assumptions, I conduct several checks. I examine the significance of pre-treatment estimates to test for differential trends before reforms, conduct sensitivity analyses with different thresholds for identifying reforms, implement alternative estimators to check robustness to methodological choices, and estimate models with regional control groups to address potential spillover effects. These validity checks provide confidence that the estimated treatment effects represent causal impacts of anti-corruption reforms rather than pre-existing trends or other confounding factors.

4 Empirical results

4.1 Descriptive patterns

Before presenting the formal analysis, I examine descriptive patterns in the data. Figure 1 compares wealth concentration trends between reformers (countries that implemented successful anti-corruption reforms) and non-reformers over time. Despite relatively modest year-to-year variation, the graph reveals an important divergence in trends following reforms. Specifically, reformers experience a notable decrease in top wealth shares relative to non-reformers, with the gap widening gradually but consistently post-reform. For reformers, the total share of wealth held by the bottom 50% increases and converges with that of non-reformers over time, though the effect for the bottom 20% is less pronounced in the raw data. Most reformer data begin in the 1990s, as many successful reform implementers come from emerging regions with reliable data starting from this period.

These descriptive patterns suggest that anti-corruption reforms might affect wealth concentration, but they do not establish causality. The formal analysis below addresses this limitation by employing panel models and difference-in-differences designs to identify causal effects. I begin by illustrating the mechanisms at work in a single country using the synthetic control method, and then turn to the main cross-country difference-in-differences analysis.

4.2 Case illustration: Georgia

To illustrate how the political mechanisms outlined in Section 2 operate in practice, I examine anti-corruption reform in Georgia, where initial reforms achieved notable success before

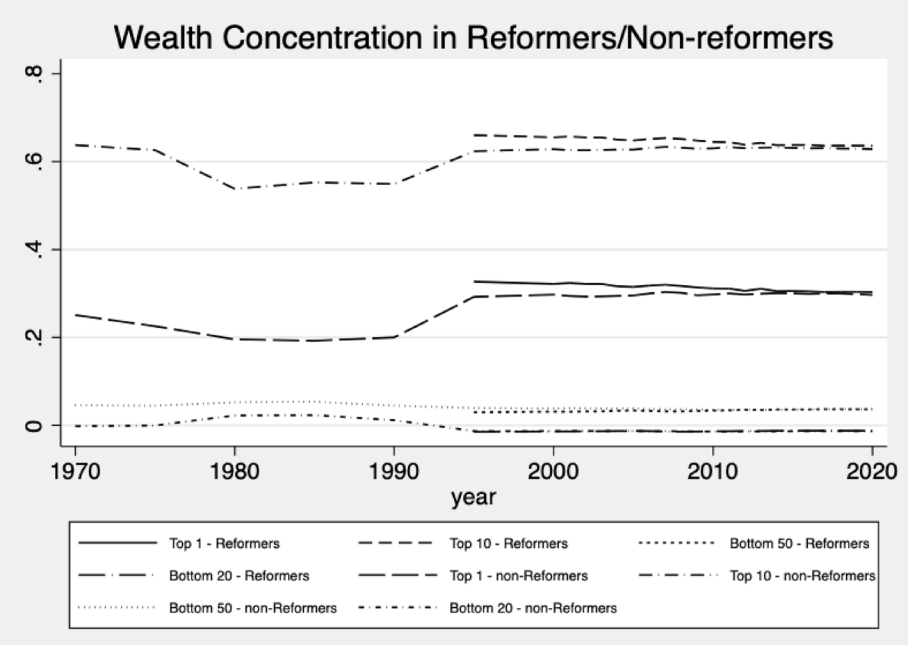

Fig. 1 Wealth concentration among reformers and non-reformers

being subsequently scaled back—providing a unique opportunity to observe the effects of both reform and reversal on wealth distribution.

4.2.1 Background: reform and reversal

Georgia presents a compelling case for examining how anti-corruption reforms reshape wealth distribution, as the country experienced both successful reforms and subsequent reversal, allowing observation of effects in both directions. Following the 2003 Rose Revolution, Georgia achieved dramatic success in its fight against corruption, with its Control of Corruption (WGI) score improving from −1.1 in 2003 to 1.1 by 2008 (z-scores; see Fig. 6 in the Appendix). The post-revolution government implemented radical anti-corruption measures, including dismissing the entire traffic police force, streamlining business regulations, and prosecuting corrupt officials from the previous regime (Papava 2009).

These reforms directly targeted state capture by disrupting the corruption networks that had enabled economic elites to secure monopolistic positions in key sectors including energy, telecommunications, and imports. By reducing barriers to entry in previously monopolized sectors, the reforms enabled new market participants to emerge and disrupted established rent-seeking arrangements (Kupatadze 2018). The reforms also dramatically reduced petty corruption, as evidenced by survey data showing that the percentage of businesses reporting having paid bribes fell from over 60% in 2003 to under 10% by 2008 (World Bank 2012).

4.2.2 Synthetic control analysis

To provide causal evidence on the distributive effects of these reforms, I employ the synthetic control method pioneered by Abadie et al. (2010). This approach constructs a counterfactual "synthetic Georgia" as a weighted combination of untreated countries (those not experiencing reforms during the same period) that most closely resembles Georgia's pre-treatment characteristics on each outcome of interest. The donor pool consists of 25 countries that did not experience a sustained anti-corruption jump during the relevant window (see the notes to Table 2 in the Appendix). Full technical details, including predictor balance, root-mean-squared prediction errors, and donor-country weights, are reported in the Appendix (Tables 2–4).

Figure 2 presents the results for the period 2000–2011, with 2004 as the treatment year, for each of the four wealth share measures reforms. During the reform period (2004–2011), Georgia experienced a decrease in top wealth concentration relative to its synthetic counterfactual. Specifically, the top 1% wealth share remained virtually unchanged, oscillating between approximately 22.6% and 22.7%, while synthetic Georgia showed an increase to approximately 23.5% (with a dip during the 2008–2009 crisis years). Similarly, the top 10% share remained relatively stable in Georgia while the "unreformed" synthetic Georgia increased. Concurrently, the bottom 50% wealth share in Georgia remained stable at around 5%, slightly outperforming the synthetic counterfactual. For the bottom 20%, reforms helped boost their share from −0.011% (where debt exceeds assets) to −0.0105%, compared with the counterfactual trajectory.

Figure 3 presents synthetic control results for the reform-reversal period (2011–2020), with 2014 as the treatment year marking the consolidation of policy changes under the

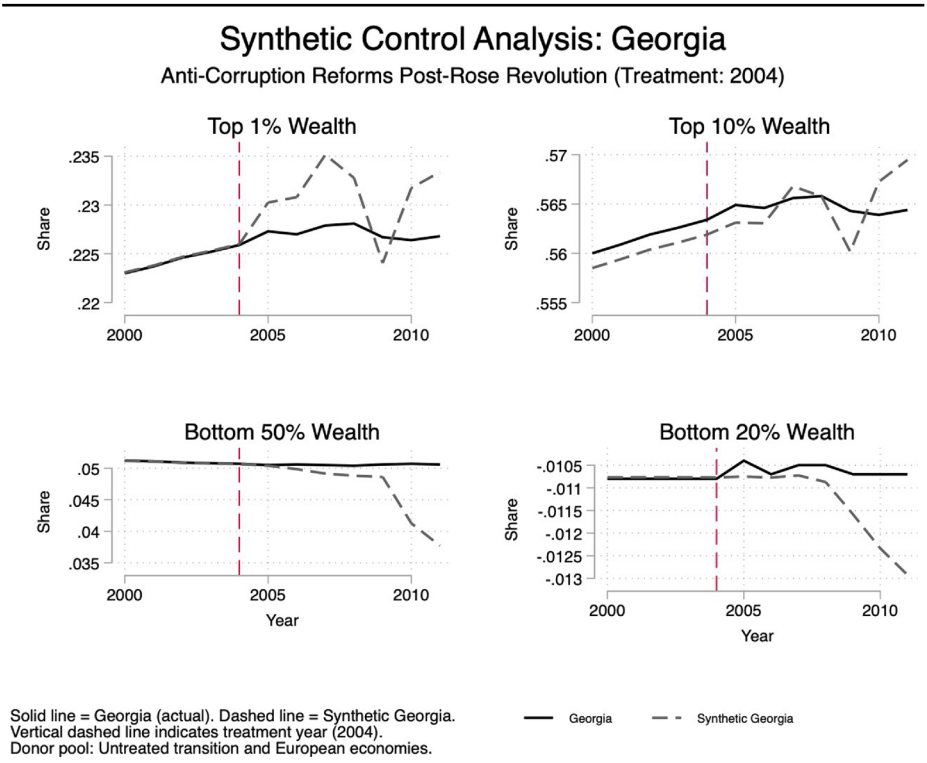

Fig. 2 Synthetic control analysis: Georgia post-Rose Revolution (treatment: 2004) The synthetic control analysis reveals clear distributive effects of the anti-corruption However, beginning around 2012–2013, Georgia experienced what scholars have termed "reform reversal" or "backsliding" (Börzel 2015; Mungiu-Pippidi and Dadasov 2016). As illustrated in Fig. 6 in the Appendix, corruption indicators began declining after 2012, coinciding with political changes following the Georgian Dream coalition's electoral victory. This scaling back of reforms was characterized by several interconnected processes: the gradual subordination of previously independent anti-corruption institutions to political control, the reassertion of informal governance practices alongside formal institutions, and the emergence of new forms of state capture by politically connected economic interests (Cecire 2015; Tudoroiu 2015).

new government. For the top wealth shares, poor pre-treatment fit prevents drawing robust conclusions relative to the counterfactual. However, for bottom wealth shares, the pattern clearly reversed: the bottom 50% share in Georgia stagnated rather than increasing as it would have had reforms persisted, remaining at approximately 4.6% through 2020 while the synthetic control rose. The bottom 20% share exhibited similar stagnation.

The Georgia case thus provides compelling within-country evidence for the causal relationship between anti-corruption reforms and wealth distribution. This pattern of reform reversal illustrates what Grzymala-Busse (2017) terms "captured reform cycles," wherein initial anti-corruption reforms are gradually hollowed out as elites adapt and reconstitute informal networks of influence. As Hellman (1998) noted in his classic analysis of post-communist transitions, powerful beneficiaries of partial reforms often have both the incentive and capacity to block further institutional changes that would limit their advantages. In Georgia's case, while petty corruption remained relatively controlled, grand corruption

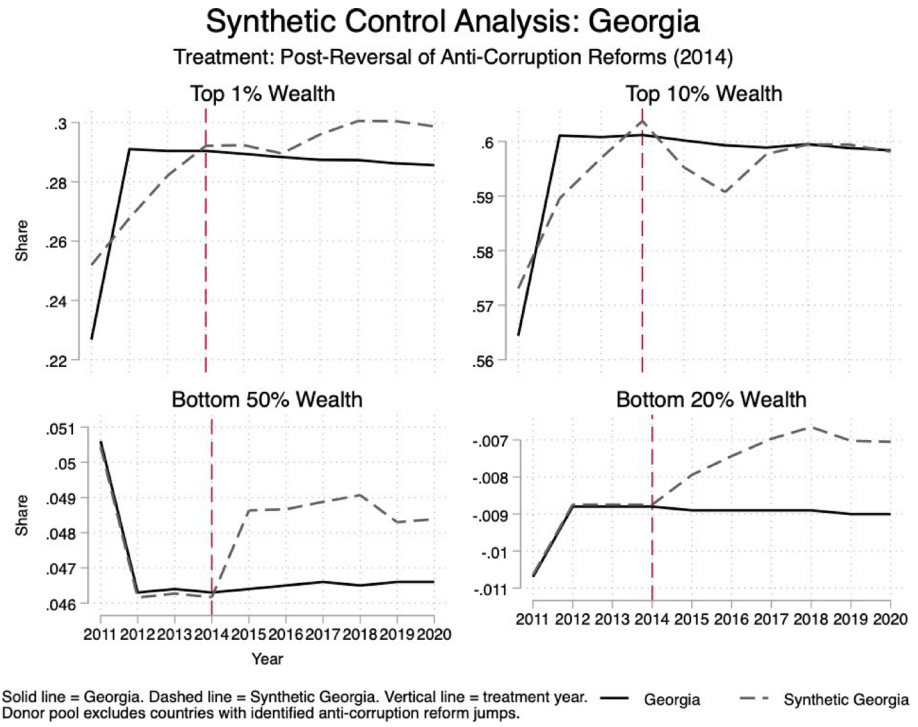

Fig. 3 Synthetic control analysis: Georgia reform reversal (treatment: 2014)

and state capture re-emerged in new forms as political elites consolidated control over key economic sectors (Rekhviashvili and Polese 2020).

4.3 Causal analysis: difference-in-differences results

To estimate the causal effect of anti-corruption reforms on wealth concentration across the full sample, I employ Local Projection Difference-in-Differences (LP-DiD) models that address the well-documented pitfalls of traditional two-way fixed effects estimators in staggered treatment settings. The analysis examines effects across different segments of the wealth distribution: the top 1%, top 10%, bottom 50%, and bottom 20% wealth shares. Standard errors are clustered by macro-region using World Bank classifications, and the control group consists of not-yet-treated and never-treated units.

Figure 4 presents results from the baseline unconditional specification. The plots display estimated treatment effects for ten years following reform implementation, with 95% confidence intervals. The horizontal axis represents years relative to the reform (year 0), while the vertical axis shows effects on wealth shares in percentage points.

The results provide strong evidence that anti-corruption reforms causally affect wealth concentration, with effects that emerge gradually and persist over time. For the top 1% wealth share, reforms lead to a decrease that becomes statistically significant at the 5% level four years after implementation. By year ten, the top 1% share falls by approximately 2 percentage points relative to the counterfactual—a substantively meaningful reduction given

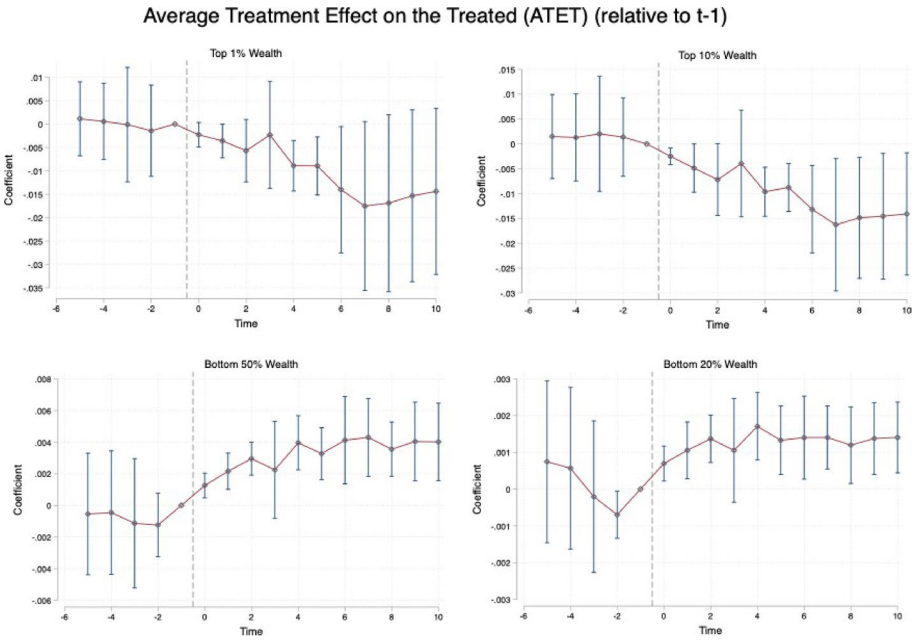

Fig. 4 The effect of anti-corruption reforms on wealth shares (unconditional)

that the average top 1% share in the sample is 30.3%. The effect on the top 10% wealth share is more immediate, achieving statistical significance from year one onward (except year three) and reaching approximately 1.5 percentage points by year ten.

At the bottom of the distribution, reforms produce positive effects on wealth shares. The bottom 50% experiences an immediate increase that is statistically significant for most post-reform years, reaching approximately 0.4 percentage points by year ten. Given that this group holds only 3.6% of wealth on average, this represents a meaningful relative gain of roughly 11%. The bottom 20% follows a similar pattern with smaller magnitude, gaining approximately 0.15 percentage points by year ten. Since this group has a negative mean wealth share (−1.3%, indicating that debts exceed assets), the positive effect suggests that reforms help reduce the debt burden of the poorest households.

To assess the validity of the parallel trends assumption underlying causal identification, I examine pre-treatment placebo estimates for years −5 to −1 in Figure 4. These coefficients are not statistically different from zero across all four outcomes, indicating that treated and control units followed similar wealth distribution trajectories before reforms occurred. This pattern supports the causal interpretation of post-reform effects.

Figure 5 presents results from a conditional specification that includes controls for inflation, national income, and political violence. The post-treatment estimates remain substantively unchanged, demonstrating that the baseline findings are robust to the inclusion of time-varying confounders. The key difference appears in the pre-treatment period: standard errors are larger in the conditional model because controlling for additional covariates makes selection on pre-treatment conditions more precise, reducing the effective sample size for placebo tests. Despite this increased uncertainty in pre-treatment estimates, the coef-

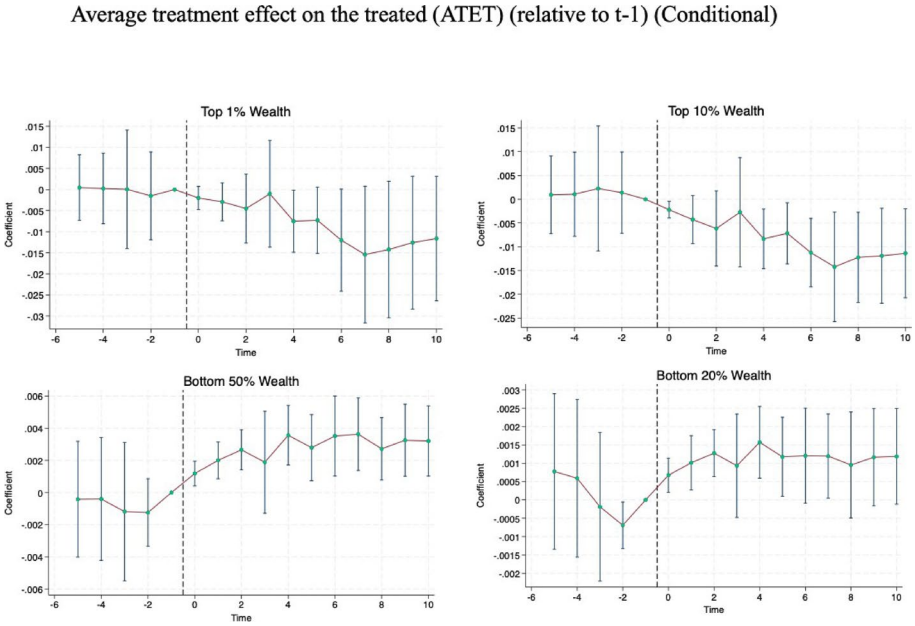

Fig. 5 The effect of anti-corruption reforms on wealth shares (conditional on controls)

ficients remain statistically indistinguishable from zero, providing continued support for the parallel trends assumption.

Taken together, these results support both hypotheses. Anti-corruption reforms reduce wealth concentration at the top (H1A) while increasing wealth shares at the bottom (H1B). The gradual emergence of effects, particularly for the top 1%, is consistent with the theoretical mechanisms outlined above: disrupting elite rent-seeking networks and expanding access to wealth-building institutions are processes that unfold over time rather than producing immediate redistribution. The persistence of effects through year ten suggests that reforms produce durable changes in wealth distribution rather than temporary fluctuations.

4.4 Robustness checks and sensitivity analysis

I conduct several additional analyses to assess the robustness of the main findings, examining sensitivity to treatment threshold definitions, alternative estimators, alternative corruption indicators, and traditional panel specifications.

The first set of robustness checks examines sensitivity to the threshold used for identifying reform "jumps." Lowering the threshold from 1.0 to 0.98 standard deviations expands the sample from 54 to 92 reform episodes but yields weaker and less precisely estimated effects (Fig. 8 in the Appendix). This pattern suggests that only substantial, sustained improvements in corruption control produce meaningful effects on wealth distribution, while more modest changes—which may reflect measurement noise or superficial reforms—do not systematically reshape wealth concentration.

The second set of robustness checks implements alternative difference-in-differences estimators designed to address heterogeneous treatment effects in staggered designs. The

Callaway and Sant'Anna (2021) estimator yields slightly larger negative effects for top wealth shares and similar positive effects for bottom wealth shares, though with fewer statistically significant coefficients due to the more demanding identification requirements of this approach (Fig. 11 in the Appendix). The Wooldridge (2024) estimator produces more conservative point estimates but maintains the same pattern of statistical significance as the main LP-DiD specification (Fig. 10 in the Appendix). The consistency of results across these methodologically distinct approaches provides confidence that the findings are not artifacts of the specific estimator employed.

The third robustness check uses an alternative corruption indicator to identify treatment. I construct reform "jumps" using the V-Dem Public Sector Corruption Index, applying the same identification criteria: improvements exceeding one standard deviation that are sustained for at least five years. This specification identifies fewer reform episodes than the WGI-based measure, as discussed in Section 3. The results, reported in Fig. 9 in the Appendix, show larger standard errors for top wealth shares but robust effects for bottom wealth shares. The overlap between WGI-based and V-Dem-based treatment identification (133 post-reform country-years, as shown in Table 5) and the consistency of results across both indicators provide reassurance that findings are not driven by idiosyncrasies of a particular corruption measure.

Finally, I estimate traditional two-way fixed effects (TWFE) models using the WGI and V-Dem indicators as continuous variables rather than discrete treatment indicators. While these specifications cannot identify the causal effects of discrete reform events and are subject to the well-known biases of TWFE estimators with heterogeneous effects, they provide a useful check on whether the basic correlational patterns align with the causal estimates. The results, presented in Table 6 in the Appendix, confirm that improvements in corruption control are associated with reduced wealth concentration at the top and increased wealth shares at the bottom, consistent with the main difference-in-differences findings. Additional baseline TWFE regressions and detailed panel results are provided in the Electronic Supplementary Material.

5 Discussion and conclusions

This study addresses a theoretically ambiguous question with clear empirical evidence: do anti-corruption reforms reduce wealth inequality? While competing perspectives suggest that corruption may either concentrate wealth among elites or provide informal pathways to economic participation for the poor (Holland 2017; Berggren and Bjørnskov 2020), the findings demonstrate that elite-disrupting effects dominate. Anti-corruption reforms causally reshape wealth distribution by decreasing concentration at the top and increasing wealth shares at the bottom through three key mechanisms: disruption of rent-seeking networks, reduced state capture by wealthy interests, and expanded access to wealth-building institutions.

The results are substantively meaningful. Ten years after reform implementation, the top 1% wealth share decreases by approximately 2 percentage points, while the bottom 50% gains 0.4 percentage points—an 11% relative increase for a group holding only 3.6% of wealth on average. These effects emerge gradually, becoming statistically significant within four years and persisting throughout the observation period. The Georgia case study reinforces these findings: synthetic control analysis shows that reforms following the Rose Revolution prevented increases in top wealth concentration, while reform reversal after 2012 caused these gains to stagnate.

These findings contribute to political economy research by providing causal evidence that institutional reforms shape wealth distribution through market-structuring mechanisms—particularly access to financial services and property rights—distinct from traditional fiscal redistribution. The results help reconcile conflicting findings in the corruption–inequality literature by demonstrating that wealth-specific mechanisms produce clearer distributional effects than income-focused analyses capture.

For policymakers, anti-corruption reforms function as effective instruments for promoting more equitable wealth distribution alongside their primary governance objectives. The gradual emergence of effects underscores the importance of sustained commitment, while the Georgia case illustrates both the potential of ambitious reforms and the risk of reversal when political conditions change.

Limitations persist, particularly regarding wealth measurement in developing countries and the inability to identify which specific reform components drive distributional effects. Future research should investigate reform heterogeneity across political contexts and examine longer time horizons to assess whether effects persist beyond the ten-year window documented here.

Appendix

Note: Additional baseline panel regression tables, detailed Georgia synthetic-control donor-weight tables, and supporting figures are provided in the Electronic Supplementary Material accompanying this article.

Supporting figures

See Figs. 6, 7, 8, 9, 10, and 11

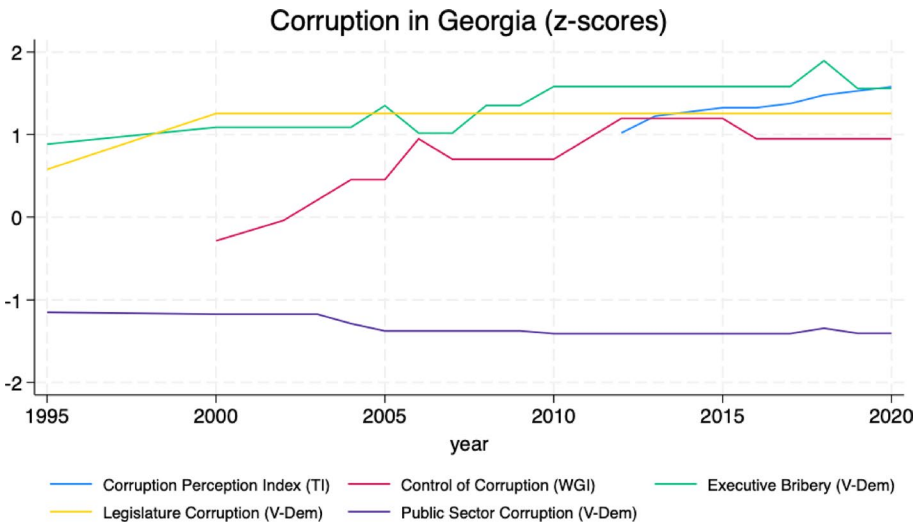

Fig. 6 Corruption indicators in Georgia

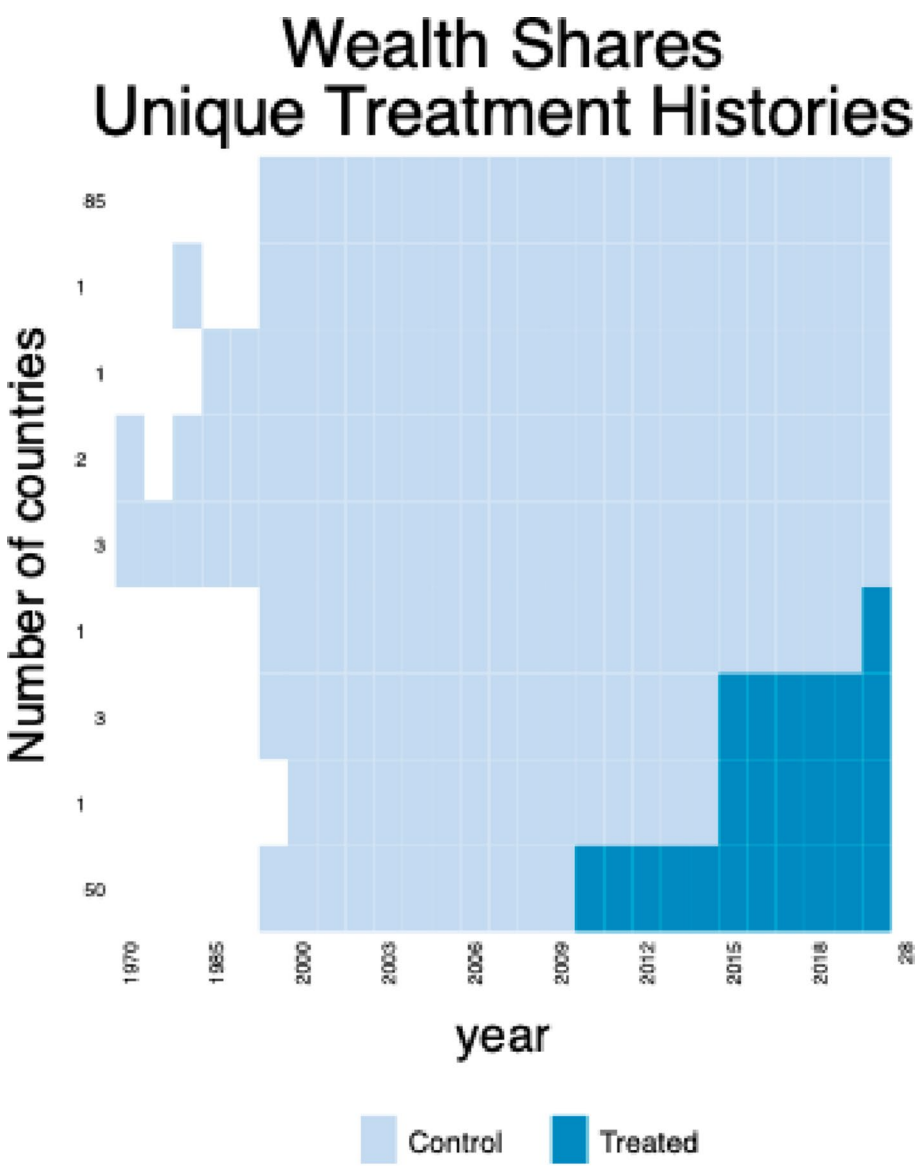

Fig. 7 Wealth shares and treatment histories

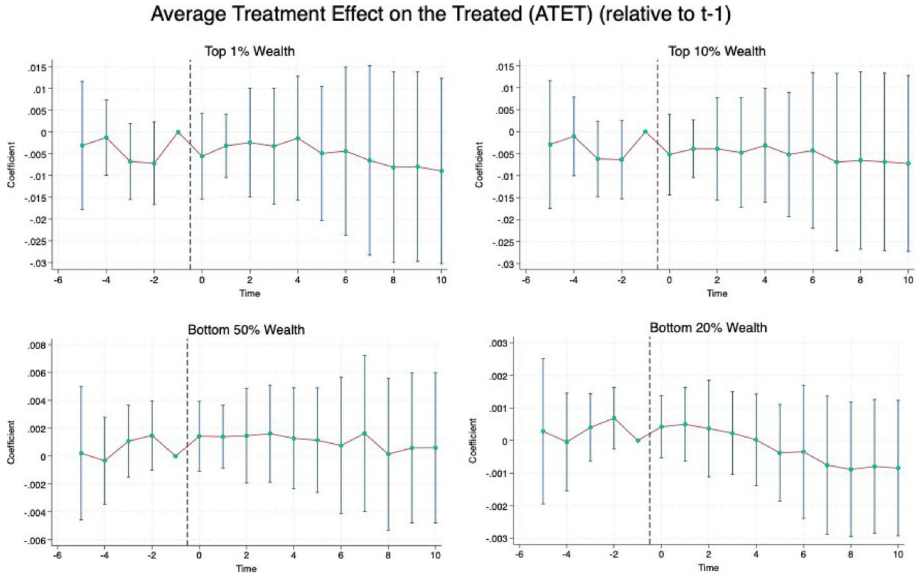

Fig. 8 Sensitivity analysis at 0.98 SD threshold. LP-DiD estimates of the effect of anti-corruption reforms on wealth shares

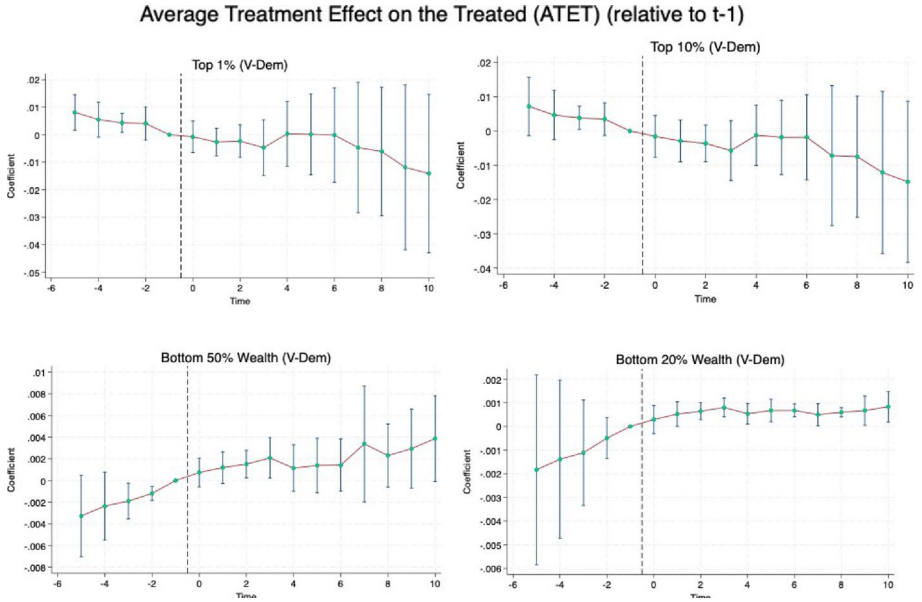

Fig. 9 The effect of anti-corruption reforms on wealth shares (V-Dem indicator, unconditional)

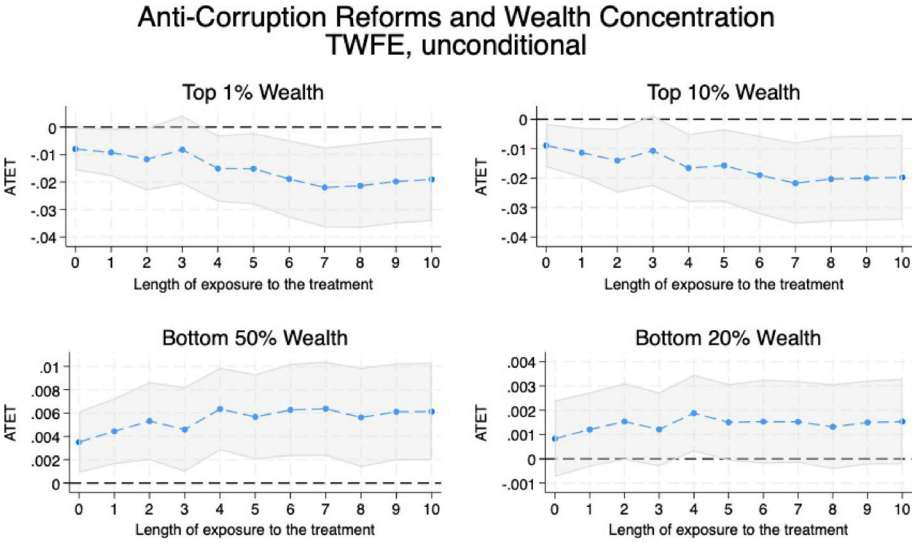

Fig. 10 Wooldridge (2024) Mundlak estimates. Pre-treatment periods are not used in this method

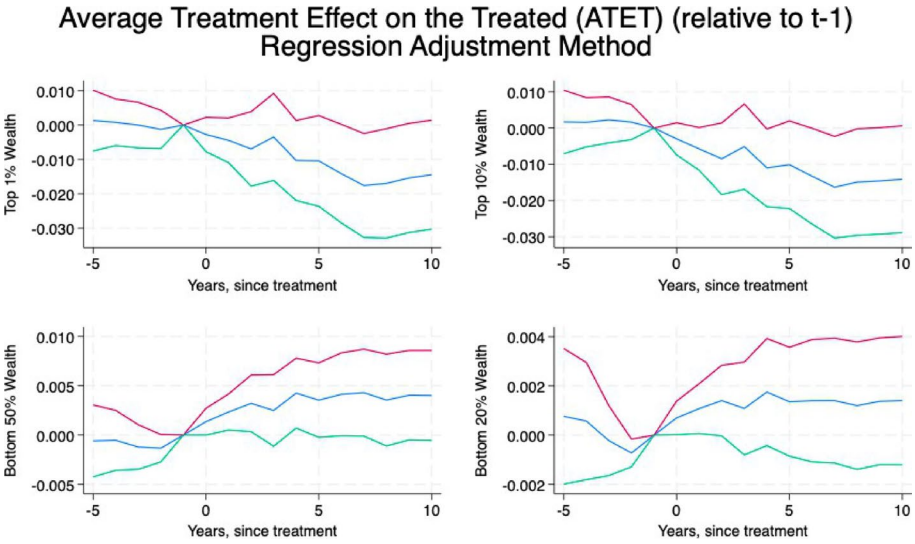

Fig. 11 Regression-adjustment estimates, following Callaway and Sant'Anna (2021)

Synthetic control: pre-treatment fit

Table 2 presents the root mean squared prediction error (RMSPE) for each synthetic control analysis. Lower values indicate better pre-treatment fit between Georgia and its synthetic counterpart.

See Tables 2, 3, and 4

Table 2 Synthetic control pre-treatment fit: root mean squared prediction error (RMSPE)

Outcome variable	Reform period (2000–2011)	Reversal period (2011–2020)
Top 1% wealth share	2.99×10^{-12}	0.0176
Top 10% wealth share	3.01×10^{-11}	0.0075
Bottom 50% wealth share	1.12×10^{-12}	1.87×10^{-13}
Bottom 20% wealth share	1.24×10^{-13}	1.02×10^{-13}

Notes: RMSPE measures the discrepancy between Georgia and synthetic Georgia in the pre-treatment period. The treatment year is 2004 for the reform period and 2014 for the reversal period. The donor pool consists of 25 countries: Albania, Armenia, Austria, Belgium, Croatia, the Czech Republic, Denmark, Finland, Germany, Greece, Italy, Kazakhstan, Latvia, Lithuania, the Netherlands, Norway, Poland, Portugal, Romania, Russia, Spain, Sweden, Switzerland, Ukraine, and the United Kingdom

Table 3 Predictor balance: Georgia reform period (2000–2011, treatment: 2004)

Outcome	Predictor (year)	Treated	Synthetic
Top 1% wealth	top1wealth(2000)	0.2230	0.2231
	top1wealth(2001)	0.2237	0.2238
	top1wealth(2002)	0.2246	0.2247
	top1wealth(2003)	0.2252	0.2253
	top1wealth(2004)	0.2259	0.2260
Top 10% wealth	top10wealth(2000)	0.5600	0.5585
	top10wealth(2001)	0.5609	0.5594
	top10wealth(2002)	0.5619	0.5604
	top10wealth(2003)	0.5626	0.5611
	top10wealth(2004)	0.5634	0.5619
Bottom 50% wealth	bottom50wealth(2000)	0.0512	0.0512
	bottom50wealth(2001)	0.0511	0.0511
	bottom50wealth(2002)	0.0509	0.0509
	bottom50wealth(2003)	0.0508	0.0508
	bottom50wealth(2004)	0.0507	0.0507
Bottom 20% wealth	bottom20wealth(2000)	−0.0108	−0.0108
	bottom20wealth(2001)	−0.0108	−0.0108
	bottom20wealth(2002)	−0.0108	−0.0108
	bottom20wealth(2003)	−0.0108	−0.0108
	bottom20wealth(2004)	−0.0108	−0.0108

Notes: Predictor balance shows excellent pre-treatment fit for all outcome variables in the reform period, with synthetic values closely matching actual Georgia values

Table 4 Predictor balance: Georgia reversal period (2011–2020, treatment: 2014)

OutcomePredictor (year)	Treated	Synthetic
top1wealth(2011)	0.2268	0.2519
$_{\text{Top 1\% wealth}}$top1wealth(2012)	0.2910	0.2678
top1wealth(2013)	0.2904	0.2821
top1wealth(2014)	0.2904	0.2921
top10wealth(2011)	0.5644	0.5731
$_{\text{Top 10\% wealth}}$top10wealth(2012)	0.6011	0.5896
top10wealth(2013)	0.6008	0.5970
top10wealth(2014)	0.6012	0.6038
bottom50wealth(2011)	0.0506	0.0504
$_{\text{Bottom 50\% wealth}}$bottom50wealth(2012)	0.0463	0.0462
bottom50wealth(2013)	0.0464	0.0463
bottom50wealth(2014)	0.0463	0.0462
bottom20wealth(2011)	− 0.0107	− 0.0106
$_{\text{Bottom 20\% wealth}}$bottom20wealth(2012)	− 0.0088	− 0.0087
bottom20wealth(2013)	− 0.0088	− 0.0087
bottom20wealth(2014)	− 0.0088	− 0.0087

Notes: Pre-treatment fit is excellent for bottom wealth shares but weaker for top wealth shares in the reversal period. The discrepancy between treated and synthetic values for top 1% and top 10% in 2011–2012 reflects the structural break in Georgia's wealth distribution following the 2012 political transition, which limits the reliability of counterfactual inference for these outcomes

Supporting tables

See Tables 5 and 6

Table 5 Cross-tabulation of corruption jumps: WGI and V-Dem.

V-Dem jump (1 SD)	WGI jump (1 SD)		Total
	No	Yes	
No	2,499	442	2,941
Yes	169	133	302
Total	2,668	575	3,243

Notes: The table shows the frequency distribution of jumps in V-Dem and WGI corruption indicators (1-standard-deviation improvement sustained over time).

Table 6 Panel data estimations, conditional on controls

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Top 1% of wealth	Top 10% of wealth	Bottom 50% of wealth	Bottom 20% of wealth	Top 1% of wealth	Top 10% of wealth	Bottom 50% of wealth	Bottom 20% of wealth
Public corruption (V-Dem)					-0.005	-0.008	0.003**	0.002**
Top 1% Income (Share)	0.633***	0.583***	-0.161***	-0.057***	0.665***	0.609***	-0.165***	-0.060***
	(0.107)	(0.100)	(0.030)	(0.013)	(0.091)	(0.083)	(0.025)	(0.011)
Inflation, %	-0.000*	-0.000**	0.000	0.000	-0.000	-0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
National income	0.000***	0.000***	-0.000***	-0.000	0.000***	0.000**	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Index of Political Violence (V-Dem)	0.001	0.000	0.001	-0.000	0.002	0.002	0.000	0.000
	(0.002)	(0.002)	(0.001)	(0.000)	(0.002)	(0.002)	(0.001)	(0.000)
Control of Corruption (WGI)	-0.015*	-0.015**	0.007***	0.003**	0.240***	0.635***	0.038***	-0.012
	(0.008)	(0.007)	(0.003)	(0.001)				
Constant	0.200***	0.538***	0.063***	-0.004**				
	(0.017)	(0.016)	(0.005)	(0.002)	(0.016)	(0.038)	(0.013)	(0.009)
Country fixed-effects	YES	YES	YES	YES	YES	YES	YES	YES
Year dummies	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2,139	2,139	2,139	2,139	2,451	2,451	2,451	2,451
R-squared	0.197	0.243	0.196	0.087	0.225	0.246	0.181	0.114
Number of countries	142	142	142	142	142	142	142	142

Robust standard errors in parentheses

***p <0.01, ** p <0.05, *p <0.1

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Author contributions Conceptualization, Methodology, Formal analysis, Investigation, Data curation, Writing—original draft, Writing—review and editing, and Visualization were carried out by the sole author of this work.

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Data availability The data that support the findings of this study are openly available from the World Inequality Database at https://wid.world, the World Bank World Governance Indicators at https://info.worldbank.org/governance/wgi/, and the V-Dem Institute at https://www.v-dem.net/. Replication code and materials are available from the author upon reasonable request.

Declarations

Competing interests The author has no relevant financial or non-financial interests to disclose.

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