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Rethinking public funding of parties and corruption: Confronting theoretical complexity and challenging measurement

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Abstract

Does the provision of state subsidies to political parties reduce their involvement in corruption? Existing research provides inconclusive evidence on this relationship, perhaps because cross-national studies on public funding and corruption are often limited by regulation-based indexes of political financing and by very general corruption measures. In this study, we use focused measures for both phenomena to investigate whether more generous public funding reduces party corruption. Our independent variable reflects the actual cash amount of budgetary subventions provided to parties in twenty-seven post-communist countries. Our dependent variable of party-centered corruption represents the share of firms considerably affected by the informal payments made by businesses to political parties and parliamentarians to influence their decisions. We find that a higher level of state subsidies is associated with a reduction in corruption; its effect diminishes as funding increases, and its impact on corruption is lagged. However, there is a wide interval of uncertainty around these results. In the context of the existing literature, our contribution reduces the estimate

of the size of a public funding effect and increases the level of uncertainty.

1 | INTRODUCTION

There is a straightforward and compelling case for why public funding of politics should inhibit corruption (Cagé, 2020, chs. 5–6). It should reduce the marginal benefit of any private funding politicians receive for political purposes. This should hold whether the private funding is legal or illicit. The reduction in the marginal benefit of the money should reduce the access and influence private interests can buy, thereby constraining corruption. Moreover, this effect should diminish as the amount of public funding increases: moving from a little to a lot public funding should substantially reduce the influence-value of private money, while moving from a lot of public funding to a huge amount of public funding should make little difference. If we could imagine a laboratory test of this mechanism, it is hard to imagine how it would not show that public funding reduces corruption.

However, attempts to evaluate this argument empirically have produced mixed results. Given the human and academic bias towards positive results, it is striking that the literature seems to include almost as many null findings as it does associations between public funding and reduced corruption. There are two principal reasons why the question is empirically challenging: measurement and theoretical complexity. The dependent and independent variables are difficult to measure. Indeed, corruption, virtually by definition, cannot be measured directly but rather only perceived, experienced, or proxied. Data on public funding is also challenging to collect and standardize across time and political systems. Even if the two key variables were perfectly measured, they are surely embedded in complex relationships. Public funding is only part of a system of political finance regulation, which is in turn part of a political and administrative system that interacts with wider social, cultural, and economic structures at national and international levels.

This article tests the relationship between public funding and corruption using more appropriate measures of public funding and corruption, as well as a control for the regulation of private funding. Public funding affects corruption by changing the marginal benefit of private funding. It matters how much funding is provided, not merely whether some level of funding exists. Therefore, in contrast to most previous researchers, we calculate exactly how much direct public funding was provided. While corruption is a general phenomenon, public funding works through a specific mechanism that changes the incentives of elected representatives. Therefore, unlike other contributions, we target corruption by political parties, the government, and parliamentarians, not a general perception of corruption. Since the mechanism reduces the marginal benefit of private funding, we control for legal constraints on private funding. Again, much other work on this question does not include other aspects of political finance regulation.

We measure corruption using the national mean of a survey of managers responding to a question about the impact of payments to the government, political parties, and parliamentarians on their enterprise performance. This question combines concrete experience and expert perception. Enterprise managers are those with the greatest knowledge of the benefits of private funding and should be sensitive to variations in these benefits across nations and over time. For public funding, we use the amount of regular and election funding per registered voter in standardized American dollars, adjusted for inflation. For private funding, we note whether donations are unlimited, where there are contribution limits, and whether corporate donations are banned.

Our sample includes twenty-seven post-communist countries between 1999 and 2020. We find that larger amounts of public funding are associated with lower corruption, but the results are not statistically significant across all models and are sensitive to econometric specification.

2 | LITERATURE AND HYPOTHESES

Very generally, we can conceive an equation explaining corruption with three vectors of independent variables ranging from the proximate to the more distant (Equation 1). Ironically, and frustratingly for political reformers and citizens, the more distant variables seem to have a greater impact. Study after study shows that economic development and democracy are associated with lower corruption (McMann et al., 2020; Treisman, 2007). However, there are less consistent results for political institutions (Gerring & Thacker, 2004; Kunicová & Rose-Ackerman, 2005; Persson et al., 2003), and not much clear evidence that public funding or political finance regulation, more generally, reduces corruption (Biezen, 2010; Casal Bértoa et al., 2014; Evertsson, 2013; Fazekas & Cingolani, 2017; Hummel et al., 2021; Lopez et al., 2017).

$$\text{Corruption} = \text{political finance regulation} + \text{politics} + \text{society} \quad (1)$$

Even if they had been perfectly measured, there are potentially many reasons why the more proximate variables in general, and public funding in particular, have not consistently shown the predicted negative relationship with corruption. We will mention four, firstly two that are prominent in the literature and then two that are seldom emphasized. First, politics is a largely self-regulating activity. Politicians make political finance regulations and are likely to avoid arrangements threatening their interests (Greene, 2010, pp. 37–45). The cartel party thesis is a brilliant reminder of this truth by attributing a central role to public funding in protecting incumbent parties from competition and accountability to society (Katz & Mair, 1995). Still, it is not about corruption but rather about the relationship between parties, the state, and society. Second, corruption is inelastic. A structure or culture of corruption does not respond immediately to institutional tinkering or marginal adjustments of incentives (Fisman & Golden, 2017, pp. 215, 245). Instead, changes in corruption are perhaps associated with critical junctures when there is a major change of direction for a polity and considerable freedom to reform fundamentally social and political relationships in a range of interlinked domains.

Now, we turn to less-discussed challenges in making a causal connection between public funding and corruption. Third, the relationship between the extent of political finance regulation and corruption may be curvilinear: the least and most corrupt societies have been those with the fewest political finance regulations. Most obviously the squeaky clean Scandinavians did not regulate parties tightly, presumably because there was little need to do so (In Norris & Abel van Es, 2016, p. 262). Fourth and last, the public funding argument assumes that all or much of private money is used for political purposes. If politicians pocket it for personal use, public funding should not affect the benefit of private funding. In other words, corruption will be inelastic. Some authors have emphasized the link between campaign costs and corruption (Della Porta & Vannucci, 2002, p. 730; Heidenheimer, 2002, p. 769), and it is assumed that personal gain is a much smaller problem in Western scandals than party fundraising (Hopkin, 2004, p. 628). Nevertheless, it is unclear to what extent personal profit and party profit co-vary, as corruption measures do not distinguish between them.

Weschle's recent book (2022) is an important exception to the blank spot on whether donations are used for personal expenditure or political purposes. It argues that forms of money entering

politics are partially fungible. Changes in the regulation of self-enrichment, campaign spending, or golden parachute jobs are likely to affect the prevalence of at least one of the others. Changes in regulation or political competition that have a first-order effect on one type are likely to have 'second-order effects in the opposite direction on the others' (Weschle, 2022, p. 36). Although he does not deal with public funding, he does provide a framework to think about its effects in a new way. The first-order effect of the introduction of public funding should have a second-order effect on the extent to which private income is allocated to campaign spending. The second-order effect, then, is that private income is re-allocated to self-enrichment. Since self-enrichment may be a more direct and stronger incentive than campaign spending for some politicians, ironically, there is a channel through which public funding could increase corruption. Nevertheless, it is unclear how strong this effect would be compared to the reduction in the need for private income due to public funding.

Much of the literature on political finance regulation and corruption does not separate public funding from other interventions (Abel van Es, 2016; Fazekas & Cingolani, 2017; Norris, 2017). Early research on public funding and corruption tended to measure it as a dummy variable or part of an index (Ben-Bassat & Dahan, 2015; Evertsson, 2013; Kostadinova, 2012; Ohman, 2012). It produced very mixed results on corruption, and skeptical academic work continues to be published (Power, 2020). Notwithstanding the contradictory evidence, the international community has taken a strong line in favor of public funding, asserts that it reduces corruption, and recommends that countries adopt it (Biezen, 2003; Council of Europe, 2001, 2003; Doublet, 2012; OECD, 2016; Speck & OECD, 2013). The Council of Europe's Committee of Ministers recommends state support of political parties as an anti-corruption measure in the funding of political parties and electoral candidates (Council of Europe, 2003). The OECD does not explicitly recommend public funding as an anti-corruption tool, but it does list it as part of a holistic recommendation of political finance regulation to limit policy capture (OECD, 2016).

Recently, there has been a surge in quantitative work on political finance regulation and public funding. Hummel et al. (2021) leverage over ten thousand country-year observations to explore the effect of public funding on corruption. This is a key advantage given the stickiness of corruption and may explain why they report a negative relationship between public funding and corruption. Their measure of public funding is an index of two de jure characteristics, their de facto implementation and the estimation of whether the majority of political financing is public. Therefore, their index is mostly a measure of the presence or absence of public funding. There is no control for the rest of the political finance system, as the authors assume it is highly correlated with public funding. The massive number of observations helps alleviate the inference problems caused by political self-regulation. It should capture many periods of political change where political actors were subject to political finance introduced by a different constellation of partisan interests. The long-time series should help overcome the inelasticity of corruption by allowing for lags between the introduction of public funding and changes in behavior. The stock transformation of the independent variable also assists in reducing the inelasticity problem. Hummel et al. (2021) do not test a curvilinear relationship between regulation and corruption and do not refer to whether bribes are used for personal consumption or political purposes.

In another study, using cross-sectional samples ranging between 27 and 37 countries, Casal Bértoa et al. (2014) report no relationship between public funding and corruption. This paper employs a much more precise measure of public funding than Hummel et al. (2021): public funding as a percentage of total party income. Moreover, they use other features of the political financing regime, such as the payout threshold for subsidies and legal accountability for political finance.

Our research design has some advantages over these two studies. Although we provide more details on research design in the next section, here it suffices to say that our dependent variable more precisely targets corruption relevant to public funding, and the respondents are likely to have experience and inside knowledge rather than generalized perceptions of corruption. Our independent variable is a cash amount and, therefore, more precisely measured than in any previous work. Unlike Hummel et al. (2021), we allow for the possibility that the political finance system is complex and control for the regulation of private funding. Unlike Casal Bértoa et al. (2014), we include more than one point in time. Next, we enumerate our hypotheses before discussing empirical issues in more detail.

We test three hypotheses, all of which are versions of the basic idea that public funding reduces corruption.

H1. The higher the level of direct public funding, the lower party corruption.

This is the basic hypothesis we test. More specifically, we measure party corruption as the percentage of firms significantly and very significantly affected by informal payments to political parties and legislators.

H2. The negative effect of direct public funding on corruption diminishes as public funding increases.

In order to test this hypothesis, we discount the independent variable, direct public funding. This hypothesis is consistent with the idea that public funding reduces the marginal benefit of private funding. At higher levels of public funding, the effect of an extra dollar of public funding on the incentive to fundraise privately is likely to diminish.

H3. There is a lag between public funding changes and their effect on corruption.

This hypothesis addresses the inelasticity of corruption and is tested by lagged values of public funding.

3 | RESEARCH DESIGN

3.1 | Regional case and hypothesis selection

We offer five reasons to think that public funding might be more likely to reduce corruption in our post-communist population than in other regions: two generic research design issues and three mitigations of the specific challenges of studying public funding and corruption.

First, there are no really low corruption countries. At the limit, if public funding is introduced where there is no corruption, it cannot reduce corruption. For example, we would not expect an increase in public funding in contemporary Finland to reduce corruption substantially. Second, there is within-country and between-country variation in public funding. Notwithstanding the increasingly normative status of public funding globally, it is hard to imagine another region with more substantial changes in the level of public funding. Third, since politics is a self-regulating activity, public funding may be designed to suit the purposes, including corrupt activities, of political elites. The region's democracies are well known for their volatility: governments are

rarely re-elected; legislators come and go; parties emerge, merge, and disappear. This means that post-communist politicians tend to operate in a public funding environment designed by other politicians and parties to a greater extent than politicians in other regions. Indeed, corruption is surely one of the explanations for this volatility. Protests (Školník, 2022), civil society (Schmidt, 2007), and even new political parties have often mobilized primarily or exclusively around corruption. Fourth, corruption is inelastic, and it is perhaps only after a critical juncture that there is scope for policy interventions to reduce corruption. The post-communist region is defined by a critical juncture—the fall of communism, after which rules and structures had to be reconstructed. In this context, new rules and structures had a greater potential to affect social outcomes than in a more settled political, economic, social, and cultural environment. Fifth, we observed that the relationship between political finance regulation and corruption is curvilinear: the highest regulation has been associated with moderate corruption, whereas high- and low-corruption countries have had lower levels of regulation. The lax regulation in low corruption environments reflects starting points. Countries, which entered the modern era with high standards of integrity, did not adopt rules and institutions to solve a problem that did not exist. Again, the relatively common starting point of the post-communist countries mitigates this problem. Although the notion of a post-communist region should not lead us to ignore important political, economic, social, cultural, historical, and geopolitical differences, these countries all shared a common challenge of reconstructing the interface between politics and the economy and of negotiating what behavior should, and should not, be regarded as corrupt.

To be clear: we are engaged in messy observational social science, and regional case selection presents advantages, not solutions. The choice of the post-communist region means that all units are within the scope of the theory, that we can leverage variation in the data, and that three serious problems are somewhat mitigated. Since it is a most likely case for a variety of reasons, if the theory fails here, it is likely to fail elsewhere. If it thwarts falsification here, it may not do so elsewhere.

3.2 | Dependent variable: Party corruption

Our dependent variable represents a mix of experience and perception as a country-level indicator aggregated from firm-level data. It is constructed using five waves (1999, 2002, 2005, 2013, 2020) of the Business Environment and Enterprise Performance Survey (BEEPS) conducted by the European Bank for Reconstruction and Development and the World Bank with a focus on post-communist regimes.[1] We use a question that asks top executives about the impact of informal payments made to different political actors to gain particularistic benefits on their firm's performance.[2] The dependent variable represents the percentage of respondents who answered that such payments had a major or decisive impact on their business, as opposed to moderate, minor, or no impact. In doing so, we follow the World Bank's approach to measuring state capture (Hellman et al., 2000) but restrict our inquiry to political actors that are most relevant to the effect of public funding on party corruption—political parties and parliamentarians.[3] It is important to note that business managers answering the respective question, do not report whether their company is engaged in corruption by making informal payments to political parties and legislators. Instead, they report the extent to which their business is affected by such payments. While this approach places our dependent variable rather in the pool of perception-based measures, it is different from expert-based measures since business is more sensitive to the demand for political

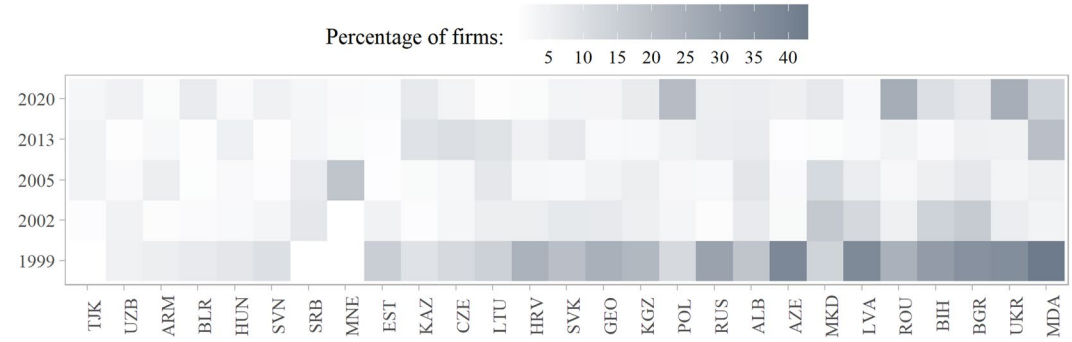

FIGURE 1 Cross-national and within-country variation in the percentage of firms significantly and decisively affected by informal payments made to political parties and parliamentarians. *Source:* Own elaboration based on Business Environment and Enterprise Performance Survey (BEEPS) data.

funds on politicians' behalf. Therefore, our dependent variable better reflects the relationship between the demand and supply of party and campaign funding and political corruption.

We also construct alternative versions of the dependent variable by including firms' responses regarding illegal payments to central and local government officials. Still, these measures are highly correlated with our main operationalization (see Figure A3 in the appendix). As Figure 1 shows, the impact of informal payments on firms' performance was particularly severe during the first decade of transition. Nevertheless, grand and petty corruption became a paramount concern for the donor community in the subsequent decade (Anderson et al., 2006; Gray et al., 2004). Despite a decrease in bribe frequency between the first and the subsequent BEEPS waves, corruption still represented one of the greatest obstacles to business (Gray et al., 2004, pp. 15, 19). By contrast, more firms were engaging in *state capture* by "using gifts and/or unofficial payments to influence laws, regulations, and decrees" (Gray et al., 2004, p. 31). As a result "the market for illicit influence has become more open...and competitive", such that even though more firms are involved the impact is smaller (Gray et al., 2004, p. 31). This is a more subtle judgment about the political corruption that interests us most. Although more firms are involved, due to competitive pressure, on average the impact is less harmful for business activity.

Figure 2 shows the distribution of our dependent variable. As Panel A shows, it is heavily skewed and requires transformation. We applied a series of standard transformations, and the Yeo-Johnson transformation (Yeo & Johnson, 2000) (Panel B) produced the most normal distribution (See the Appendix pp. 6–7).

3.3 | Independent variable: Public funding

Due to the uneven gaps between BEEPS waves, we have an irregularly spaced panel, but our independent variables are available annually between 1990 and 2020. Therefore, we average independent variable between BEEPS waves to maximize our use of the available information. We also show models that deal with the potential lagged impact of public funding and the data structure in different ways.

Our key independent variable is the actual amount of public funding provided to political parties in twenty-seven post-communist regimes. The information was collected from state budget laws, various decisions and reports of supervisory and electoral management bodies, party/party funding laws, electoral regulations, and other governmental statistical indicators

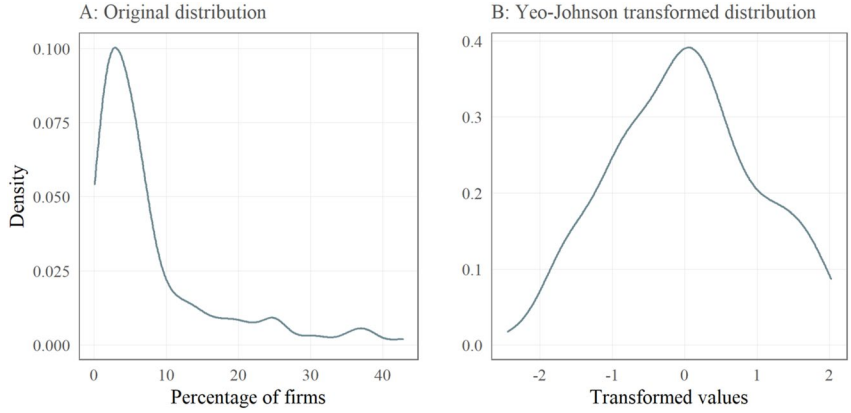

FIGURE 2 Original and transformed distribution of dependent variable: Percentage of firms affected by informal payments to political parties and parliamentarians. *Source:* Own elaboration based on BEEPS data. [Correction added on 12 May 2023, after first online publication: Figure 2 caption has been corrected]

such as wage levels, budgetary revenue and expenses or GDP, international electoral monitoring reports and other secondary literature (See Lipcean (2021)). Our data combine subsidies intended for party regular and electoral activities. We standardize it by accounting for the size of the electoral market, expressed in the number of votes and the national currency exchange rates against the American dollar, adjusted for inflation. Hence, our independent variable reflects direct public funding per vote.

To account for the diminishing returns of state funding in reducing party corruption, we transform our independent variable by applying a one percent discount coefficient (See Stratmann (2006) for an analogous approach). Since these transformations affect the range of our public funding variable, we employ two versions in our analysis: inflation-adjusted and inflation-adjusted and discounted. Figure A2 in the online appendix presents scatterplots of these versions and the nominal value of public funding. Additionally, Figure A3 depicts the relationship between the amount of subsidies per registered voter and vote. This is a relevant factor that might affect the estimation due to uneven within-country developments between the size of electorate and turnout. However, it does not raise concerns given the almost perfect correlation between the two measures.

As Figure 3 shows, there is substantial cross-national and within-country variation in subsidies. Most previous research suppresses this variation by including only the presence of public funding or combining such binary measures with estimates reflecting the balance of private and public funding in the structure of party budgets.

3.4 | Control variables

We first control for private funding. The control of private funding should reduce the frequency and magnitude of illegal payments to political parties, thus diminishing the impact of corruption on enterprises. We operationalize private funding restrictions as an aggregate index of donation limits on individuals and legal entities (corporate donations) for regular and election financing. We code these restrictions as zero—no limits, one—limits on the amount contributed by individuals or businesses, and two—full ban of corporate donations. These restrictions can be applied to party statutory and election financing, which make our index an ordinal variable ranging from

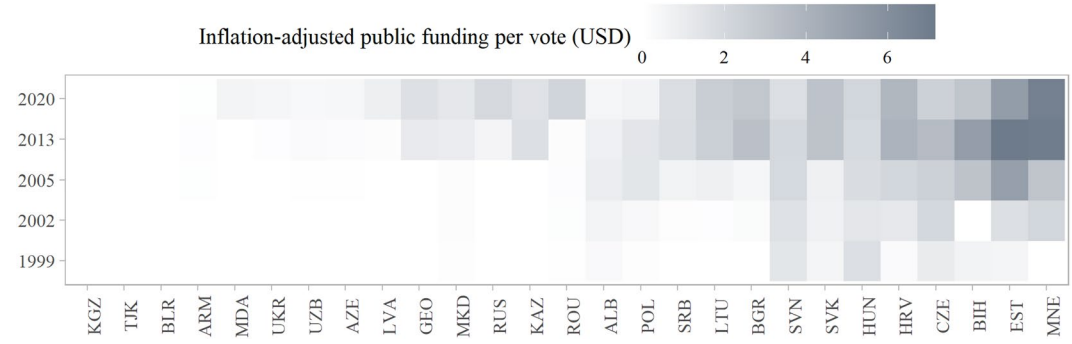

FIGURE 3 Cross-national and within-country variation in direct public funding over time: aggregated by Business Environment and Enterprise Performance Survey (BEEPS) round. *Source:* Own elaboration based on data from Lipcean (2021).

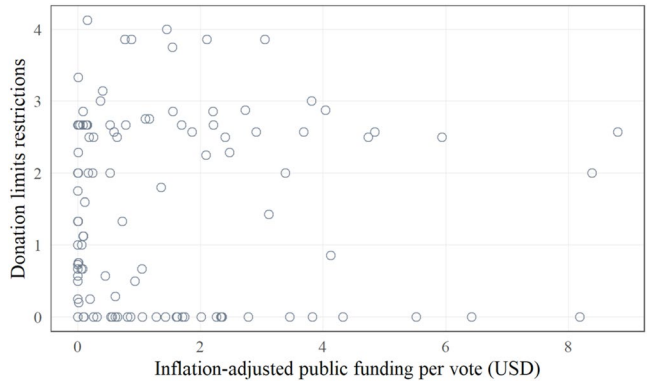

FIGURE 4 Relationship between the level of public funding and donation limits. *Source:* Own elaboration based on Lipcean (2021) and national regulations on donation limits.

zero to six. However, none of the post-communist regimes reaches the maximum score on donation restrictions. Within the "carrot and stick" framework applied to party financing regulations, it is often assumed that the provision of public funding comes with strings attached, represented by stricter rules on donations, spending, transparency, and oversight. However, as Figure 4 shows, for the post-communist regimes, the amount of public funding and the regulation of private financing do not co-vary. Public funding often comes with no (or very weak) strings attached. Conversely, the absence or low amounts of public funding coexist with much stricter rules on political contributions. This suggests that the view of political finance regulation as a single dimension of state intervention may need to be reconsidered (Abel van Es, 2016, p. 210; Hummel et al., 2021, p. 874).

Besides private funding, we control for: democracy, economic development, electoral system (majoritarian, mixed, proportional), regime type (parliamentary, semi-presidential, presidential), the size of government, ethnic fractionalization, and inequality (See Tables A1-A2 for data sources and summary statistics in appendix).

Democracy and wealth are the two most robust and stable deterrents of corruption (Treisman, 2007). Notwithstanding vivid debates (Saha et al., 2014), overall, there is an agreement that democratic competition reduces the potential for rent-seeking (McMann et al., 2020). We use the liberal democracy index from Coppedge et al. (2021), rescaled to between zero and

ten to reduce the scale range between variables. For wealth, we employ logged GDP per capita in constant 2017 American dollars from the Gapminder dataset.

Additionally, we account for ethnic fractionalization, income distribution, and the size of government. The evidence regarding the relationship between ethnic fractionalization and corruption is mixed (Treisman, 2000, p. 429). Our measure is from Drazanova (2020) rescaled to between zero and ten. Inequality and corruption are two social phenomena that reinforce each other, thus creating a vicious circle that is hard to break (Uslaner, 2010). We take the Gini coefficient from the World Inequality Database. Likewise, some argue that larger governments provide more opportunities for rent-extraction (Kotera et al., 2012; Rose-Ackerman, 1999). Data on government size is taken from the International Monetary Fund.

Evidence suggests that parliamentary regimes are better equipped to constrain corruption due to more robust accountability checks (Gerring & Thacker, 2004; Kunicová & Rose-Ackerman, 2005). We combine Cruz et al. (2021) and Elgie (2018) coding one for presidential, two for semi-presidential, and three for parliamentary regimes. Finally, we control for the effects of the electoral system, although the debate on whether proportional or majoritarian representation better curbs corruption is not fully settled (Chang & Golden, 2007; Persson et al., 2003). This variable comes from International IDEA (2021) and Bormann and Golder (2013) and is coded one for majoritarian, two for mixed, and three for proportional systems.

3.5 | Empirical analysis

Figure 5 shows the relationship between our dependent and independent variables by country. Nineteen of twenty-seven countries exhibit a negative relationship between public funding and corruption. The seven exceptions are Armenia, Kazakhstan, Moldova, Romania, Russia, Tajikstan, and Ukraine. Moldova, Ukraine, and Tajikstan introduced public funding relatively late, while Romania only recently increased the amount of subsidies considerably. The absence of negative slopes may be due to the inelasticity of corruption and insufficient time having passed for public funding to have reduced corruption. Russia, Kazakhstan, and Tajikstan have been dominated by strongmen who presumably did not introduce public funding to constrain the corruption of their clients. Figure 6 again plots the dependent against the independent variable, but this time by survey wave. There is a negative relationship between corruption and public funding for each of the five BEEPS waves, but the slopes are noticeably steeper for the first and last waves, for which the independent variables are averaged across more years. This is also consistent with the short-term inelasticity of corruption. Next, we introduce our estimation strategy.

We have annual data for all countries for our independent variables. However, using the BEEPS surveys as our dependent variable means that we have an irregularly spaced panel. There is no straightforward way to deal with this data structure. We employ the maximum amount of information available by averaging independent variables for the periods before and between BEEPS waves. This means that implicitly we assess a lagged impact of public funding on corruption, although the different lengths over which data is averaged imply we do not measure the impact of lags in a conventional way. Later, when we focus on Hypothesis Three, we employ different techniques to probe whether the effect of public funding on corruption is lagged.

We test our hypotheses using two-way fixed effects (TWFE) and within-between random effects (WBRE). The WBRE model is often described as 'hybrid' because it combines features of more traditional fixed and random effects approaches. (The Appendix also presents separate results for country- and time-fixed effects models: Tables B1–B8, B12, B14). The theoretical differences between these techniques are subtle and complicated (Allison, 2009; Bell &

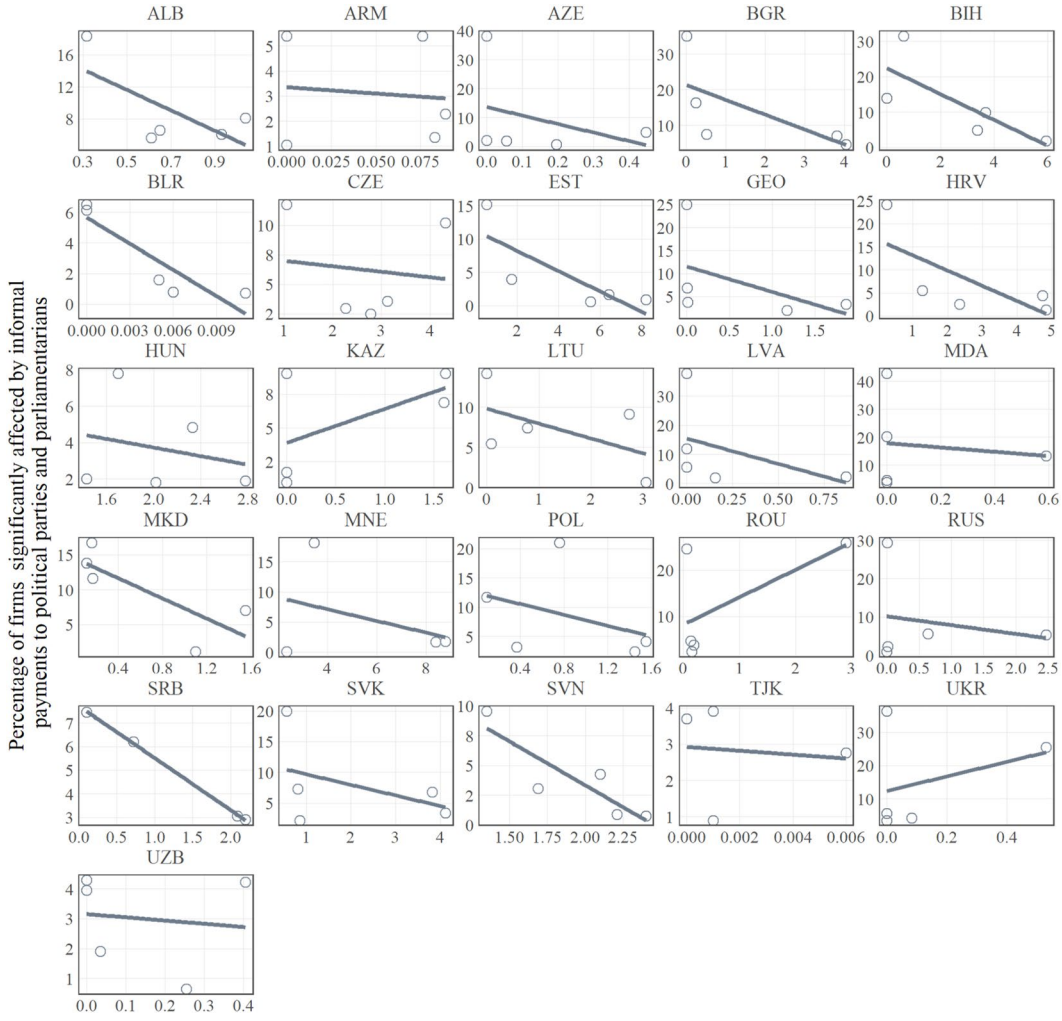

FIGURE 5 Relationship between state funding of parties and party corruption by country. *Source:* Own elaboration based on data from Lipcean (2021) and Business Environment and Enterprise Performance Survey (BEEPS). Kyrgyzstan is removed due to the lack of variation in public funding.

Jones, 2015). Here, we highlight two issues: bias and interpretability. TWFE was once regarded as the gold standard for minimizing bias from unobserved heterogeneity. This judgment has been questioned by recent contributions (Imai & Kim, 2021; Kropko & Kubinec, 2020). Nonetheless, TWFE is still regarded as an essential method to reduce bias. However, its coefficients combine variation within and between units in a way that is difficult to interpret (Kropko & Kubinec, 2020). By contrast, WBRE estimates separate coefficients for within and between-unit variation. This is especially relevant for us because policymakers considering public funding as an anti-corruption measure will want to know about within effects. Therefore, ideally, we would see consistent and significant results from both techniques, allowing a relatively confident interpretation of WBRE coefficients. Since TWFE is no longer a gold standard, we can still draw conclusions if models fail significant tests under TWFE but pass them in WBRE, albeit with a higher level of uncertainty.

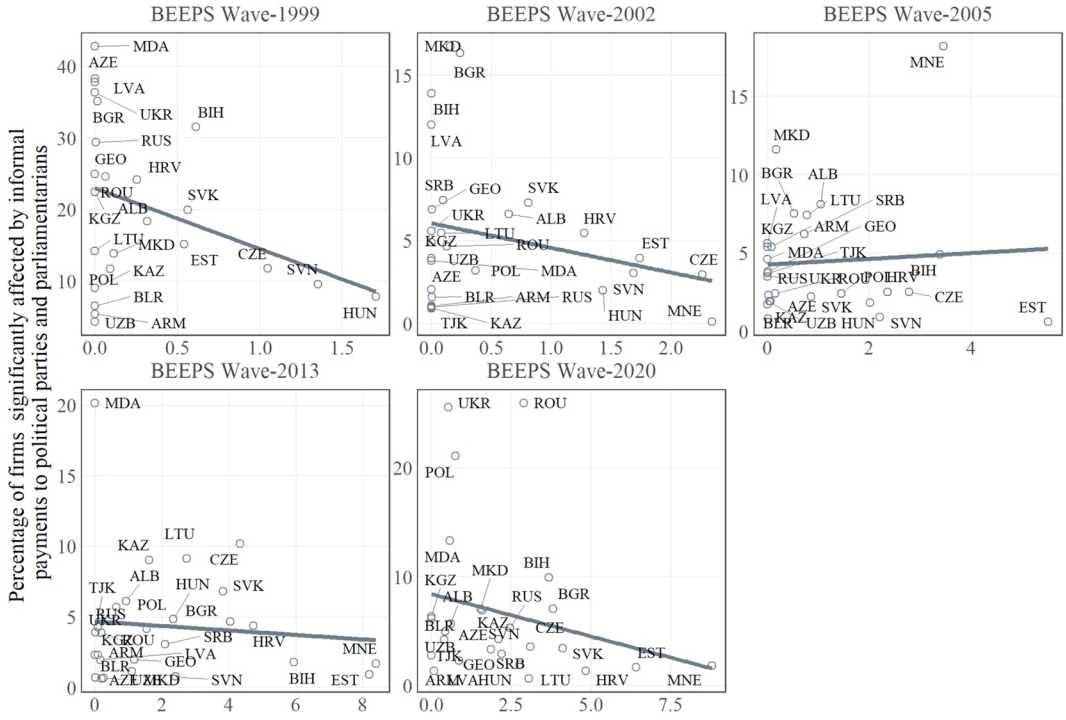

FIGURE 6 Relationship between state funding of parties and party corruption by Business Environment and Enterprise Performance Survey (BEEPS) wave. *Source:* Own elaboration based on data from Lipcean (2021) and BEEPS.

4 | EMPIRICAL RESULTS

Table 1 shows the results from two-way fixed-effects models. We present two models: Model 1 is a baseline model, while Model 2 exhibits the most conservative public funding estimate of permutations, including more independent variables. Models that build systematically from Model 1 to Model 2, as well as estimates including inequality and ethnic fractionalization features in the Appendix (Tables B1–B8). The first two models test Hypothesis One, which is a straightforward linear reduction in corruption as the amount of public funding rises. Models 3 and 4 are identical to Models 1 and 2, except that we use a discounted version of public funding. Therefore, models 3 and 4 test Hypothesis Two, which is that the reduction in corruption associated with increases in public funding diminishes at higher levels of public funding. All public funding coefficients are negative. The coefficients are significant at the five percent level in the baseline models. However, they are twelve to thirteen percent smaller in the full models and only significant at ten percent level. The coefficients for the discounted versions of the dependent variable are eighteen to twenty percent larger. Overall, the two-way fixed-effects approach shows tentative support for both Hypotheses One and Two.

Table 2 presents the same models in a within-between random-effects setup, which produces estimates of within-country and between-country effects. Again all public funding coefficients are negative. However, none of the within coefficients reach statistical significance, even at the generous ten percent level. By contrast, there are relatively strong between-effects. Given that the between component of WBRE models combine within- and cross-country impact on corruption, the coefficients are 1.6 to 3.7 times larger than their equivalents for within-effects. The baseline

TABLE 1 Public funding and party corruption: two-way fixed-effects models with alternative versions of public funding.

	Model 1	Model 2	Model 3	Model 4
DPF level-inflation adjusted	−0.139*	−0.121+		
	(0.067)	(0.071)		
DPF level-inflation adjusted & discounted			−0.164*	−0.145+
			(0.079)	(0.084)
Donation limits	−0.089	−0.048	−0.091	−0.048
	(0.095)	(0.084)	(0.095)	(0.085)
Democracy	−0.008	0.044	−0.006	0.046
	(0.079)	(0.090)	(0.079)	(0.090)
GDPpc (log)	−0.416	−0.472	−0.370	−0.431
	(0.491)	(0.576)	(0.492)	(0.577)
Electoral system		0.267		0.274
		(0.294)		(0.296)
Parliamentary		−0.569*		−0.573*
		(0.218)		(0.218)
Government size % GDP		0.019+		0.019+
		(0.011)		(0.011)
Num.Obs.	132	132	132	132
R2	0.592	0.617	0.591	0.617
R2 Adj.	0.448	0.467	0.448	0.467
FE: Country	X	X	X	X
FE: year	X	X	X	X

Note: Table entries represent unstandardized coefficients with robust standard errors clustered by country.
+ $p < 0.1$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

versions are significant at the five percent level, while in the full model, public funding is significant at 0.1%. The discounted dependent variable produces results that are nineteen to twenty two percent larger. Overall, we see a weak or non-existent within-effect and a much stronger and relatively certain between-effect.

We take three approaches to testing for a lagged impact of public funding on corruption. First, we use our existing data structure and lag by each period in the dataset. This approach treats that data as a panel and concentrates on what happens between each BEEPS round. Second, we change the data structure so that the independent variables are averaged across 3 years before each BEEPS round, 3 years being the shortest time between the surveys. This means that the coefficients refer to a standard length of time rather than the change of values from survey wave to wave. Third, we adopt a more typical time series approach and lag by a set number of years: we test for lags of between one and 5 years. This allows us to assess and compare the impact of different lags, although it takes less account of the irregular spaces between waves.

Table 3 presents the two-way fixed-effects results for our first two lagging strategies. We omit the baseline model and only show estimates for the full model, but again use our two dependent variables relating to Hypotheses One and Two. In Models 1 and 2, we lag by BEEPS wave. Both coefficients are approximately fifty percent larger than their equivalents in Table 1, and both are significant at the five percent level. This procedure accounts for lags but does not allow us to

TABLE 2 Public funding and party corruption: within-between random-effects models.

	Model 1	Model 2	Model 3	Model 4
DPF inflation-adjusted (within)	−0.083	−0.072		
	(0.090)	(0.088)		
DPF inflation-adjusted, discounted (within)			−0.117	−0.107
			(0.110)	(0.108)
Donation limits (within)	−0.218*	−0.160	−0.218*	−0.158
	(0.096)	(0.102)	(0.093)	(0.099)
Democracy (within)	−0.099	−0.047	−0.093	−0.040
	(0.090)	(0.122)	(0.090)	(0.122)
GDPpc (log) (within)	−0.472+	−0.598*	−0.451+	−0.571+
	(0.259)	(0.289)	(0.261)	(0.292)
Electoral system (within)		0.258		0.258
		(0.296)		(0.300)
Parliamentary (within)		−0.629+		−0.636+
		(0.353)		(0.351)
Government size % GDP (within)		0.026+		0.026
		(0.016)		(0.016)
DPF inflation-adjusted (between)	−0.154*	−0.265***		
	(0.069)	(0.047)		
DPF inflation-adjusted, discounted (between)			−0.188*	−0.316***
			(0.084)	(0.056)
Donation limits (between)	0.036	−0.034	0.033	−0.036
	(0.088)	(0.067)	(0.088)	(0.067)
Democracy (between)	0.095*	0.010	0.097*	0.013
	(0.040)	(0.036)	(0.040)	(0.036)
GDPpc (log) (between)	−0.095	−0.132	−0.095	−0.135
	(0.204)	(0.157)	(0.204)	(0.158)
Electoral system (between)		0.445**		0.438**
		(0.149)		(0.151)
Parliamentary (between)		0.225*		0.228*
		(0.097)		(0.095)
Government size % GDP (between)		0.004		0.004
		(0.011)		(0.011)
Obs.	132	132	132	132
AIC	365.5	358.2	364.9	357.5
BIC	394.3	404.3	393.7	403.6
Log.Link.	−172.75	−163.105	−172.433	−162.752
RMSE	0.90	0.83	0.89	0.83

+ $p < 0.1$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

TABLE 3 Public funding and corruption: two-way fixed-effects models, with alternative operationalization of public funding and lags.

	By BEEPS wave		Over 3 years	
Aggregation of independent variables	**Model 1**	**Model 2**	**Model 3**	**Model 4**
DPF level-inflation adjusted	−0.186*		−0.072	
	(0.072)		(0.085)	
DPF level-inflation adjusted & discounted		−0.215*		−0.073
		(0.085)		(0.105)
Donation limits	−0.165	−0.166	−0.039	−0.039
	(0.138)	(0.138)	(0.099)	(0.100)
Democracy	−0.302+	−0.303+	−0.035	−0.036
	(0.154)	(0.155)	(0.083)	(0.083)
GDPpc (log)	0.278	0.329	−0.539	−0.497
	(0.728)	(0.728)	(0.591)	(0.574)
Electoral system	−0.168	−0.157	0.367	0.374
	(0.300)	(0.302)	(0.247)	(0.247)
Parliamentary	0.317	0.322	−0.553*	−0.557*
	(0.454)	(0.457)	(0.261)	(0.260)
Government size % GDP	−0.021*	−0.021*	0.022+	0.022+
	(0.010)	(0.010)	(0.011)	(0.011)
Num.Obs.	105	105	132	132
R2	0.488	0.487	0.618	0.617
R2 Adj.	0.216	0.215	0.467	0.466
FE: Country	X	X	X	X
FE: year	X	X	X	X

Note: Table entries represent unstandardized coefficients with robust standard errors clustered by country.
+ $p < 0.1$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

calculate their impact due to the uneven length of time between waves. Therefore, in models 3 and 4, we show the same models, this time with independent variables averaged over a consistent 3 years before the corruption observation. The coefficients are only thirty-four to thirty-eight percent of the size of their equivalents for the lagged-wave setup.

Table 4 shows within-between random effects tests of the two lagging structures. Lagging by BEEPS waves produces a statistically significant within effect (at five percent), as well as between effects of approximately twice the size (significant at one percent). The results for the three-year average also look different. Again, there are negligible coefficients for within-effects but relatively large coefficients for between-effects: the estimates are forty percent larger than the equivalents when lagging by BEEPS waves and are significant at 0.1%.

Finally, Table 5 shows models with lags from a single year referenced to the year of the dependent variable. Unfortunately, our data structure could not accommodate the inclusion of more than one lag in a single estimation. Table 5 exhibits little or no effect for t-1 to t-3. However, the four-year lag exhibits a much larger coefficient and a much tighter standard error. While the coefficient is comparable to the larger equivalents in previous tables, the standard error is much smaller, and statistical significance reaches 0.1%. The t-5 result is also much larger than t-1 to t-3

TABLE 4 Public funding and corruption: within-between random-effects models, with alternative operationalization of public funding and lags.

	By BEEPS wave		Over three years	
Aggregation of independent variables	**Model 1**	**Model 2**	**Model 3**	**Model 4**
DPF level-inflation adjusted (within)	−0.114*		−0.002	
	(0.053)		(0.088)	
DPF level-inflation adjusted & discounted (within)		−0.139*		−0.004
		(0.064)		(0.111)
Donation limits (within)	−0.189+	−0.190+	−0.167	−0.166
	(0.101)	(0.101)	(0.105)	(0.105)
Democracy (within)	−0.311*	−0.309*	−0.092	−0.092
	(0.156)	(0.156)	(0.103)	(0.103)
GDPpc (log) (within)	0.980**	0.990**	−0.620*	−0.617*
	(0.367)	(0.367)	(0.271)	(0.274)
Electoral system (within)	−0.008	−0.007	0.388	0.388
	(0.280)	(0.280)	(0.242)	(0.242)
Parliamentary (within)	0.249	0.250	−0.860**	−0.860**
	(0.407)	(0.408)	(0.330)	(0.330)
Government size % GDP (within)	−0.018+	−0.018+	0.029+	0.029+
	(0.010)	(0.010)	(0.016)	(0.016)
DPF level-inflation adjusted (between)	−0.230**		−0.323***	
	(0.086)		(0.056)	
DPF level-inflation adjusted & discounted (between)		−0.266**		−0.374***
		(0.095)		(0.071)
Donation limits (between)	−0.026	−0.027	−0.072	−0.069
	(0.070)	(0.070)	(0.069)	(0.070)
Democracy (between)	−0.003	−0.002	0.010	0.011
	(0.045)	(0.045)	(0.029)	(0.030)
GDPpc (log) (between)	−0.319**	−0.319**	−0.098	−0.104
	(0.114)	(0.114)	(0.162)	(0.166)
Electoral system (between)	0.490***	0.487***	0.428**	0.431**
	(0.135)	(0.136)	(0.151)	(0.153)
Parliamentary (between)	0.207+	0.209+	0.271**	0.263**
	(0.117)	(0.117)	(0.102)	(0.102)
Government size % GDP (between)	0.011	0.011	0.002	0.002
	(0.010)	(0.010)	(0.011)	(0.011)
Num.Obs.	105	105	132	132
AIC	254.4	253.8	357.5	357.8
BIC	296.8	296.3	403.6	403.9
Log.Lik.	−111.181	−110.925	−162.759	−162.889
RMSE	0.70	0.70	0.83	0.83

+ $p < 0.1$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

TABLE 5 Public funding and corruption: two-way fixed-effects models, with one to 5 years lags.

	Model 1:	Model 2:	Model 3:	Model 4:	Model 5:
	1-year lag	2-years lag	3-years lag	4-year lag	5-years lag
DPF level	−0.024	−0.034	−0.006	−0.146***	−0.095+
	(0.090)	(0.096)	(0.091)	(0.034)	(0.048)
Donation limits	0.036	−0.042	−0.054	0.011	−0.074
	(0.052)	(0.062)	(0.068)	(0.071)	(0.094)
Democracy	−0.007	−0.074	−0.065	−0.069	−0.105+
	(0.076)	(0.070)	(0.058)	(0.059)	(0.053)
GDPpc (log)	−0.412	−0.513	−0.450	−0.708	−0.220
	(0.598)	(0.588)	(0.530)	(0.492)	(0.368)
Electoral system	0.351	0.257	0.137	0.041	0.067
	(0.285)	(0.269)	(0.234)	(0.226)	(0.208)
Parliamentary	−0.481*	−0.280	−0.274+	−0.101	0.090
	(0.177)	(0.181)	(0.149)	(0.158)	(0.217)
Government size % GDP	0.017+	0.020+	0.014+	0.009	0.0009
	(0.010)	(0.010)	(0.008)	(0.006)	(0.005)
Num.Obs.	132	132	132	131	131
R2	0.609	0.610	0.600	0.599	0.591
R2 Adj.	0.455	0.457	0.443	0.439	0.428
FE: Country	X	X	X	X	X
FE: year	X	X	X	X	X

Note: Table entries represent unstandardized coefficients with robust standard errors clustered by country.
+ $p < 0.1$, *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

but is only 65% the size of t-4 and makes only the ten percent threshold for statistical significance. A four to five-year lag is theoretically appealing because it suggests that changes in the amount of public funding take approximately one parliamentary and/or presidential term to feed through into corruption.

In these lagged models, the discounted version of the dependent variable continues to produce stronger results, thereby providing more support for Hypothesis Two. Overall, the models strengthen when lagging is introduced, which suggests support for Hypothesis Three on the inelasticity of corruption's response to increases in public funding. Nevertheless, our data structure makes it hard to quantify the inelasticity, and its extent is uncertain.

So far, we have concentrated on the direction, relative size, and uncertainty of estimates. In Figure 7, we plot predicted corruption values to gauge the effect size and how uncertainty varies across different values of public funding. The top panel shows the results from the transformed dependent variable, while the bottom one depicts the same results after converting back to original scale the predicted values and confidence intervals using the same Yeo-Johnson algorithm to normalize our dependent variable initially.

Hence, it reflects the predicted effect of public funding on corruption in the original metric, which makes it more intuitive to interpret.[4] Panels A and D are derived from the two-way fixed effects estimates in the full model for the discounted version of public funding (model 4) from Table 1. The effect is not distinguishable from zero at the ninety percent confidence level until four

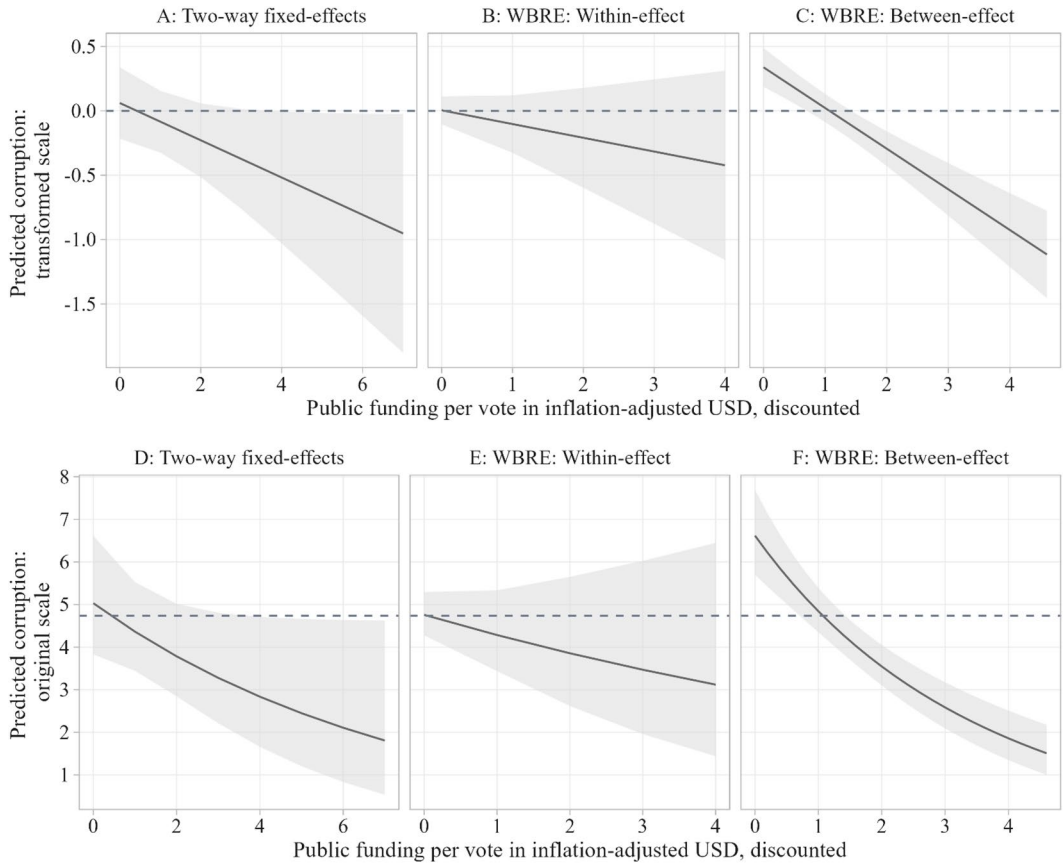

FIGURE 7 Predicted corruption level conditional on the amount of public funding. Confidence intervals reflect the 0.1 percent level

dollars per vote. This is a rather generous amount of public funding, but not beyond the means of several countries in our sample. This relatively high number may explain why within-effects were weak: not enough countries increased their public funding from below four to above four dollars per vote over time. Panels B and E and C and F are derived from the within-between random-effects estimates in the full model (model 4, Table 2) for the discounted version of the dependent variable. The within-effect is too weak and, once again, suggests that it is due to insufficient within-country increase in public funding across time. The between effect, by contrast, is stronger and has a narrow confidence interval. This should not be surprising given the pooling of within and between-country variation in public funding in assessing its effect on corruption. Here the estimate reaches ninety percent confidence at only 1.5 dollars, a low amount that the vast majority of our countries had exceeded by 2020. However, this effect is largely a contrast between countries providing generous and parsimonious subsidies to parties (net of other variables in our equations) rather than a result of within-country changes in public funding. While the between-effect is policy-relevant, the within-effect would have been much more encouraging for those hoping to employ public funding as an anti-corruption tool. Nevertheless, even if one focuses on much more conservative predictions of the two-way fixed-effects model (Figure 7, d panel), the increase in public funding (once it becomes statistically significant) is associated with about a one percent decrease in firms significantly affected by illegal payment to political parties

and legislators. Since the average percentage of firms affected by such payments in our sample is eight percent, this implies a decline in corruption by about 12%, which is not negligible, substantively speaking.

Our appendices report further analyses. First, we entered different versions of public funding and other control variables (Tables B1–B8). Second, we discarded authoritarian regimes - Central Asia republics, Russia, Azerbaijan, and Belarus (Tables B10–B12). Overall, our results are robust to alternative specifications or even more robust. Third, we showed that election dummies or counts of the frequency of elections do not change the results (Table B9). Fourth, we show that alternative versions of corruption operationalization make no substantive difference (Tables B17 and B18).

Finally, we carry out three types of Jackknife resampling: country-, observation-, and BEEPS round-based variance estimations. For each resampling benchmark, we remove one unit at a time, re-estimate the parameters for public funding and compare them against the main results. Figures C1–C2 show the variation in estimates for the country- and observation-based fixed-effects models; Figure C3 depict the estimates for the within-between random-effects model, while Figure C4 presents them for all BEEPS round-based model specifications. Overall, the results from the Jackknife estimations are consistent with our main results.

5 | CONCLUSIONS

We will first summarize the direct implications of this research and then reflect on how this work contributes to the wider academic and policy debate on public funding and corruption. Consistent with recent theory and practice (Imbens, 2021), we use significance tests as a guide to uncertainty, not an indicator of the existence or non-existence of a phenomenon. We believe that the effect must exist, at least in a perfect laboratory experiment. Instead, we wish to estimate the strength and uncertainty of policy intervention. We report a consistent negative association between public funding and corruption. However, the statistical significance of this association is not robust to all specifications: significance levels range from 0.001 to insignificant. Moreover, our coefficients imply that the small amounts of money dedicated to public funding by most governments in our dataset have tiny effects on corruption. From this perspective, then, public funding is unlikely to be a noticeably effective anti-corruption tool. Nevertheless, the coefficients show that the most generous public financing is associated with more substantial reductions in corruption. Of course, any predictions at the extremes of our distribution are even more uncertain than those made closer to its center. High levels of public funding are hardly noticeable in national budgets, and corruption is an enormously damaging phenomenon. From another perspective, then, our research suggests that large increases in public funding may be worth trying as a way to reduce corruption, although it might have no effect and any effect it does produce may take a long time to realize the hoped-for improvement.

Interpreting a contribution to the literature is a Bayesian exercise. In the social sciences, research works are usually far from directly comparable, which makes it harder to decide how to revise priors. The only other longitudinal study addressing this question is Hummel et al. (2021), who argue that public funding has a strong anti-corruption effect. We believe that prior beliefs in the efficacy of public funding should be revised downwards in effect and upwards in uncertainty when our research is compared to theirs. The stronger effect in Hummel et al. (2021) probably derives from two aspects of their study: their impressively long time series and their very general independent variable. They do not test the impact of actual amounts of public funding.

Instead, their index probably probes something more akin to political finance reforms. Political finance regulation and corruption are related systems. The systemness of corruption suggests that gradual or particular reforms are unlikely to be effective; breaking down a power structure requires a comprehensive package (Weschle, 2022, p. 200) and maybe a broader political movement (Fisman & Golden, 2017, pp. 215, 244–245). A systemic perspective requires parsing of its constituent mechanisms, their interactions, and effects (including, as emphasized by Weschle (2022), higher-order effects). This requires a targeted dependent variable and a precise independent variable. It also necessitates approaches to the self-regulating nature of politics, the inelasticity of corruption, the curvilinearity of political finance regulation, all of which we take on, to some extent at least. Finally, there is one challenge we have not managed to confront: estimating the extent to money in politics funds political competition or personal enrichment. This will be very difficult, but more objective corruption measures and forensic accountancy (Fazekas & Cingolani, 2017; Golden & Picci, 2005; Weschle, 2022) are two promising methods. The theoretical challenge is even more daunting: how does public funding interact with other political finance regulations, such as donations, spending limits, and enforcement?

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CONFLICT OF INTEREST STATEMENT

The authors declare no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

DATA AVAILABILITY STATEMENT

The data that supports the findings of this study are available in the supplementary material of this article.

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ENDNOTES

[1] The 2009 BEEPS round did not include the question of our interest present in other BEEPS editions. Additionally, Montenegro, Serbia and Tajikistan were not included in the 1999 BEEPS round, which leaves us with 132 country-year (BEEPS) observations.

[2] The text of the question is: "It is often said that firms make unofficial payments/gifts, private payments or other benefits to public officials to gain advantages in the drafting of laws, decrees, regulations, and other binding

government decisions. To what extent have the following practices had a direct impact on your business: No impact, minor impact, moderate impact, major impact, decisive impact."

[3] We also use an alternative operationalization of high-level corruption by calculating the proportion of respondents mentioning decisive, major, or moderate impact of informal payments on their firm's performance, but the results are not substantially different from our main operationalization. Please see Figure N in supplementary materials.

[4] The *Y*-intercept from the bottom panel represents the equivalent of the *Y*-intercept in the top panel and was obtained using the same reversion algorithm as for predicted values and confidence intervals.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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