EU Accession, Anti-Corruption Reforms, and Income Inequality: Evidence from Bulgaria, Croatia, and Romania

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Abstract

This paper applies a two-part empirical strategy to investigate whether EU accession-driven anti-corruption reforms reduced income inequality in Romania, Bulgaria, and Croatia. Using World Bank Enterprise Survey data on firm-level corruption matched to SWIID inequality measures, the analysis first documents the association between corruption and inequality, then employs a difference-in-differences design with EU accession as a quasi-natural experiment. Results suggest that while accession reduced bribery incidence, the effects on income inequality are driven primarily by Bulgaria's independent upward inequality trajectory rather than by accession itself. Romania shows the most promising evidence that institutional reform can coexist with inequality reduction, while Croatia's recent accession limits inference.

Introduction

Does externally imposed institutional reform reduce income inequality? The expansion of the European Union provides a compelling setting in which to investigate this question. EU accession conditionality requires candidate states to adopt comprehensive anti-corruption legislation, backed by unprecedented monitoring mechanisms — making accession one of the most comprehensive external reform programs in modern history. If corruption worsens inequality through inefficient allocation of public resources, regressive bribery, and elite rent-seeking (Gupta, Davoodi & Alonso-Terme, 2002), then reforms that successfully reduce corruption should, in principle, also reduce inequality. This paper tests whether that prediction holds in practice.

Examining three late-accession countries — Romania and Bulgaria (both acceded in 2007) and Croatia (acceded in 2013) — I estimate the causal effect of EU accession on income inequality, focusing on the anti-corruption channel. These countries are particularly informative because they were subject to monitoring mechanisms that extended well beyond their accession dates: the Cooperation and Verification Mechanism (CVM) for Romania and Bulgaria, and the Chapter 23 closing benchmarks for Croatia. These mechanisms required the implementation of specific, measurable anti-corruption

requirements, providing a clear policy treatment whose effects on both corruption and inequality can be assessed. Given that accession to the EU is itself corruption-alleviating in nature, these effects might vary with the underlying institutional and economic conditions in each country. For example, states with higher pre-existing corruption could experience greater reductions in inequality following reform, or alternatively, the structural drivers of income inequality in post-communist countries might dominate the corruption channel entirely; with the overall effect on inequality therefore being ambiguous ex-ante.

The analysis proceeds in two stages, each addressing a distinct question. First, is the corruption–inequality channel operative in these economies? If higher corruption is not associated with higher inequality in these specific countries, then the hypothesis that anti-corruption reform could reduce inequality through this channel would lack empirical grounding. Using World Bank Enterprise Survey (WBES) data on three corruption indicators — bribery incidence, bribery depth, and the share of firms identifying corruption as a major constraint — matched to annual SWIID Gini coefficients and WID top 10% income shares, I estimate country fixed effects regressions on the WBES panel data. The results confirm a positive association: a one percentage point increase in bribery depth corresponds to approximately a 0.33-point increase in the Gini coefficient ($p=0.095$), and the corruption constraint measure is also positively associated with inequality ($p=0.052$). While these associations are only marginally significant given the small sample (13 country-year observations), they are directionally consistent with the theoretical predictions and the broader cross-country literature, establishing that the corruption channel is relevant in this context.

Second, and centrally: did EU accession reduce income inequality through this channel? I employ a difference-in-differences analysis on the annual panel spanning 2000–2023, comparing the three treatment countries to the broader Europe and Central Asia (ECA) regional average. Within the treatment countries, EU accession shows no significant effect on inequality. When compared to the ECA average, accession is associated with a rise in inequality of approximately 3.5 Gini points ($p=0.012$). However, this result is driven mainly by country-specific linear time trends — particularly Bulgaria's sustained increase in the Gini coefficient at roughly half a Gini point per year throughout the sample period — rather than by a causal effect of EU accession itself. Once country-specific trends are included, the accession effect vanishes entirely ($\beta=-7.40$, $p=0.916$). In an environment where the WBES panel is highly unbalanced and limited to 13 reported indicator observations, I include robustness checks through placebo tests and alternative specifications to increase confidence in these findings.

These findings carry implications for EU enlargement policy. The CVM and Chapter 23 frameworks appear to have succeeded in reducing corruption, but this success did not translate into measurable inequality reduction. The disconnect suggests that the corruption–inequality channel, while theoretically and empirically relevant, is second-order relative to other structural determinants of inequality in these transition economies. Hence, my results use publicly available data to contribute to the literature not primarily by re-establishing the corruption–inequality correlation, but by showing that one of the most comprehensive anti-corruption reform programs in modern history failed to produce detectable effects on income distribution. While the underlying economic variables used (e.g. corruption indicators, the ECA control group, country-specific trends) offer a good

initial starting point, they are not meant to offer an exhaustive analysis of all the channels through which EU accession might affect income inequality, as the transmission mechanism is undoubtedly affected by many other variables such as but not limited to labor market restructuring, capital account liberalization, and worker skill premia. Finally, while my results suggest that anti-corruption reforms and inequality reduction require separate policy strategies, they should also act as a cautionary tale with regard to the existing structural economic forces on the distributional concerns of institutional reform, should they exist.

The remainder of this paper is organized as follows. Section 2 reviews the theoretical and empirical literature on corruption, inequality, and EU accession. Section 3 describes the institutional context. Section 4 presents the data sources and variable operationalization. Section 5 details the empirical strategy. Section 6 presents the main results. Section 7 extends the analysis to heterogeneity by firm size and sector. Section 8 presents robustness checks. Section 9 discusses the findings, and Section 10 concludes.

Existing Literature

Corruption and Inequality

The theoretical relationship between corruption and income inequality operates through several channels. Gupta, Davoodi & Alonso-Terme (2002) identify three primary mechanisms: corruption distorts tax progressivity by enabling tax evasion among the wealthy elites; it distorts public expenditure away from redistributive programs towards

more capital-intensive projects which are amenable to rent extraction by those elites; and it reduces the quality of public services upon which lower-income households disproportionately depend. Apergis, Dincer & Payne (2010) formalize these intuitions in a model where corruption acts as a regressive transfer from the poor to the politically connected elites.

There is ample research on this relationship - both empirical and theoretical. Perhaps not surprisingly, these employ a variety of methodologies and arrive at different conclusions. This alone suggests that the relationship between corruption and inequality is perhaps much deeper and more complex than previously considered. Cross-country empirical evidence broadly supports a positive relationship: Gyimah-Brempong (2002) finds that corruption increases income inequality across African countries, where Li, Xu & Zou (2000) document a non-linear relationship in which corruption increases inequality mostly at intermediate levels of development, and Jong-Sung & Khagram (2005) identify the existence of a feedback loop where inequality increases corruption by strengthening 'elite-capture' of institutions. More recently, panel evidence from Apergis, Dincer & Payne (2010) confirms the positive corruption-inequality nexus using instrumental variables approaches. While this literature establishes the relevance of the corruption–inequality channel in cross-country settings, the question of whether specific policy interventions that reduce corruption also reduce inequality remains largely unanswered — and it is this question that the present study addresses.

EU Accession and Institutional Reform

The EU accession process represents one of the most comprehensive external institutional reform programs in modern history. Schimmelfennig & Sedelmeier (2005) document how the 'credible conditionality' of EU membership candidates induced reforms in states which would not have had them implemented through domestic political impetus alone. Vachudova (2005) emphasizes the distinction between 'passive leverage' (the attractiveness of EU membership accession) and 'active leverage' (specific reform requirements for said accession), arguing that the latter was the decisive force in post-communist democratization. However, the effectiveness of EU conditionality in producing deep institutional reform - as opposed to formal, surface-level compliance - remains debatable. On this front, Noutcheva (2009) argues that compliance was often 'fake' or only 'partial,' with candidate countries adopting laws without necessarily changing the enforcement of these laws in practice in any meaningful way. Sedelmeier (2012) also adds that post-accession compliance significantly deteriorates, which would suggest that conditionality effects were contingent on continued membership 'leverage.' This last point is of particular relevance to the present study, seeing as how the CVM was designed precisely to maintain this leverage even after accession.

Post-Communist Inequality

The post-communist transition produced dramatic increases in income inequality across Central and Eastern Europe. Milanovic (1999) documents how the collapse of the socialist wage compression regime, combined with privatization and labor market liberalization generated rapid inequality increases throughout the 1990s. Bandelj & Mahutga (2010) find

that EU accession itself may have contributed to rising inequality through trade liberalization and capital mobility, despite having improved governance indicators. Bohle & Greskovits (2012) identify distinct 'varieties of capitalism' across post-communist states, with Southeastern European countries (including Romania and Bulgaria) exhibiting particularly weak social protection systems which exacerbate existing inequalities. This literature highlights a central tension for the present study: EU accession may simultaneously reduce corruption through conditionality and increase inequality through economic integration, with the net effect depending on which channel dominates. The contribution of this paper is to estimate whether the net effect of accession on inequality is detectable, and whether the anti-corruption channel plays a measurable role.

Institutional Background

The Accession Process

In this regard, the stories of the three countries diverge significantly. Romania and Bulgaria submitted EU membership applications in 1995 and began accession negotiations in 2000. Both countries became members on January 1, 2007, but with the unprecedented condition that the anti-corruption and judicial reform processes would continue to be monitored through the Cooperation and Verification Mechanism. Croatia in turn applied for membership as early as 2003, opened negotiations in 2005, and became a member on July 1, 2013. Croatia's accession was subject to rigorous Chapter 23 (Judiciary and Fundamental Rights) closing benchmarks which required specific anti-corruption measures to be taken prior to accession. At this juncture, it is worthwhile to note that the staggered accession dates

provide some variation for identification purposes, though the limited number of treatment units (three) constrains the analysis in important ways that are addressed in the subsequent sections.

The Cooperation and Verification Mechanism

The CVM was established by the European Commission in December 2006. For Romania, it set four benchmarks covering 1) judicial independence, 2) anti-corruption agency effectiveness, 3) high-level corruption prosecution, and 4) integrity in vulnerable sectors. For Bulgaria, it set six benchmarks spanning 1) judicial reform, 2) organized crime, 3) anti-corruption legislation, 4) high-level corruption cases, 5) border corruption, and 6) fraud against EU funds. The Commission issued annual or biannual progress reports assessing compliance with these measures, with the implicit threat of triggering the EU Treaty's safeguard clauses if compliance wasn't met. In practice, however, the CVM's enforcement turned out to be limited in nature. No safeguard clauses were ever invoked (despite repeated reporting of stalled compliance in progress reports), and the mechanism relied mainly on reputational pressure and the continued withholding of Schengen area accession. Bulgaria was assessed as having met all CVM benchmarks in 2019 where Romania's CVM was formally closed in 2023.

Croatia and Chapter 23

Croatia's accession process incorporated lessons from the Romanian and Bulgarian experience. The Chapter 23 closing benchmarks required Croatia to demonstrate effective prosecution of corruption cases, establish asset declaration systems, and implement conflict-

of-interest legislation before accession was finalized. Unlike the CVM, Croatia's benchmarks operated as genuine gatekeeping conditions, as accession could not proceed until they were met.

Data & Methodology

Corruption Measures

Corruption is measured using three key indicators (bribery incidence, bribery depth, and corruption constraint) taken from the World Bank Enterprise Surveys (WBES), which survey representative samples of formal-sector firms in each country. 'Bribery incidence' measures the percentage of firms expected to give gifts or make informal payments to 'get things done' with public officials. 'Bribery depth' captures the percentage of public transactions in which a gift or informal payment was requested from the reporting firm. 'Corruption constraint' reports the percentage of firms identifying corruption as a major or 'very severe' obstacle to their operations each year. The WBES panel is highly unbalanced by nature: Bulgaria was surveyed in 2007, 2009, 2013, 2019, and 2023 (5 waves); Romania in 2009, 2013, 2019, and 2023 (4 waves); and Croatia in 2007, 2013, 2019, and 2023 (4 waves). This yields 13 country-year observations. More critically, only Croatia has a pre-accession observation (2007) among these three, which seriously limits the identification of within-country accession effects for the purpose of this study.

It is worth noting that the WBES contains rich firm-level microdata that goes well beyond the aggregated country-level indicators used in this study. At the individual firm level, the

surveys collect detailed information on bribery experiences, the frequency and size of informal payments, perceptions of corruption as a business obstacle, as well as firm characteristics such as size, sector, ownership structure, age, and export status. For any given survey round, this can amount to hundreds of firm-level observations per country (the average in this sample being 382 firms per round, as shown in Table 1). However, seeing as how the publicly available data reports only aggregated indicators at the country-year level rather than the underlying firm-level responses, the analysis here is conducted on these aggregated measures. This aggregation, while necessary given the public data constraints, means that within-country heterogeneity in corruption experiences across firms is not directly exploited in the main regressions. As noted in the concluding section, future work using the confidential firm-level microdata could exploit this within-country variation to provide more granular estimates of the corruption–inequality relationship.

Inequality Measures

The primary inequality measure is the SWIID Gini coefficient (v9.91) of disposable income inequality from the Standardized World Income Inequality Database (Solt, 2020). Seeing as how the SWIID uses Bayesian methods to standardize observations from a wide variety of sources (including the OECD Income Distribution Database, Eurostat, and national statistical offices), this database achieves the utmost comparability of the income inequality data that is freely available for the largest possible sample of countries and years. In the sample, the Gini ranges from 26.8 (Croatia, 2007) to 39.7 (Bulgaria, 2023). As a secondary measure, the top 10%'s share of pre-tax income from the World Inequality Database (WID) was also used, ranging from 30.9% (Croatia, 2003) to 45.3% (Romania, 2007).

Control Variables and Control Group

The control variables (log GDP per capita (constant USD), trade openness, government expenditure, unemployment, FDI net inflows, and gross tertiary enrollment) were drawn from the World Development Indicators database. In the WBES panel regressions (Table 2), no additional controls are included: with only 13 observations split across a 2×2 difference-in-differences design comparing Croatia to Romania and Bulgaria, the group dummy already absorbs the between-group level difference, and adding GDP or other covariates would quickly exhaust degrees of freedom. The full set of controls is thus reserved for the annual panel only (where N=72). For the main DiD analysis (Table 4), the Europe and Central Asia (ECA) regional average serves as the control group.[18]

Descriptive Statistics

Table 1 displays the descriptive statistics for the WBES panel of 13 country-year observations across the three treatment countries. The sample consists of survey rounds with an average bribery incidence of 7.8 percent (ranging from 0.9 to 19.7 percent), an

[18] The ECA regional average is computed from the following 27 countries: Albania, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Czech Republic, Estonia, Georgia, Hungary, Kazakhstan, Kosovo, Kyrgyz Republic, Latvia, Lithuania, Moldova, Montenegro, North Macedonia, Poland, Russia, Serbia, Slovak Republic, Slovenia, Tajikistan, Turkey, Turkmenistan, Ukraine, and Uzbekistan. Of these, eight (Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Slovak Republic, and Slovenia) acceded to the EU in 2004. These countries are retained in the ECA average because the regional benchmark serves as a broad comparator capturing common inequality trends rather than a strict "never-treated" control group, and their accession predates the treatment dates in this study by 3-9 years, meaning any accession-related effects on their inequality trajectories are already absorbed into the regional trend well before the treatment switches on for Romania, Bulgaria, and Croatia. Excluding them would arguably shift the ECA average toward Central Asian and post-Soviet economies that are structurally less comparable to the treatment countries.

average bribery depth of 5.4 percent, and with approximately 29 percent of firms identifying corruption as a major or 'very severe' constraint on their business operations. On the inequality side, the SWIID Gini averages 31.7 on the standard 0-100 scale (with Bulgaria reaching 39.7 by 2023), while the top 10% income share averages 38.1 percent. Already at a first glance, we see that there is ample variation in both corruption and inequality across the three countries and over time (Figure 1), making the sample suitable for the analysis that follows.

Empirical Strategy

The empirical strategy is organized around the paper's central question: does EU accession reduce income inequality through the anti-corruption channel? Answering this requires two steps. First, I establish that the corruption–inequality channel is operative in these economies — a necessary condition for the hypothesis to be viable. Second, I estimate the causal effect of EU accession on inequality using a difference-in-differences design, which constitutes the paper's primary identification strategy.

Motivating Evidence: Corruption and Inequality (Tables 2–3)

Before turning to the causal analysis, it is worthwhile to first examine whether EU accession produced differential corruption outcomes across the treatment countries. Seeing as how Croatia acceded in 2013 — six years after Romania and Bulgaria in 2007 — I exploit this staggered timing to construct a 2×2 difference-in-differences design, where Romania and Bulgaria serve as the control group, Croatia as the treated unit, and the post-2013 period as

the treatment window. The specification for Table 2 is as follows:

$$Corruption_{it} = \alpha + \beta_1 \text{Post2013}_t + \beta_2 \text{Croatia}_i + \beta_3(\text{Post2013}_t \times \text{Croatia}_i) + \varepsilon_{it}$$

where Post2013_t equals one for survey waves conducted in 2013 or later, Croatia_i equals one for Croatian observations, and β_3 is the difference-in-differences estimator — that is, it captures whether Croatia's corruption trajectory diverged from that of Romania and Bulgaria after 2013. I use HC1 (heteroskedasticity-robust) standard errors throughout. It is worth noting that the specification intentionally omits GDP controls and country fixed effects; with only 13 observations split across a 2×2 design, the Croatia dummy already absorbs the between-group level difference, and adding further controls would quickly exhaust the available degrees of freedom. Hence, these results should be interpreted as descriptive DiD estimates, albeit informative ones, rather than fully identified causal effects.

For Table 3, I estimate the relationship between corruption and inequality in the following manner:

$$Inequality_{it} = \alpha_i + \delta \text{Corruption}_{it} + \gamma \ln(\text{GDP/cap})_{it} + \varepsilon_{it}$$

Together, these regressions establish whether the corruption channel is relevant in this setting, motivating the causal analysis that follows. For Table 3, models are weighted by the number of firms surveyed, as larger survey rounds provide more precise corruption measures in this case. Since the publicly available WBES data reports aggregated indicators from firm-level microdata, precision-weighting by the number of underlying observations is deemed the appropriate approach to account for any possible variation in measurement quality across survey rounds.

Primary Identification: EU Accession and Inequality (Table 4)

The main identification strategy uses a difference-in-differences design on an annual panel spanning 2000–2023. I estimate three specifications, which differ in sample composition and identifying assumptions.

Within-treatment analysis (Columns 1–2)

Using only the three treatment countries (N=72), I estimate:

$$Inequality_{it} = \alpha_i + \lambda_t + \beta \cdot Post_{it} + X_{it}'\gamma + \varepsilon_{it}$$

where α_i are country fixed effects, λ_t are year fixed effects, and X_{it} includes log GDP per capita, trade openness, government expenditure, unemployment, FDI, and tertiary enrollment. $Post_{it}$ equals one if country i has acceded to the EU by year t — that is, $Post_{it} = 1$ for Romania and Bulgaria from 2007 onward and for Croatia from 2013 onward. β is identified from the staggered timing of accession: Croatia's later switch from 0 to 1 relative to Romania and Bulgaria provides the variation, conditional on country and year fixed effects.

Comparison with ECA control group (Columns 3–4)

To benchmark the treatment countries against a broader reference, I expand the panel to include the Europe and Central Asia regional average as a fourth unit (N=96) and estimate:

$$Inequality_{it} = \alpha_i + \lambda_t + \delta \cdot (Post_{it} \times Treated_i) + X_{it}'\gamma + \varepsilon_{it}$$

where $Treated_i$ equals one for the three accession countries and zero for the ECA average, and $Post_{it}$ is defined as before (with Post = 0 for ECA in all years, since it does not undergo

accession). The coefficient δ captures the change in inequality in the treatment countries after accession relative to the change in the ECA control group over the same period.

Trend-adjusted specification (Column 5)

Returning to the treatment-only panel, I add country-specific linear time trends to address the possibility that divergent pre-treatment inequality trajectories — particularly Bulgaria's sustained upward trend — bias the baseline estimates:

$$Inequality_{it} = \alpha_i + \lambda_t + \beta \cdot Post_{it} + X_{it}'\gamma + \theta_i \cdot t + \varepsilon_{it}$$

where $\theta_i \cdot t$ allows each country to follow its own linear trajectory, and β captures any deviation from that trajectory attributable to accession. Heteroskedasticity robust standard errors are used throughout, as the small number of clusters (3-4) renders cluster-robust inference unreliable.

Results

Descriptive Evidence

Figure 1 provides descriptive evidence on corruption and inequality trajectories. Panel (a) shows that bribery incidence declined substantially in Romania, from 19.7% in 2009 to 2.7% by 2023 - a significant reduction that is among the most pronounced in the region. Bulgaria on the other hand exhibits a fluctuating pattern, peaking at 10.9% in 2009 before declining to 0.9% by 2023. Croatia shows the least clear trend, with bribery incidence rising from 4.9% in 2007 to 10.1% in 2019 before falling to 6.8% in 2023. The ECA regional average (5.7% in

2023) adds some context: by 2023, Romania and Bulgaria had reduced bribery incidence below the regional benchmark, while Croatia remained slightly above it.

Panels (b) and (c) reveal divergent inequality trajectories. Bulgaria's Gini coefficient rose substantially from 32.8 in 2000 to 39.7 in 2023, overtaking the ECA average by an increasing margin. Romania's Gini coefficient fluctuated around 30-32 with no clear trend throughout the time period considered. Finally, Croatia remained stable around the 27-30 level marks. The ECA average was essentially flat at approximately 34-35. These divergent trajectories - and particularly Bulgaria's persistent upward trend - pose an underlying challenge for the DiD analysis, calling to mind the importance of the parallel trends assumption in such analyses. Despite this limitation, I start exploring the econometric results in the following sub-section.

Motivating Evidence: EU Accession and Corruption (Table 2)

As Table 2 shows, the Post_{2013} coefficient is negative across all three corruption measures, suggesting that Romania and Bulgaria experienced substantial declines in corruption after 2013 — a pattern that is in line with the continued pressure exerted by the Cooperation and Verification Mechanism. More specifically, bribery incidence fell by approximately 9.5 percentage points ($\beta=-9.49$, $p<0.05$), bribery depth by about 5.0 percentage points ($\beta=-4.97$, $p<0.10$), and the corruption constraint index declined by 17.2 points ($\beta=-17.23$, $p<0.10$). The DiD interaction term, however, tells a different story. It is positive and significant for both bribery incidence ($\beta_3=11.82$, $p<0.05$) and bribery depth ($\beta_3=6.49$, $p<0.05$), which is to say that Croatia did not experience comparable corruption reductions following its own accession. Put simply, the net effect for Croatia after 2013 is approximately

−9.49+11.82≈+2.3 percentage points for bribery incidence; corruption essentially did not improve relative to the pre-accession baseline. For the corruption constraint, the DiD coefficient is positive but insignificant (β_3=12.71, p=0.15), pointing to a similar — though statistically weaker — pattern. At this juncture, it could be said that these results are consistent with the argument that Croatia lacked the sustained post-accession monitoring that the CVM provided for Romania and Bulgaria, and that EU accession alone, without continued conditionality, may not be sufficient to sustain anti-corruption momentum. Bearing these motivating patterns in mind, I now turn to the question of whether the corruption–inequality channel is in fact relevant in these economies — a necessary condition for the hypothesis that anti-corruption reform could reduce inequality.

Corruption and Inequality (Table 3)

Table 3 presents the results on the main outcome variables. All three corruption indicators carry positive coefficients, which is in line with the theoretical predictions. While Bribery Incidence is positive but insignificant, Bribery Depth is marginally significant (p=0.095). Put simply, a one percentage point increase in bribery depth is associated with approximately a 0.33 Gini point increase. The corruption constraint is also marginally significant (p=0.052) and yields roughly a 0.15 point increase in Gini because of a percentage point increase in 'corruption constraint.' Although different in statistical precision and magnitude, these results are directionally consistent across all three corruption measures: inequality seems to be increasing in the reported corruption measures.

Additional results (not shown for brevity) replicate the analysis with the top 10% income share as the dependent variable. Coefficients in this case are also uniformly positive, but

none of them are statistically significant, with bribery depth showing the largest effect (β=0.0055, p=0.178). The weaker results for the top 10% share may reflect that corruption affects the middle and bottom parts of the income distribution more so than the very top, implying that corruption functions as a regressive tax on lower-income households rather than directly enriching those at the top of the income distribution (as laid out in the literature review). These results, while suggestive rather than definitive given the small sample, establish that the corruption–inequality channel is operative in these economies. I now turn to the paper's central question: whether EU accession produced a measurable effect on inequality through this channel.

Difference-in-Differences: EU Accession and Inequality (Table 4)

Table 4 presents the central results of this paper. Columns (1–2) show the within-treatment DiD analysis: the pooled 'Post' coefficient is small and insignificant for both the Gini and the top 10% share measures. Simply put, EU accession had no statistically significant effect on inequality within the treatment group observed.

Columns (3–4) introduce the ECA average as a control group, and it is here that the results become more interesting. The Post×Treated coefficient is positive and significant: the Gini increased by approximately 3.5 standard points and the top 10% share by 3.2 percentage points in treatment countries relative to the ECA. The finding that EU accession seemingly causes an increase in inequality merits further investigation.

Column (5) provides a key diagnostic in this regard. Adding country-specific linear time trends reduces the Post coefficient to nearly zero, yielding it statistically insignificant. The

Bulgaria trend is large and highly significant, thus indicating that this country's inequality rose at roughly half a Gini point per year throughout the sample period, independent of its EU accession. Croatia also trends upward throughout this period when taking Romania as reference. Once this variation in inequality trends is taken into consideration, it can be asserted that EU accession has no statistically significant effect on income inequality. This explains that the result in columns (3-4) is more of a representation of Bulgaria's independent story, and decidedly not a causal consequence of EU accession. Bearing these results in mind, the trend-adjusted specification of the model is preferable for inference.

To further corroborate the findings, Figure 2 presents event study estimates to visually complement the DiD results. As can be seen, none of the pre-treatment coefficients are individually significant, though the large confidence intervals somewhat limit the power of formal pre-trend tests. Post-treatment coefficients are also uniformly insignificant, with the point estimates showing no systematic break at the accession date. While the pattern is admittedly noisy, it does not suggest a break at the accession period, and these findings further corroborate the result showcased in the last column of Table 4.

Heterogeneity Analysis

The WBES publishes aggregated bribery indicators by firm size (small: 5–19 employees, medium: 20–99, large: 100+) and by sector (manufacturing and services) for each country-year survey. Figures 3a–3c and 4a–4c plot these breakdowns to examine whether corruption patterns differ along these dimensions.

The firm size figures reveal considerable heterogeneity but no consistent pattern across countries. In Romania, small firms initially reported the highest bribery rates but converged with larger firms to low single digits by 2023. Bulgaria and Croatia display substantial volatility across all size categories. In the pooled regression (not reported), post-accession bribery declines by 3.24 percentage points for small firms, but the coefficients for medium and large firms are insignificant. The sector analysis is similarly inconclusive: the Post × Services interaction is negative but insignificant ($\beta=-2.30$, $p=0.143$). Given the limited number of WBES observations underlying these breakdowns, these results should be treated as descriptive rather than inferential. The confidence intervals are too wide to draw firm conclusions about differential effects by firm size or sector.

Robustness Checks

Placebo Test

To check and see if the results obtained thus far are robust to various specifications, I assign false EU accession dates three years before the actual dates (i.e. 2004 for Bulgaria and Romania, and 2010 for Croatia) and re-estimate the annual DiD. The Post coefficient comes back positive and significant for both the Gini and the top 10% share implying that there was an upward trend in inequality levels before the accession periods, thereby violating the parallel trends assumption for the parsimonious DiD. This further amplifies the validity of the trend-adjusted specification in Table 4 (Column 5), which captures these pre-trends and

yields statistically insignificant results. In fact, the result that the placebo accession dates produced significant effects while the real accession dates (with trends) did not, reveals more information about underlying trajectories in inequality regardless of any potential effect of EU accession.

Unweighted Estimates

Finally, I estimate the key models without using precision weights. This reveals that the Post coefficient on bribery incidence remains positive and statistically significant, while the bribery-Gini relationship becomes smaller in magnitude and loses its significance. Therefore, the weighted estimates seem to be more desirable, which is consistent with the precision-weighting rationale that larger survey rounds provide more reliable corruption measures. The qualitative conclusions remain unchanged: there is suggestive evidence of a positive corruption–inequality association, and the DiD analysis yields null results for the effect of EU accession on inequality.

Discussion

The results across both the WBES panel and the annual DiD point to a clear set of conclusions regarding the effect of EU accession on income inequality. First, corruption and income inequality are positively correlated within these three economies: higher levels of firm-reported bribery are associated with higher Gini coefficients, consistent with the theoretical predictions of Gupta, Davoodi & Alonso-Terme (2002) and the broader cross-country literature. This confirms that the corruption–inequality channel is empirically relevant in

this context — a necessary condition for the central hypothesis. Second, and more importantly, EU accession did not produce a measurable reduction in income inequality despite its demonstrable success in reducing corruption.

Several explanations are consistent with these findings, and it is worthwhile to consider them all. The most straightforward argument is that the 'corruption→inequality' channel is simply second-order relative to the other determinants of inequality in these transition economies. Labor market restructuring, skill-biased technological change, capital account liberalization, and the uneven distribution of EU cohesion funds may all have higher order effects than the corruption channel does in practice. In this regard, Bandelj & Mahutga (2010) provide evidence that EU economic integration itself contributed to rising inequality through factor price equalization, which would potentially offset any inequality-reducing effects the anti-corruption reforms might have had as a result of EU accession.

Another explanation concerns the nature of these anti-corruption reforms. The CVM and Chapter 23 benchmarks mainly focused on legal and institutional frameworks - establishing agencies, passing legislation, prosecuting cases - rather than on the small-level corruption as measured by the WBES bribery indicators employed in this study. If high-level corruption and petty corruption have different effects on income inequality (which is not implausible), then reforms targeting the former may not translate into inequality reduction through the latter. This distinction is not often made in the literature, but it is one that the present findings would seem to establish.

This disconnect between the level at which reforms operated and the level at which corruption is measured in the data raises two important questions. First, did the high-level

reforms reduce inequality through channels other than everyday firm-level bribery? If the CVM and Chapter 23 benchmarks curbed elite-level rent extraction or improved the allocation of public funds, these effects might not be captured by the WBES indicators at all, seeing as how the surveys measure the corruption that firms encounter in their day-to-day interactions with public officials rather than systemic corruption at the top. Second, and perhaps more fundamentally, was targeting high-level institutional corruption the right approach to reducing people's everyday interactions with corruption in the first place? If petty bribery is sustained by local enforcement cultures, bureaucratic discretion, and low public-sector wages rather than by the absence of anti-corruption agencies or high-profile prosecutions, then top-down reforms may have limited reach into the day-to-day corruption that the WBES captures. The fact that Romania saw dramatic declines in firm-level bribery alongside its institutional reforms while Croatia did not, despite both undergoing rigorous accession conditionality, suggests that the transmission from high-level reform to everyday corruption is far from automatic and likely depends on country-specific factors that merit further investigation.

Acemoglu and Robinson's (2012) distinction between inclusive and extractive institutions offers a useful framework for understanding this disconnect. Inclusive institutions - characterized by broadly distributed political power, secure property rights, and a rule of law that benefits the population at large - tend to generate sustained, *equitable* growth, whereas extractive institutions concentrate power and resources among elites. The CVM and Chapter 23 benchmarks can be understood as attempts to build inclusive institutions from above: establishing independent judiciaries, creating anti-corruption agencies, and prosecuting high-level corruption cases. Yet the WBES indicators capture something closer

to the persistence of extractive practices at the local level - the everyday bribery, informal payments, and bureaucratic rent-seeking that firms encounter in their routine interactions with public officials. Hence, the results of this paper are consistent with a reading in which formal institutional architecture moved in an inclusive direction while extractive local practices proved more resilient to top-down reform. If the everyday corruption measured by the WBES is sustained by deeply embedded extractive norms - driven by local enforcement cultures, discretionary bureaucratic power, and patronage networks that operate below the level at which the CVM and Chapter 23 intervened - then the gap between improving institutional frameworks and persistent firm-level bribery becomes more intelligible. Romania's relative success in reducing both high-level and firm-level corruption may reflect a case where inclusive institution-building reached deeper into local practice, whereas Croatia's stagnation in these regards may indicate that formal compliance with Chapter 23 benchmarks did not resolve extractive local norms. This framing also helps explain why corruption reduction, even where it occurred, did not translate into inequality reduction: if extractive practices persist at the local level, they can continue to exert regressive distributional effects even as the formal institutional environment shows improvement.

On this exact front, the Bulgarian case is particularly revealing. Namely, Bulgaria's inequality steadily rose from a Gini of 32.8 in 2000 to 39.7 in 2023, while its corruption indicators declined drastically in the same period (bribery incidence falling from 10.9% to 0.9%). This divergence between decreasing corruption and increasing inequality is hard to reconcile with a firm corruption→inequality channel, and suggests that other forces (such as but not

limited to economic structure, weak labor market institutions, EU fund allocation and distribution, etc.) dominated in effect.

Limitations

As with any research, this paper has its limitations, and so can benefit from several extensions. The most fundamental limitation in this case is the sample size. With three treatment countries and 13 WBES observations, statistical power is severely limited, and the WBES regressions should be interpreted more as motivating associations rather than definitive causal estimates. The DiD design also rests on a parallel trends assumption that is somewhat questionable given the divergent trajectories in income inequality - and while the trend-adjusted specification addresses this, it does so at the cost of capturing much of the identifying variation. Additionally, the ECA control group uses an aggregation of countries with that have very different underlying economic structures and reform paths, thereby making it an imperfect control. Furthermore, as per the nature of the data, the WBES only captures firm-level bribery, which is only one dimension of corruption. Finally, the SWIID Gini, while the best available harmonized measure, is itself an estimate subject to uncertainty that is not incorporated in the standard errors reported here in this paper.

Concluding Remarks

This paper asks whether EU accession-driven anti-corruption reforms reduced income inequality in Romania, Bulgaria, and Croatia. The answer, within the scope of this analysis, is no. The 2×2 difference-in-differences evidence in Table 2 reveals that Romania and Bulgaria

experienced significant post-2013 corruption declines, likely sustained by CVM monitoring, while Croatia's corruption indicators showed no comparable improvement following accession — a finding that is in line with the argument that accession without continued conditionality may be insufficient on its own. Despite these differential corruption trajectories, the difference-in-differences analysis on the main outcome variable establishes that EU accession did not produce a measurable inequality-reducing effect through the anti-corruption channel. When compared to the ECA control group, the treatment countries experienced rising inequality overall, but this last result mostly reflects Bulgaria's story and is fully captured when country-specific time trends are included in the specification.

These findings may have potential implications for EU enlargement policy. First, the CVM and Chapter 23 frameworks seem to have succeeded in reducing corruption in a meaningful degree, but the expectation that anti-corruption conditionality would produce broader socioeconomic improvements appears unfounded - at least within the scope of this study. This implies that allocative efficiency in EU conditionality could potentially be further enhanced by pairing anti-corruption benchmarks with explicit inequality-reduction targets, ensuring that the reform agenda addresses distributional concerns directly rather than relying on institutional improvement alone. Future EU accession frameworks - relevant as they are for current candidates in the Western Balkans - could consider incorporating such explicit inequality benchmarks alongside the governance reforms to further ensure that the benefits of EU accession are more equitably shared.

Similar studies could be carried out with longer time series, alternative corruption measures, and the inclusion of additional accession countries to provide far greater statistical power.

Furthermore, micro-level analysis using firm-level WBES data (rather than the aggregated indicators here employed) could also exploit within-country variation that the present analysis presently lacks. The mechanisms through which EU integration might affect inequality – such as labor mobility, FDI composition, or the allocation of EU funds - also deserve further investigation. While the underlying economic variables used here offer a good starting point, they are by no means an exhaustive list, as the transmission mechanism from institutional reform to income inequality is indubitably affected by many other factors. The astute policymaker could thus use this research as a starting point to determine how conditionality frameworks might be redesigned to target inequality directly, rather than relying on the indirect - and, as this paper finds, insufficient - channel of corruption reduction alone.

Tables and Figures

Table 1: Summary Statistics (WBES Panel)

Variable	N	Mean	SD	Min	Max
Bribery incidence (%)	13	7.77	4.64	0.90	19.70
Bribery depth (%)	13	5.38	3.26	0.90	14.10
Corruption constraint (%)	13	29.19	14.51	12.10	52.30
SWIID Gini (×100)	13	31.74	3.84	26.79	39.70
Top 10% income share	13	0.381	0.042	0.338	0.452
Log GDP per capita	13	9.19	0.33	8.71	9.75
N firms surveyed	13	382	173	157	799

Notes: SWIID Gini stored as ×100 in data (e.g., 3174 = 31.74). Statistics here reported on standard 0–100 scale.

Table 2: EU Accession and Corruption — 2×2 DiD (WBES Panel)

	(1) Bribery Inc.	(2) Bribery Dep.	(3) Corr. Constraint
$Post_{2013}$	−9.490**	−4.968*	−17.232*
	(3.763)	(2.390)	(7.825)
Croatia	−7.179*	−4.673**	−26.449***
	(3.463)	(2.006)	(3.693)
DiD ($Post_{2013}$ × Croatia)	11.817**	6.491**	12.709
	(3.910)	(2.503)	(8.025)
Intercept	12.079***	7.573***	45.349***
	(3.463)	(2.006)	(3.693)
N	13	13	13
R^2	0.476	0.365	0.573

*Notes: HC1 robust SEs in parentheses. *p<0.10, **p<0.05, ***p<0.01. $Post_{2013}$=1 for survey waves 2013 or later. Croatia=1 for Croatian observations. DiD=$Post_{2013}$ × Croatia. Control group: Romania and Bulgaria.*

Table 3: Corruption and SWIID Gini (WBES Panel)

	(1) Bribery Inc.	(2) Bribery Dep.	(3) Corr. Constraint
Corruption	24.42	33.44*	15.38*
	(9.732)	(11.105)	(3.657)
ln(GDP/cap)	710.0	663.2	673.5
	(649.0)	(684.1)	(725.8)
Country FE	Yes	Yes	Yes
Weighted (n firms)	Yes	Yes	Yes
N	13	13	13
R^2	0.759	0.755	0.882

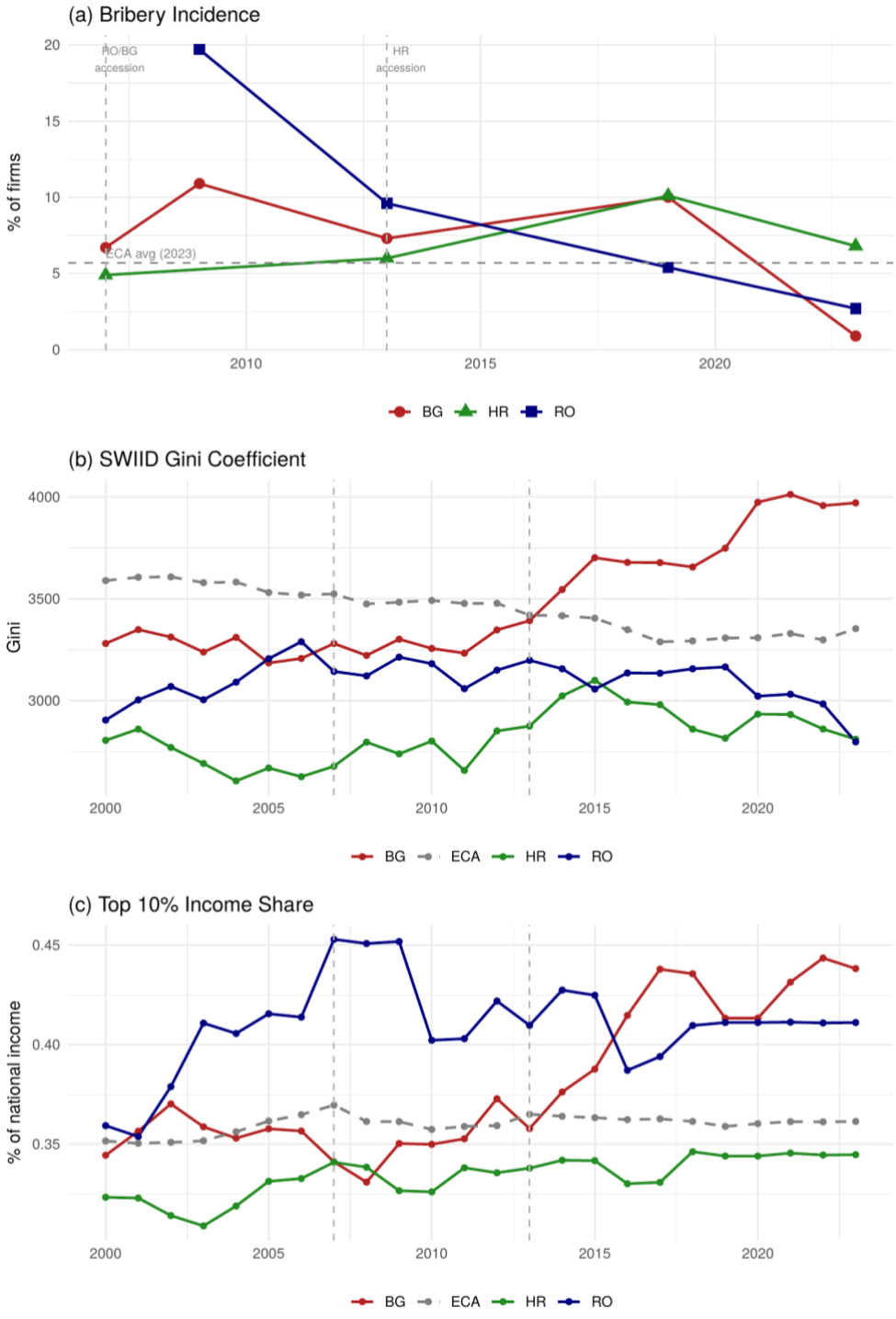
*Notes: DV is Gini (×100 scale). Coefficients: SWIID Gini points per 1pp increase in corruption. *p<0.10, **p<0.05, ***p<0.01.*

Table 4: DiD — EU Accession and Inequality (Annual Panel, 2000–2023)

	(1) Gini	(2) Top10	(3) Gini	(4) Top10	(5) Gini
	Pooled	*Pooled*	*vs ECA*	*vs ECA*	*Trend*
Post	−31.33	−0.007			−7.40
	(120.2)	(0.015)			(69.4)
Post×Treated			353.8**	0.032**	
			(137.0)	(0.014)	
BG trend					47.83***
					(7.87)
HR trend					28.18*
					(14.77)
Full controls	Yes	Yes	Yes	Yes	Yes
Country + Year FE	Yes	Yes	Yes	Yes	Yes
Country trends	No	No	No	No	Yes
N	72	72	96	96	72
R^2	0.911	0.905	0.868	0.858	0.967

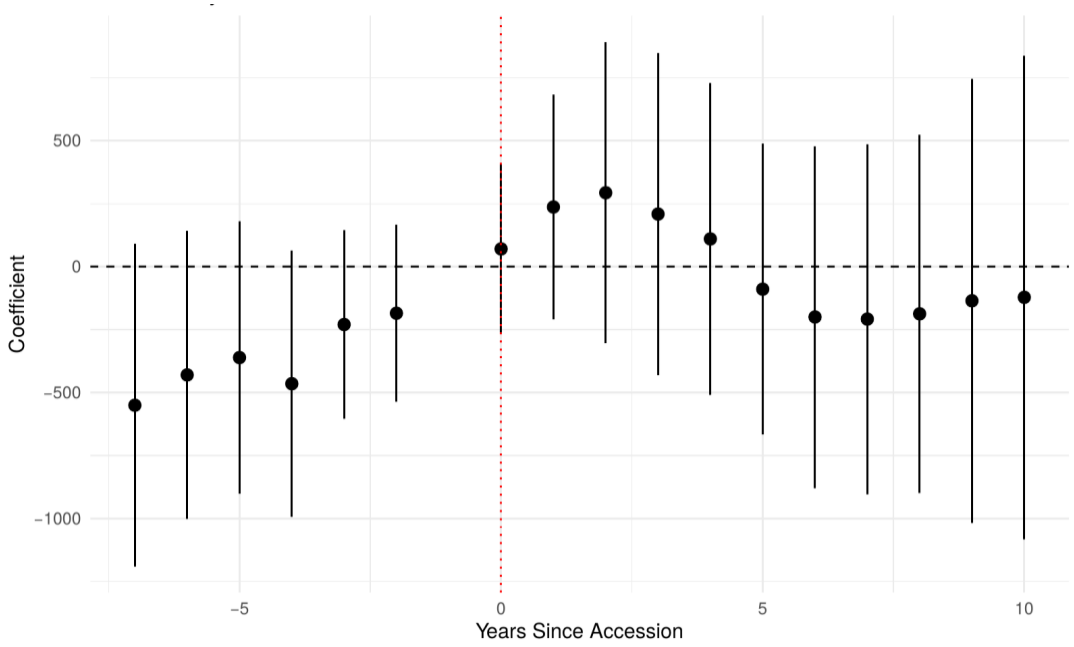
*Notes: HC1 robust SEs in parentheses. *p<0.10, **p<0.05, ***p<0.01. Columns (1–2) and (5): treatment countries only (RO, BG, HR; N=72). Columns (3–4): treatment countries plus ECA regional average (N=96). $Post_{it}$=1 if country i has acceded by year t (2007 for RO/BG, 2013 for HR; 0 for ECA). $Treated_i$=1 for RO, BG, HR; 0 for ECA. SWIID Gini on ×100 scale (353.8 ≈ 3.54 Gini points). Top 10% as proportion. Full controls: ln GDP/cap, trade openness, govt expenditure, unemployment, FDI, tertiary enrollment. Romania is reference country for trends in Column 5.*

Figure 1: Corruption and Inequality Trajectories



Notes: Panel (a): WBES bribery incidence. Dashed lines mark RO/BG (2007) and HR (2013) accession. ECA 2023 shown as reference. Panels (b)–(c): SWIID Gini and WID top 10% share. ECA regional average shown as dashed line.

Figure 2: Event Study — SWIID Gini Around Accession



Notes: Point estimates and 95% CI from event study specification with year-relative-to-accession indicators, country FE, year FE, and full controls. Reference period: t−1.

Figure 3a: Bribery Incidence by Firm Size — Bulgaria

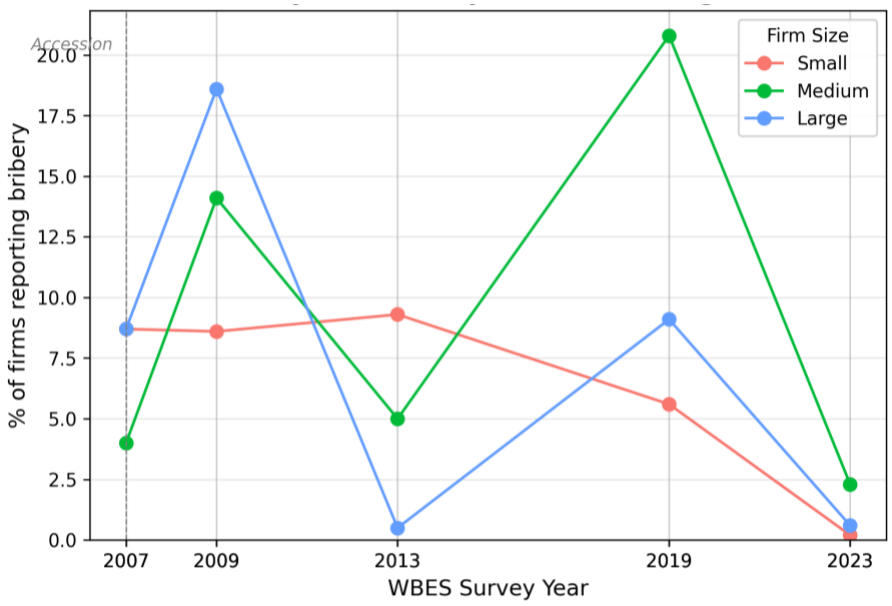


Figure 3b: Bribery Incidence by Firm Size — Croatia

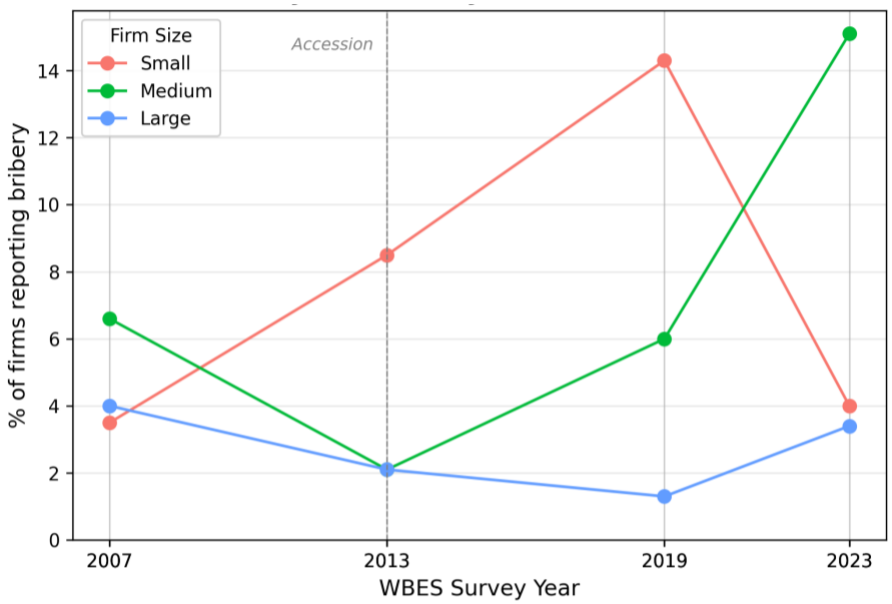
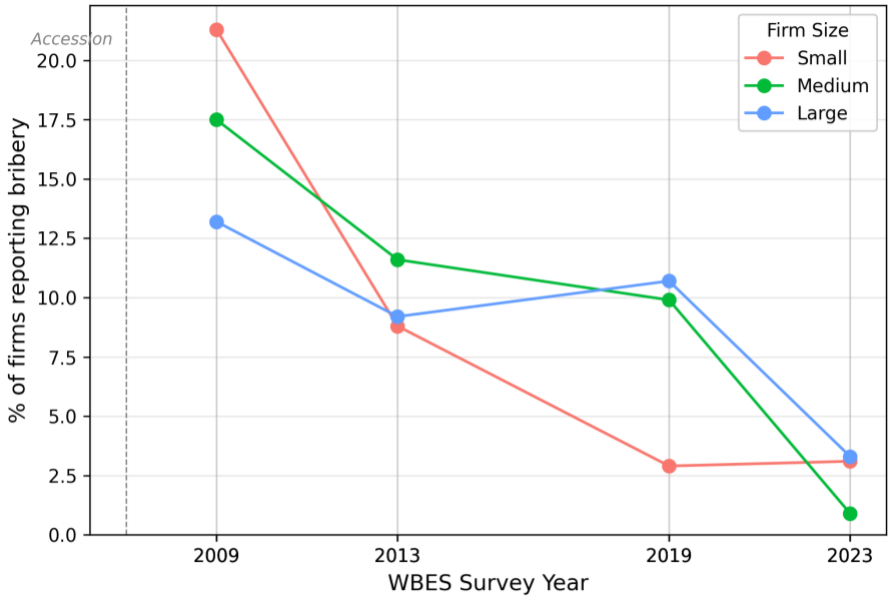


Figure 3c: Bribery Incidence by Firm Size — Romania



Notes: WBES bribery incidence by firm size category. Dashed vertical lines mark EU accession year.

Figure 4a: Bribery Incidence by Sector — Bulgaria

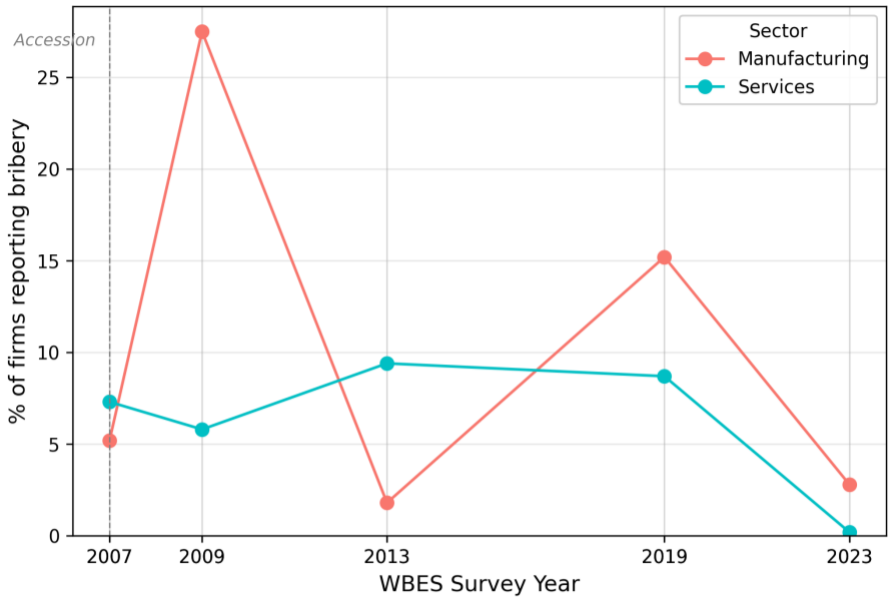


Figure 4b: Bribery Incidence by Sector — Croatia

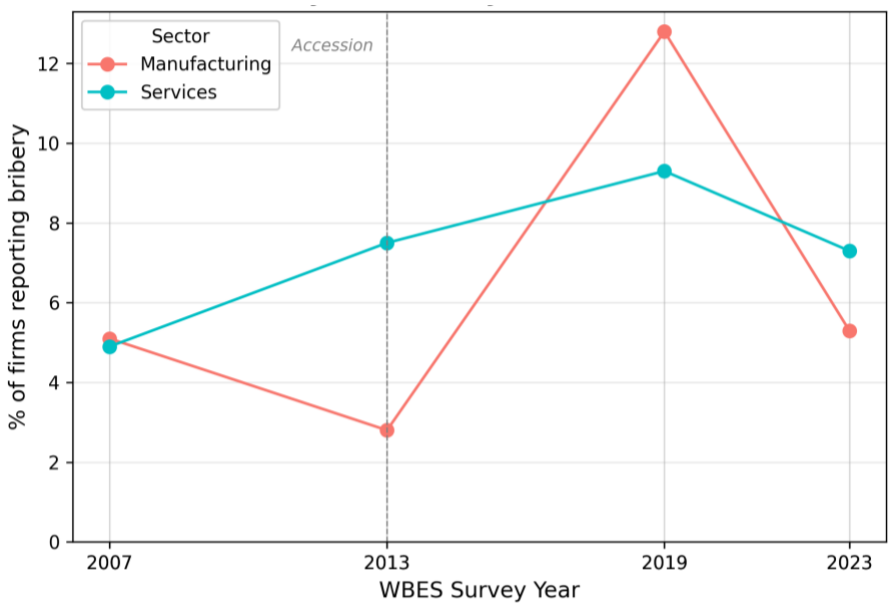
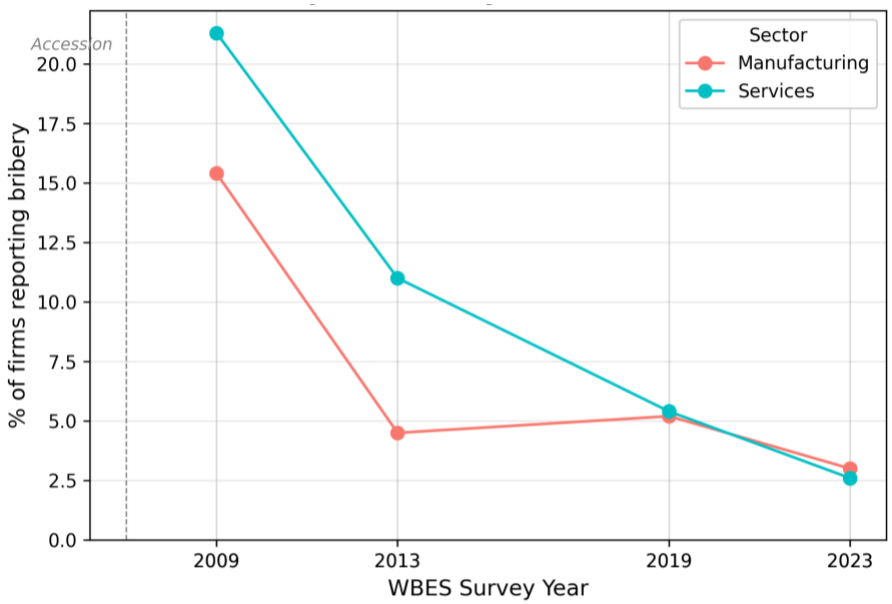


Figure 4c: Bribery Incidence by Sector — Romania



Notes: WBES bribery incidence by broad sector. Dashed vertical lines mark EU accession year.

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